Everybody talks about it...

By A.WIIN NIELSEN

Matematik-fysiske Meddelelser **44**:4

Det Kongelige Danske Videnskaberne Selskab
The Royal Danish Academy og Sciences and Letters

Commissioner: Munksgaard · Copenhagen 1997

Abstract

Weather predictions have, during the last half century, become more accurate and can be produced for a longer time. The reasons are better and more plentiful observations combined with a total change of the scientific foundation on which weather forecasts are made. The change from an empirical, statistical foundation to a firm foundation on the basic laws of classical physics is described.

The first formulation of the basic laws for the atmosphere was given in the first decade of the present century. During the next decade a heroic attempt was made to produce a forecast based on a very general approach to the problem. It was premature and resulted in totally unrealistic results. With better observations and with the design of the first computers by the middle of the century new attempts were made based on a new philosophy of using simpler models of the atmosphere. These models could produce useable forecasts for 1 to $1^1/_2$ days.

The further development of more realistic atmospheric models and, having learned how to handle the basic form of the equations, a gradual return to a very general methodology resulted in both better short-range forecasts and the extension of useable weather forecasts to about one week. These developments are described in non-mathematical terms. The concept of limited predictability, setting an upper time for the validity of weather forecasts, is discussed. A comparison is finally made between weather forecasts and climate simulations with some remarks about the possibility of making climate change predictions.

Dr. A. C. WIIN NIELSEN
Solbakken 6
DK-3230 Græsted
Denmark

Printed in Denmark by Special-Trykkeriet Viborg a-s
ISSN 0023-3323 ISBN 87-7304-288-9

Table of content

Foreword

The title of the present book is taken from the general statement that 'Everybody talks about the weather, but nobody does anything about it'. The origin of the statement is often ascribed to Mark Twain in the U.S.A. and to various national humorists in other countries. In Denmark we refer to Robert Storm Petersen, a cartoonist of national fame with a whimsical mind, as the originator. It is apparently used for the first time by Charles Dudley Warner who wrote it in an Editorial in the Hartford, Connecticut, The Courant, in 1890.

Atmospheric scientists cannot do anything about the weather. The days of optimism when one believed that it was possible to modify the weather by dropping various chemicals in the clouds to produce precipitaion are long gone. One of the tasks of the meteorologists is to predict the weather as far into the future as possible.

The present book aims to describe the development of weather forecasting in the latter half of the present century in a mostly untechnical way. During these decades weather forecasting has experienced a revolutionary development where the process of preparing weather forecasts has changed completely from the art of preparing a forecast for the next 24 hours using empirical or statistical methods of questionable validity to objective methods based on the classical physics applied to the atmosphere.

During the same period we have also learned about limited predictablity of non-linear systems in general and atmospheric systems in particular. The impact of limited predictability is severe not only on weather forecasts, but also on the efforts to determine climate changes possibly created by anthropogenic effects.

1. Introduction and purpose

The following chapters will contain a description of the development of the field of weather prediction in the second half of the present century. Preparations of the forecasts are normally called numerical weather prediction (NWP), and they constitute a complete break with the way in which forecasts were made in earlier times.

It is neither my purpose to write the history of numerical weather prediction nor of meteorology in the second half of the 20'th century, because the main emphasis will be on only the main events that created new developments in the field of NWP, and because other fields of meteorology will enter the discussions only if they have an impact on the prediction models. The description will therefore not satisfy the point of view of the historian, for whom all details should be included, but it will emphasize the ideas and actions that gradually created the revolution in weather forecasting.

To understand these developments it has been found useful to give a brief description of the development of meteorology from ancient times to the middle of the present century. The details of the developments have already been covered in an excellent way by authors writing the history of meteorology (see, for example, Frisinger, 1977 and Kutzbach, 1979).

Although numerical weather prediction rests on the time integration of a set of equations which in turn are formulated on principles of classical physics as applicable to the atmosphere, no equations will be presented in the following chapters. It will rather be attempted to describe the physical and mathematical principles involved and to show their relevance to the prediction problem.

Weather forecasts are never totally accurate. Since the beginning of numerical weather prediction the validity of the forecasts have increased from half a day or a day to about a week, which is the present upper limit for forecasts that can be used with advantage by the operational meteorologists. We know that the atmosphere from a theoretical point of view can be predicted only for a limited time period, which has been estimated to be about 3-4 weeks. The possibilties of reaching closer to the theoretical limit will be discussed.

Industry and agriculture, as well as other sectors of society, could use good monthly and seasonal forecasts advantageously for planning purposes. They cannot be produced at the moment by simply extending the integrationts to larger times due to the limited predictability. Similarly, while we may be able to simulate the present climate on the large scale, it does not seem to be possible to predict climate changes regardless of all the attempts that have been made in the last couple of decades. Also these questions will be discussed.

The author has had the great benefit of being present and participating in many, but far from all, of the events that gradually improved numerical forecasts. The progress has not been made by single individuals working alone, but rather by teams of researchers put together for the purpose of giving some new ideas the necessary operational tests. Needless to say, new ideas have not always been found to be good ideas. What we have today is based on the ideas that have survived the tests. In the following we shall describe actual progress, present some of the problems that have been solved, but, in general, abstain from a presentation of the procedures that did not survive the tests. It is also true that what did survive did not necessarily live for a long time. However, it served a good purpose for the duration.

The chapters will contain the names of some of the major players in the developments. It is, needless to say, impossible to mention all the players, but it is recognized that numerical weather prediction would not be where it is today without the hard work and the long hours of all the team members who contributed to the testing of the possible improvements, and all the operational meteorologists who through the years have provided useful feedback to the researchers of their experiences in using the forecasts for operational purposes. Occasionally, the latter group has expressed wishes that, however justified, could not be fulfilled due to the present limited ability characteristic of the whole subject of predictions.

Weather forecasting shares the limited predictability with almost all others physical fields dominated by nonlinear processes. All of these fields rely to a very large extent on numerical, rather than analytical, procedures. It has been the rule rather than the exception that new ideas have been tested on simplified systems containing rather few degrees of freedom. The following chapters will also discuss the results of the so-called low order models because it is possible to illustrate the behavior in much simpler ways and thereby obtain a good understanding of the major processes. However, the atmosphere is a system with very many degrees of freedom, and it can be described only by high order systems. While low order systems therefore are very useful to test ideas in a preliminary way, they will never give the final answer although they have a great role to play for educational purposes. Once again, the tests have to be conducted with the real prediction models usually running not one, but a large sample of forecasts.

It is the hope of the author that the present text will help to increase the understanding of the ways in which numerical forecasts have been and are being produced, and that the readers will realize the real impact of the limited predictability. In addition, the description of the developments in this highly specialized field over the last five decades is a good example of the gradual improvements that may be obtained in a field of applied research where the outcome is of immediate use to the public. As the reader will realize, it is also an example of the application of results of basic research, created for the purposes of understanding the general processes in the atmosphere and not necessarily for the purposes of producing forecasts.

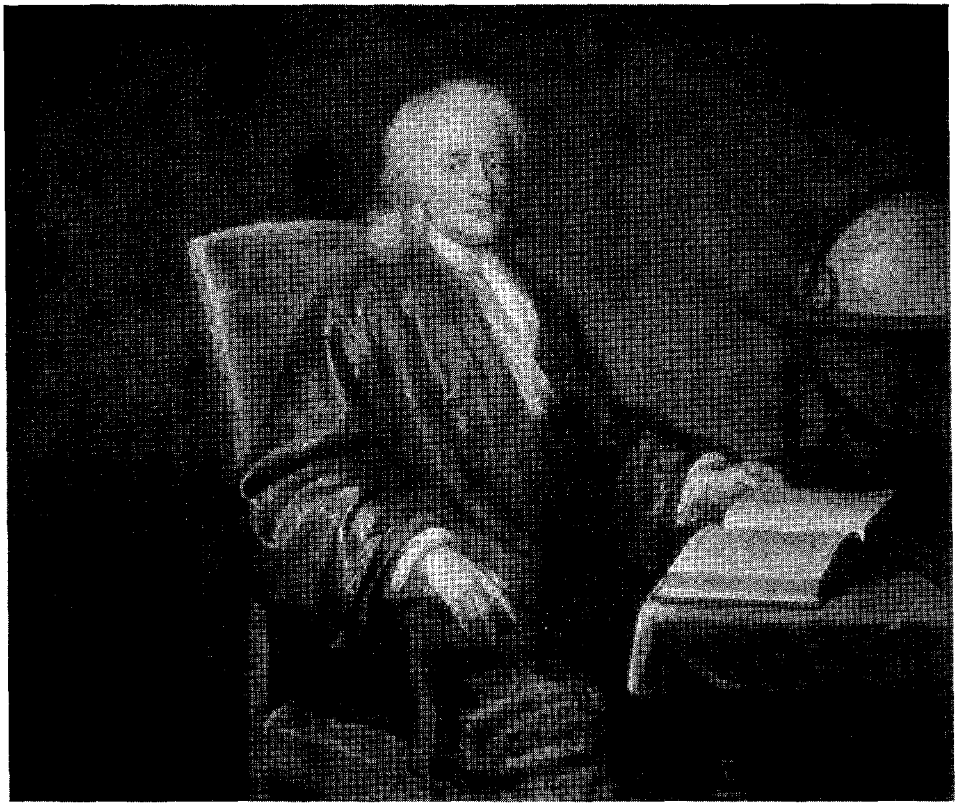
2. The beginning of meteorology

As long as humanity has existed there must have been an interest in the weather. The ideas of the atmosphere and the processes going on in it were vague and imprecise in antiquity and mostly relying on the descriptions provided by Aristotle's »Meteorologica«. The period from c. 600 B.C. to c. 1600 A.D. has been called »The Period of Speculation«, while the period c. 1600 A.D. to c. 1800 A.D. has been labeled »The Dawn of Scientific Meteorology« by H.H. Frisinger. The reason for this division is to a large extent dominated by the fact that reasonably good measurements of the properties of the atmosphere could not be made before the invention of the thermometer by Galileo somewhere between 1590 and 1600 and the construction of the first barometer by Torricelli in 1643-44. These instruments together with wind measuring devices (anemometers), of which many versions exist, permitted a real study of the physical characteristics of the atmosphere. It did not take long before it was realized that both the pressure and the temperature normally decrease with height. In addition, one established systematic meteorological measurements often carried out by already existing astronomical observatories. The first network of meteorological stations was apparently established by Societas Palatina in Germany, but as has been the case later, a war was necessary to create a new phase in meteorology.

During the Crimean War of 1852-54 major parts of the British and French fleets were destroyed by a violent storm in the Black Sea. The storm had moved across Europe from north-west to south-east and could be followed using the sparse network of observations in operation at the time. Not surprisingly, it was argued that if a dense network of meteorological observations had been in place at the time, the storm and its path could perhaps have been predicted. In the years to follow new meteorological stations were established in many of the European countries. During the next two decades the national meteorological institutes were created in the countries which had not already done so (The Netherlands 1854, Great Britain 1854, France 1855, Sweden 1859, Italy 1863, Norway 1866, United States,1870, Denmark 1872, India 1875, Switzerland 1880). Similar institutions were already in existence in Belgium (1826), Germany (1847), Russia (1849) and Austria (1851).

A greatly increased network of surface observations was created, surface

Vilhelm Bjerknes (1862-1951)



Sir Isaac Newton (1643-1727)

weather maps were being prepared, and the field of synoptic meteorology was being advanced. The first relationship between the pressure field and the wind field at the surface of the Earth was established and has become known as »Buys-Ballot's Law«. For the Northern Hemisphere it says that if one stands with one's back to the wind, the pressure will be low to the left and high to the right. Today it is known as the geostrophic relationship, which says that there is a tendency for a balance between the pressure force and the Coriolis force. If the balance is exact, the wind will blow along the lines of constant pressure, called the isobars, with the low pressure to the left and the high pressure to the right. In the Southern Hemisphere the geostrophic wind will blow with the low pressure to the right.

The geostrophic rule is not exact anywhere in the atmosphere because frictional forces will always be present. However, above the atmospheric boundary layer, dominated by the friction between the atmosphere and the surface of the Earth, it is almost correct in straight flow. In the boundary layer, covering about the lowest kilometer of the atmosphere, the influence of the surface friction is larger, and the actual wind blows across the isobars from high to low pressure with an angle between the wind direction and the direction of the isobars of 10 to 30 degrees. In some cases of weak winds, the angle may be even larger.

The discussion of the geostrophic relationship has been carried out in some detail because the »almost-geostrophic« relation has played an important role in the models designed in the early part of the development of NWP.

Observations from the upper atmosphere were not part of the daily routine in the early networks of meteorological stations. Upper air instruments for daily routine measurements were developed well into the 20'th century. However, one did obtain new information from the upper atmosphere collected from manned balloon flights, carried out many places in the world, but with the greatest results in France.

Any national meteorological service need to have access to data from other regions in the older days and from the total atmosphere today. Such data exchanges are arranged by international agreements. It is thus charateristic for the meteorological field that a number of directors of the national institutions as early as in 1874 formed the International Meteorological Organization. It is this institution that we know today as the World Meteorological Organization (WMO). The latter was created after World War II and became a special organization under the United Nations in 1951. The former international organization was independent and could be considered as an informal 'club' of the directors of the national meteorological institutes. WMO is today an organization dealing with all international aspects of meteorology and operational hydrology.

On the theoretical side, the basic equations applicable to the physical processes and the motion of the atmosphere were known. The problem of writing these equations in such a form that the motion could be measured relative to the rotating Earth had been solved. The Coriolis and centrifugal accelerations had been understood. Scientific journals had been established, some by the newly-formed meteorological societies and others by the national meteorological institutes. Considerable research was carried out on many meteorological questions. A major topic was the attempts to understand the formation of the middle latitude cyclones with contributions by quite a number of meteorologists (Kutzbach, 1979). It appears, however, that no serious attempts were made in the nineteenth century to use this knowledge for the purposes of predictions on a physical-mathematical basis. Weather predictions were made for periods up to one day based on surface pressure analyses and using empirical rules or collected experiences. It would therefore be correct to say that as far as weather prediction is concerned nineteeenth century meteorology could be called descriptive and empirical. This does not mean that the dynamical laws of the atmosphere were not used. They were indeed applied to increase the understanding of atmospheric structures such as various forms of clouds and the low and the high pressure systems. The existence of low pressure systems were thought to be due to heating of the air from the surface below, or, in other words, a process similar to the formation of convective clouds, but on a much larger scale. Due to the size of the systems it was necessary to introduce the influence of the Coriolis acceleration in order to explain the rotation of the air in these structures. To deal with these matters it had been necessary to investigate the temperature changes under adiabatic and non-adiabatic conditions. The role of moisture in these processes had been investigated in detail. On the dynamical side the concept of the thermal wind relating the vertical wind shear of the horizontal wind to the horizontal temperature gradient was developed in the nineteenth century.

Nothing seems to have happened on the prediction problem before Vilhelm Bjerknes, in a short paper in 1904, points out that the prediction problem from a formal point of view is a well-posed problem. He points out that the atmosphere is characterized by seven scalar quantities: the three components of the wind, the pressure, the temperature, the density and the humidity, and that seven coupled differential equations connecting these seven scalar quantities are known. They are the three equations of motion relative to the Earth (one equation for each component), the thermodynamic equation, the continuity equation, the equation of state, also called the gas equation, and the equation for the rate of change of the moisture. All of these equations, with the exception of the gas equation, are first order partial differential equations with respect to time. Since these equations are expressed in a system relative to fixed points on the Earth, they contain terms, the so-called advection terms, describing the fact that the local value of any parameter is influenced by the motion of the atmosphere. The local change of such a parameter is determined by the product of the wind and the gradient of the parameter, where the last quantity means the spatial change of the parameter per unit distance. One should note that these terms are the product of two parameters or their derivatives. This fact means that the rate of change of any of the parameters depends on so-called nonlinear terms, defined as the above products. It means also that the system of equations from a mathematical point of view falls in a category of partial differential equations, where solutions normally can be obtained by numerical procedures only. For systems of linear equations we are always in a position to determine if they have solutions, and if so we can always obtain them by analytical procedures. For nonlinear equations it is the exception rather than the rule that solutions may be obtained in a closed form, and the meteorological equations do not fall in this class.

Bjerknes realized that no mathematical solutions were available for the system of equations that he formulated. Considering the time of the publication it is understandable that he did not think of solving them by numerical procedures, since no computing machinery of sufficient capacity and speed was available to him. He mentions that it would perhaps be possible to develop graphical procedures and in this way obtain approximate solutions. It is, however, a fact that he did not return to the problem of the graphical procedures later in his research.

As we shall see later, his idea of using graphical procedures to obtain solutions was used 50 years later by his countryman, Ragnar Fjørtoft, but he used a much simpler model having only one partial nonlinear equation.

The general impact of the article by Bjerknes was small. The reasons are obvious. The meteorologists realized that although the theoretical foundation for formulating the seven nonlinear equations were in order, the practical way of solving the coupled equations was not known. In addition, the formulation required observations from the total atmosphere, and they were not available. However, the paper by Bjerknes found at least one very interested reader in the English physicist Lewis Fry Richardson. He got the idea that the system of equations could be solved by numerical procedures. The reason for this is probably that Richardson already had some experience in such procedures having solved some diffusion problems for which he developed numerical procedures. Richardson's attempt to attack the numerical prediction problem will be described in the next chapter.

3. A General attempt: L. F. Richardson

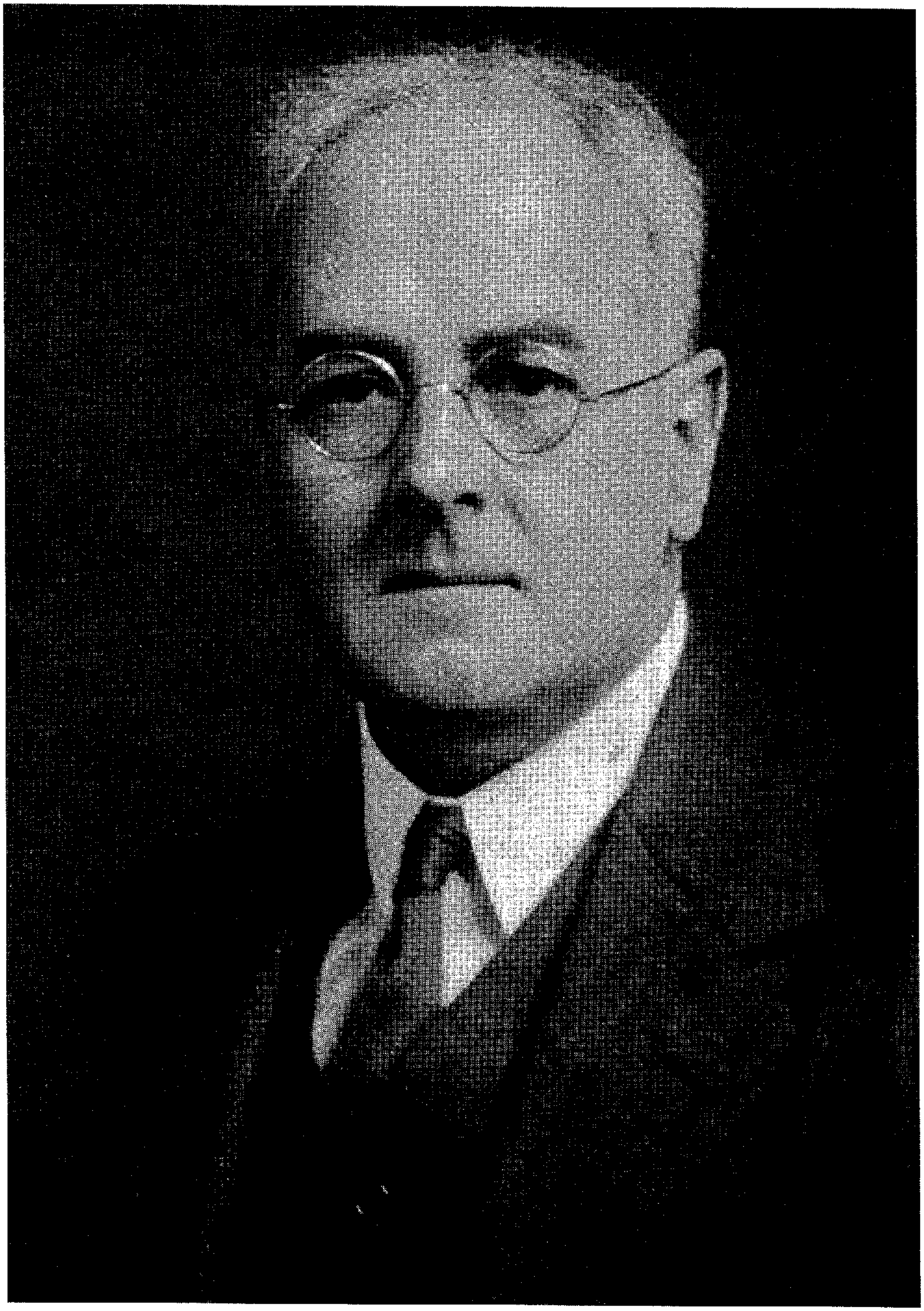
The attempt to produce a weather forecast by numerical procedures carried out by L.F. Richardson is well documented since he published a book inwhich the experiment is described in great detail (Richardson, 1922). The original edition was printed in a relatively small number of copies, since the publication had to be supported by a grant from the Royal Society (£ 100) and a smaller grant (£ 50) from the Meteorological Office. Today the original edition belongs to the category of rare books. It has, however, been reissued in a paperback edition in 1965 with a foreword by Sydney Chapman. George P. Platzman was asked to review the new edition, but the review turned into a long article, in which he gives a retrospective view of the efforts of Richardson (Platzman, 1967). While we are discussing the person, L.F. Richardson, it should also be mentioned that his very interesting life is described in a biography written by his distant relative Oliver M. Ashford (1985).

Richardson's plan was to make a real weather forecast. Such a forecast required in his formulation a three-dimensional grid consisting of a two-dimensional grid in each horizontal surface and a number of these surfaces to cover the vertical distribution of the atmospheric parameters. Due to the limited availability of data he defined a horizontal grid at the surface of the Earth covering the European region. A weather forecast has to start from an initial field, and the first very serious problem was that only surface observations were available at the time, while data for the upper air was also required by the model. It was thus necessary for him to construct artificial upper air data as required. It is easy to understand that there was considerable uncertainty about the real state of the upper levels of the atmosphere in his model and the uncertainties will be carried into his experiment.

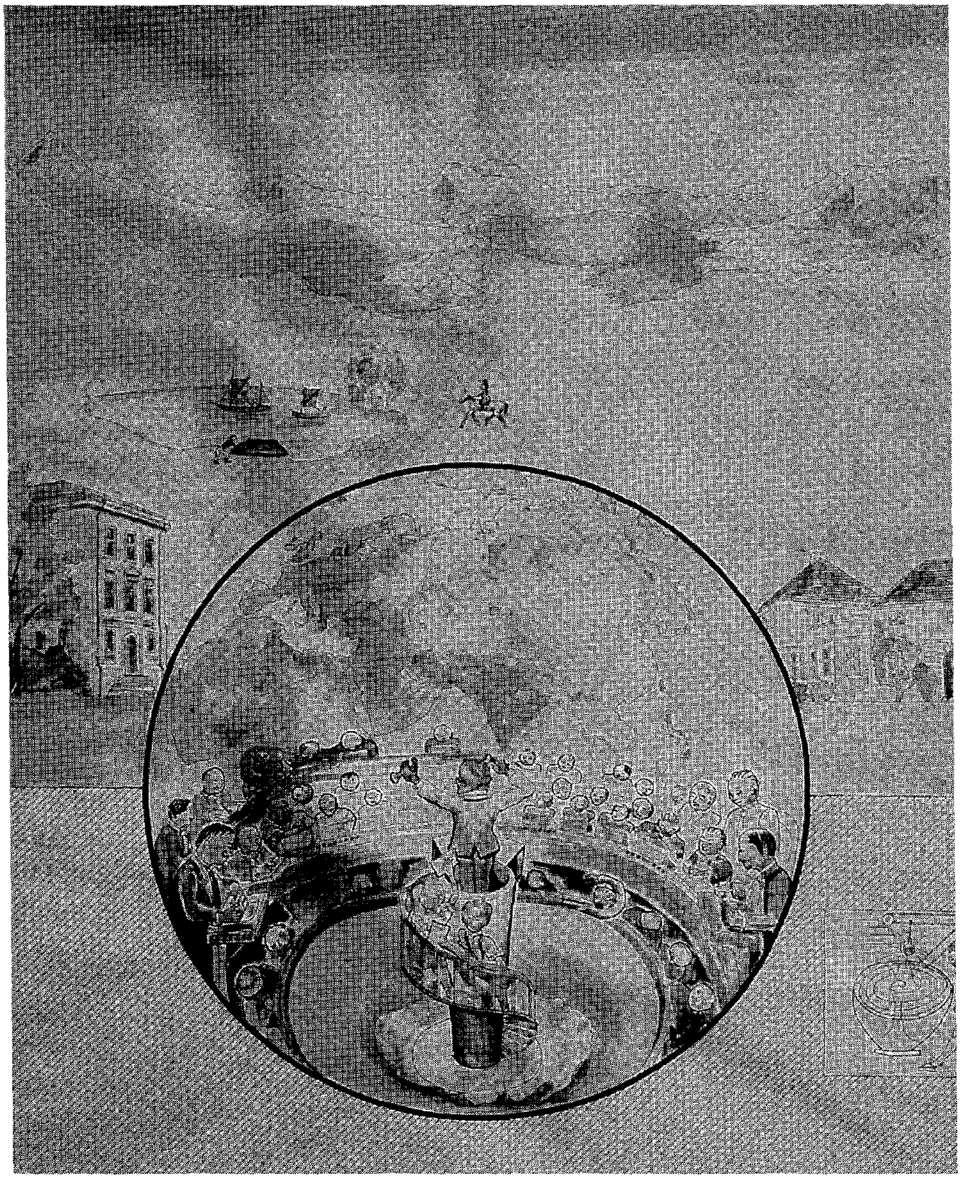
The strategy was then to arrange the equations in such a way that all the time derivatives were on the left side, while all the terms determining the rate of change with respect to time were on the right side of the equations. All the terms on the right-hand sides can then be computed from the initial state and added together. The seven sums should then (in principle) be multiplied by a suitably small time step and added to the original values in each gridpoint. In this way it should be possible to produce a forecast by repeating the procedure described above as many times as required to make a forecast of the desired length in time. However, Richardson never made more than one time step because calculating the right-hand sides of the equations was very cumbersome and time consuming, and especially because the predicted changes in the variables were much too large and too erratic from one point in the grid to the next.

The book by Richardson describes in detail how he computed the various terms, and how he formulated the physical terms entering his equations. He went into great detail to formulate the various processes determining the driving mechanism for his forecast, i.e. the heating of the atmosphere. In a similar way he formulated the dissipation of the kinetic energy in the model. As a somewhat curious example it may be mentioned that Richardson is very interested in the heat budget for soil covered with vegetation. For this purpose he considers in detail the heat budget of a single leaf depending on temperature, the humidity in the air and the amount of water in the leaf itself.

One gets the impression that Richardson was rather unwilling to make approximations. Whenever possible, he incorporates the processes in a way that is as general as possible. He was, however, forced to make an approximation to define the state of the atmosphere at the upper level, and he kept the ap-



Lewis Fry Richardson (1881-1953)



Richardson's dream.

proximation in the model. The approximation is called the hydrostatic approximation, and it may be described in two ways. One way is to say that the third equation of motion is reduced to a balance between the gravity and the vertical component of the pressure force. The other way to express the same is to say that it was assumed that the pressure on a horizontal surface of, say, 1 m^2 is equal to the weight of the atmospheric column from that surface to the top of the atmosphere. The approximation is actually the best justified approximation in large scale meteorology because it is possible to show by estimating the magnitude of all the other terms in the third equation of motion that these terms are several thousands times smaller than the two terms remaining in the equation, i.e. the vertical component of the pressure force and gravity.

Having made the approximation Richardson realized that it put him in an entirely new situation because by making the approximation he had lost the equation from which he was going to predict the new values of the vertical velocity. Did the approximation mean that no vertical displacement could be present in his model atmosphere? If so, the approximation was after all too serious because without the vertical velocities there would be no clouds and no precipitation. His analysis showed, however, that vertical velocities can exist in a hydrostatic atmosphere. They are not the real vertical velocities, but the vertical velocities that are consistent with the maintenance of hydrostatic equilibrium at all times and in all points. As a matter of fact he used this principle to formulate an equation from which the vertical velocities could be computed, and this new equation was then incorporated in the model as the new equation from which the vertical velocities were to be determined in each time step. He became aware of the desired equation, known today as Richardson's equation, by noting that he could derive one equation from the hydrostatic assumption saying how much the pressure will change per time unit. From the thermodynamic equation he could obtain another equation for the rate of change of the pressure per time unit. Since the two independently derived pressure tendencies have to agree with each other he obtained two expressions, both containing the vertical velocity, and that equation could then be used to compute the desired values.

The grid defined by Richardson is shown in Figure (3.1). One will notice that the south-north distance in the grid is 1.8 degrees of latitude. It is also seen that the grid does not have a meteorological station in every grid square. There are none in the north-west corner of the grid. Figure (3.2) shows a very limited portion of the grid. As seen from the points marked M for momentum and P for pressure he used a staggered grid.

Before we discuss a few of Richardson's results it should be stressed that he was engaged in a major undertaking. He had to compute all the terms on the right-hand sides of his model equations, but he had only very simple computing machines which were so slow that the computations took such a long time that a forecast of, say, 24 hours would take several days to produce. From this limited point of view his procedure was impractical, but that would, from

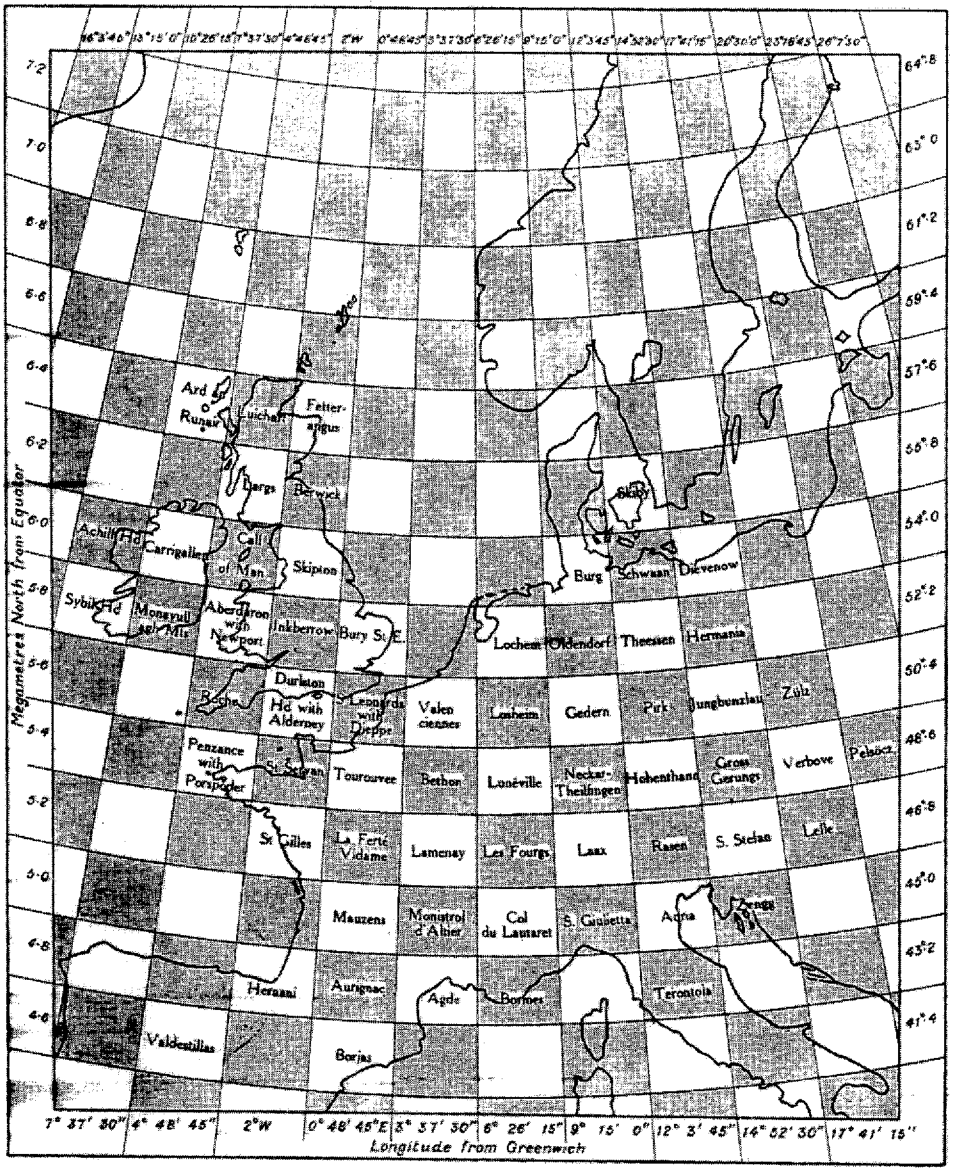


Fig. 3.1: Richardson's grid. The words in the grid are the names of a meteorological station. Note that many of the grid squares have no station.

his point of view, be unimportant if he was able to show that the method produced essentially correct values.

This was, however, not the case. After having computed all the right-hand

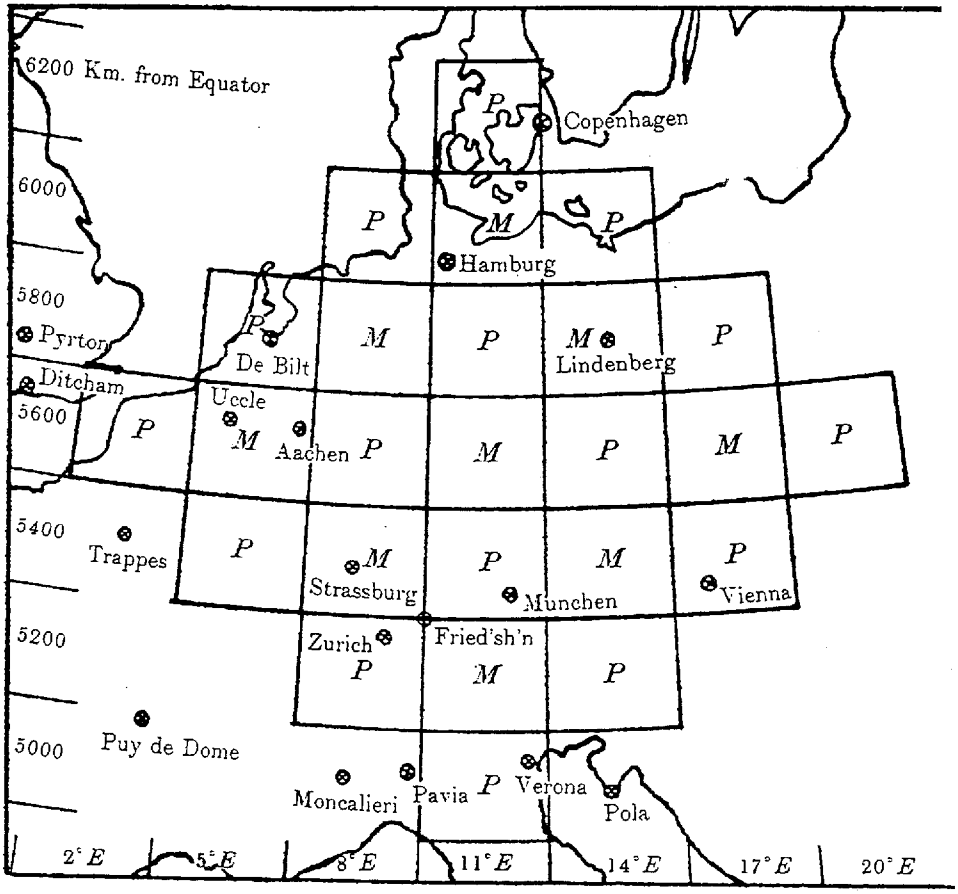


Fig. 3.2: A limited part of Richardson's grid. 'M' and P stand for the point in which the momentum and the pressure are calculated.

sides he multiplied them by a time step corresponding to 6 hours, because the observations were taken with this time interval. He observed first of all that in certain points his predicted change of the pressure was much larger than the actual pressure change. But his computed pressure changes were also much larger than had ever been observed in practice. For example in one point, where the observed pressure change was very small, he found a computed pressure change of almost 145 hPa (hectoPascal equal to millibars) over six hours, and such a large pressure change has never been observed. Consequently, there was something seriously wrong with either his initial state or with his formulation of the model. After discussing the matter in his book he arrived at the conclusion that the error most likely was in the initial state and not in the basic formulation of his model or his computing scheme. As he says: »It is claimed that the above form a fairly correct deduction from a somewhat unnatural initial distribution«.

Much later it was discovered that his proposed computing scheme, in which the computed value of the right-hand side is multiplied by the time step and then added to the preceeding value, actually is an unstable scheme. It would thus be unsuitable for the proposed integration, but this fact can have no influence on the result produced by Richardson since he never made more than a tendency calculation. We have also learned later that if a numerical scheme is to be stable, it normally puts an upper limit on the size of the time step. Also this information, obtained much later, has been proposed as a possible explanation of Richardson's poor result. The argument is, however, not valid for the same reason as stated above. He did not carry out a time-integration with a time step of 6 hours (which would have been too large). He just multiplied the computed tendencies by six hours to see how they compared with observed tendencies.

Richardson already started on his project in 1911. During the First World War, being a Quaker and thus a conscientious objector, he did service with the »Friends' Ambulance« in France. He was attached to a motor ambulance convoy used by the French army. During this period he had, as Chapman says: »..the determination and mental energy to develop further his conception of weather prediction.« He actually lost his manuscript for a while, and it was discovered months later under a heap of coal. Shortly after the war the Meteorological Office was placed under the Air Ministry, a department of war and defense. Richardson resigned in 1920 due to the new association and became head of the physics department of the Westminster Training College (for teachers). Here he continued his meteorological studies and completed his book. Later in life he became principal of the Technical College and School of Art in Paisley, Scotland. During this time he became more and more interested in psychology, especially in relation to the question of war and peace. It appears that he is just as famous in this area as he is in the atmospheric sciences.

Richardson was well aware of the difficulties encountered in his attempt to introduce numerical weather forecasting. As Mr. Ernest Gold (1954) said in an obituary notice: »The results of the computation were strikingly at variance with the observed facts.« Considering the present review processes for scientific papers it is very unlikely that his book would have been accepted for publication. He was, however, also a strong believer in his general approach to weather forecasting, and the later development has shown that his faith in numerical forecasting was justified. In his book he has an small entertaining section called »A Forecast Factory« (Chapter 11/2), but it could just as well be called »Richardson's Dream«. First he estimates how many individuals would be needed to produce a real forecast, and he arrives at the staggering number of 64,000. It has been pointed out by Chapman in his introduction to the paperback edition that an uncharacteristic error was made in this estimate, and that the number of people actually should be four times as large.

The forecast factory is imagined as »a large hall like a theatre, except that the circles and galleries go right around through the space normally occupied by the stage.« He is talking about a global forecast and indicates where each region is located. »A myriad of computers are at work ...but each computer attends only to one equation or part of an equation.« The whole operation is conducted by an »official of higher rank«, and he acts indeed as a conductor of a symphony orchestra, because he is located on a tall pillar in the middle. From there he can control a uniform speed of progress by turning a beam of rosy light upon a region that is running ahead of the rest, and a beam of blue light upon those falling behind.

The whole fantasy finishes with a brief description of the total organization including a research department and an administration. The last sentence of the »dream« is: »Outside are playing fields, houses, mountains and lakes, for it was thought that those who compute the weather should breathe of it freely.«

Later, there has been several discussions of why the computed tendencies in Richardson's calculation turned out to be so very large. Several misinterpretations have been made. Chapman repeats one of them in his introduction to the later paperback edition. He says, quoting Jule G. Charney and Philip D. Thompson, that »it became clear that one reason for Richardson's lack of practical success was that the space and time increments used in his work grossly failed to meet a computaional stability criterion (Courant-Friedrichs-Lewy).« As pointed out earlier, this explanation is untrue. We shall discuss this question in connection with the return to the same system of equations by the numerical prediction community at a much later time.

It is, however, obvious that two necessary conditions of a technological nature would have to be fulfilled before one could think of numerical prediction from an operational point of view. It would be necessary to expand the network of observing stations to a reasonable global coverage including reliable observations of the upper atmosphere covering at least the troposphere and the lower stratosphere, and computing devices of great speed would have to be invented in such a way that the weather forecasts could be ready in good time for use by the operational forecasters. These conditions were fulfilled *in part* after the Second World War.

One may of course also ask if Richardson's book with its detailed discussion of the major (and sometimes minor) driving forces for the general circulation had any impact on the research of the meteorological community. It does not seem to have been the case. In the years after his book (1922) was published to the middle of the century, the meteorological community was mainly occupied by the practical problems of operational meteorology including observing networks and the introduction in most countries of the analysis and empirical forecasting procedures introduced by the Norwegians in Bergen. The emphasis in this approach was totally different from Richardson's numerical interests. Robert M. Friedman (1989) has described the main content of the so-called Bergen School.

4. A new foundation of dynamical mereorology

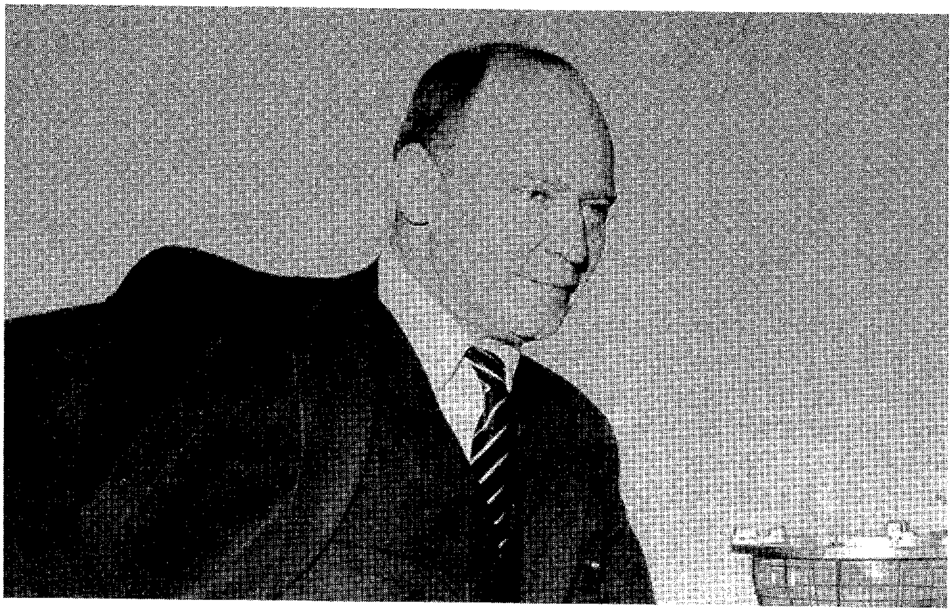
Eighteen years after Richardson published his book a new era started in dynamical meteorology. It was put into motion by Carl-Gustaf Rossby who was born in Sweden and educated in mathematics and fluid dynamics at the Stockholm School of Higher Learning, later to become the University of Stockholm. He was familiar with the main ideas of the Norwegian (Bergen) school from a stay in Bergen for a year and a half, and was interested in both theory and observations. At a rather young age (see Tor Bergeron, 1959) he immigrated to the United States of America on a stipend. He stayed in the U.S.A. for many years since no good job opportunities were available in Sweden. After various jobs with the U.S. Weather Bureau and some practical work involving the creation of an air weather service in California (Horace Byers, 1959), he became the first head of the first department of meteorology in the U.S.A. at the Massachusetts Institute of Technology in Boston. At a later time he accepted a professorship at the University of Chicago.

Rossby was interested in many aspects of the atmospheric and oceanic sciences and prefered to treat new problems rather than to continue the development of the meteorological approach that he had been exposed to in Scandinavia, particularly in Bergen. He had an ability to formulate simple problems of a fundamental nature, and he was the first to ask the question of why and how the atmosphere displays an almost geostrophic equilibrium on the large scale above the atmospheric boundary layer. In a strict sense geostropic equilibrium exists if there is an exact balance between the pressure force and the horizontal component of the Coriolis force, where the Coriolis force is defined as the opposite of the Coriolis acceleration. He investigated this basic question (Rossby, 1937 and 1938) by assuming that a limited strip of a fluid (or an atmosphere) had been given a uniform current, corresponding to a certain kinetic energy. With the action of the Coriolis force the current will turn to the right in the Northern Hemisphere, and fluid levels will increase to the right and decrease to the left. Rossby then computed the water levels and the current speed when the Coriolis force and the pressure force eventually would come into a complete balance.Among other conclusions he showed that the kinetic energy in the final state was smaller than the initial kinetic energy given to the strip due to the fact that gravity waves carried kinetic energy away from the strip. Rossby's original paper was later generalized to stratified fluids by Albert Cahn (1945) and Bert Bolin (1953).

In several important papers in 1939 and 1940 he investigated the speed of atmospheric waves in an artificial atmosphere which again was made as simple as possible. Rossby's simplified approach to a problem was therefore the opposite of the very general and complicated formulations used by Richardson as described in the preceeding chapter. In the case in question he decided to use an atmosphere in two-dimensional, horizontal motion. In such an atmosphere the flow will have no divergence since it would be connected with vertical motion, so the flow can be called non-divergent.

The immediate reaction to such a model would be that it would be rather uninteresting since it is so far removed from the real atmosphere. His model contained no vertical velocity and no humudity. Clouds and precipitation could not be created in the model. On the other hand, while his assumptions certainly cannot be fulfilled exactly in the real atmosphere, his simple model atmosphere could be a first approximation to the real atmosphere if the divergence and thus the vertical velocity are small compared to the quantities dominating in strictly horizontal flow. A key quantity is the vorticity which is a measure of the rotation properties of the atmosphere. Rossby must have felt in his bones that the divergence would be smaller in magnitude than the vorticity although he does not actually investigate the problem in his paper. In any case, he completed his investigation of the properties of such a fluid based upon an equation which says that the vorticity is conserved for a particle in an atmosphere in horizontal motion.

Using the model he also derived his very famous formula for the speed of waves. If we assume that the motion in the atmosphere consists of a uniform velocity from west to east at speed U, then he could show that the waves – riding so to speak on the constant zonal current – will move at a speed smaller than the speed U, and that the reduction of the speed was determined by certain aspects of the rotation of the Earth. Intuitively, one can understand that the Earth's rotation will be relevant, because in the horizontal wave motion a particle will sometimes be at higher latitudes where the speed of rotation is smaller than when the particle is at the lower latitudes. In any case, Rossby's derivation showed that the reduction in speed was influenced also by the wavelength in such a way that the speed of the longer waves were re-



Carl-Gustav Rossby (1898-1957)

duced more than short waves. In view of these facts it is understandable that one can determine a wavelength where the reduction in speed is equal to the basic zonal speed (U) in which case the wave will not move at all, i.e. the wave is stationary. If the wavelength is larger than the one giving a stationary wave the wave will actually move from east to west, opposite of the basic current. Synoptic meteorologists on his staff investigated whether or not actual atmospheric waves behaved in accordance with the derived velocity and found that it was the case with reasonable accuracy and more could not be expected due to the severe assumptions in the model formulation.

Rossby's basic model of approximating the atmospheric flow by a two-dimensional, non-divergent flow should later play a large role in the first attempts on numerical weather prediction due to its simplicity. The waves on a uniform zonal current investigated by Rossby are neutral in the sense that their amplitude remain constant. However, atmospheric waves grow and decay during their motion. What is responsible for the growth of atmospheric waves? It turns out and was shown by later investigations that growth requires non-uniformity in the basic current. One may divide the non-uniformity in two forms although both of them are present in an actual atmosphere. However, for the purposes of theoretical investigations it is almost always an advantage to consider one effect at the time. In any case, non-uniformity may exist as a change in the zonal wind in the south-north direction, i.e. as hori-

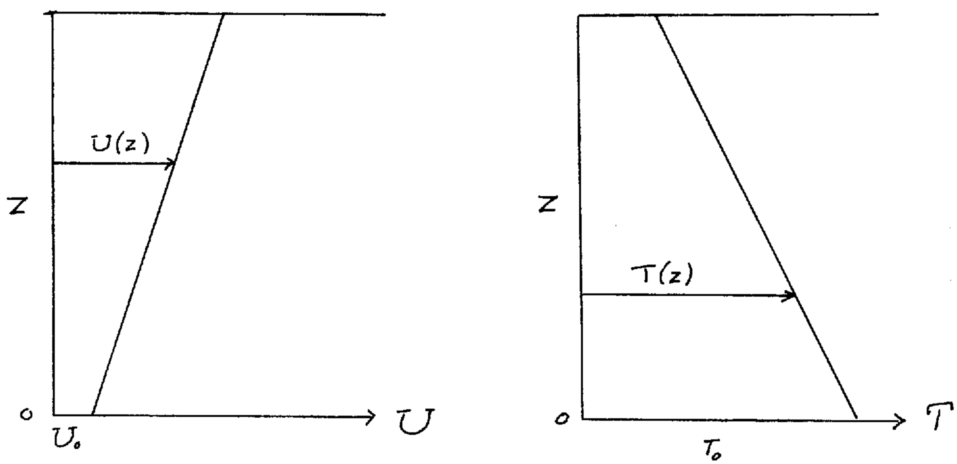


Fig. 4.1: The basic state in Charney's investigation of baroclinic stability. The zonal wind increases linearly with height, and the temperature decreases linearly with height.

zontal shear, which means change per unit distance, but it can also be introduced as vertical wind shear, which measures the rate of change of the horizontal wind in the vertical direction.

As it happens the latter problem dealing with only the vertical windshear was solved first by Jule Gregory Charney (1947), while he was a Ph.D. student at the University of California in Los Angeles. The basic model has a zonal (west to east) wind increasing linearly with height, but with no change in the lateral direction. It was also decided to have a stratification characterized by a linearly decreasing temperature with height in the most simple formulation of the model (see Figure 4.1). Thereafter one investigates what will happen to waves of small amplitude superimposed on the basic state. The mathematical aspects of the problem are far from simple, but Charney succeeded in showing that when the vertical shear in the basic flow exceeds a certain limit that is wavelength dependent, then the waves will grow in amplitude. The longer the superimposed waves are, the larger is the vertical windshear necessary to get the waves to grow. Charney provided therefore a model that showed one mechanism for growing waves.

It should be pointed out that under balanced conditions a certain increase in the zonal wind with height corresponds to a temperature variation in the south-north direction. To be exact, a westerly current increasing with height corresponds to a temperature variation in the horizontal direction with the high temperatures to the south and the low temperatures to the north in the Northern Hemisphere.

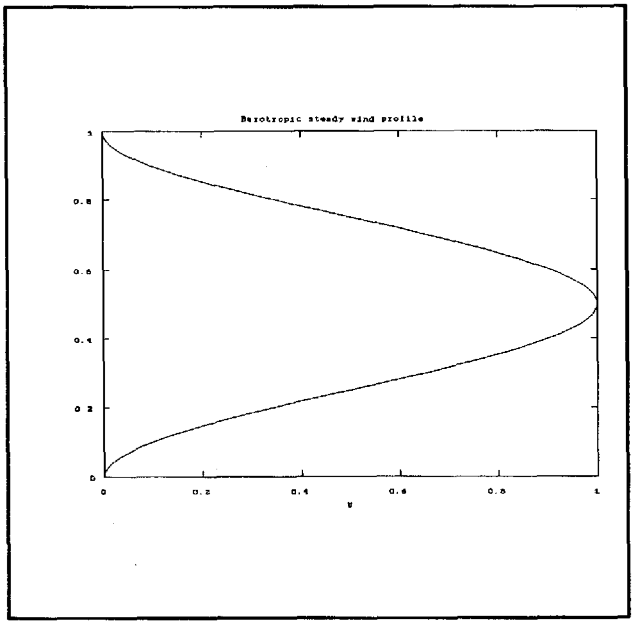


Fig. 4.2: An example of the variation of the zonal wind in the south-north direction in Kuo's investigation of the barotropic stability. The profile has a maximum in the middle (say, 45 deg. north) and is zero at the southern and northern boundaries.

The basic state described above is called a baroclinic state because it has horizontal variations in the temperature, and the growing waves found by Charney are therefore called baroclinic waves. Many investigations have been made of the growing baroclinic waves under various conditions, but with results of a similar nature. Since the first investigation involved rather complicated mathematics, it has been simplified in various ways. A typical simplification is to replace the continuous atmosphere by a number of discrete layers where the minimum number of layers is two. It is also true that Charney's results have been slightly modified by a South-African meteorologist, A.P. Burger (1958), who pointed out that growing waves are present for almost all conditions. However, these new solutions, apparently overlooked by Charney, grow so slowly that it takes quite a few days to double the amplitude, while shorter ways may double the amplitude in less than a day.

The other possibility of having horizontal, but no vertical shear in the basic current, was investigated at the University of Chicago by H.L. Kuo (1949), (see Figure 4.2). Also in this case it turns out that for sufficiently large horizontal variation of the zonal wind one obtains growing amplitudes of the waves. This form of growth is called barotropic instability. Both Charney and Kuo could show that the conditions necessary for growth of the waves were present repeatedly in the atmosphere, and it was therefore concluded that the real atmospheric disturbances are of a mixed barotropic-baroclinic type.

It should be pointed out that the investigations of barotropic and baroclinic instability are based on the same type of equations as used by Rossby in his investigation of neutral waves.

The theories that have been described above gave a new understanding of why the waves in the upper atmosphere exist. It gave also insight in the properties of the waves. For example, the purely baroclinic waves attain such a structure that they transport sensible heat from south to north in agreement with results from observational studies. On the other hand, the barotropic waves transport momentum from the zonal current to the waves, which is contrary to what one finds on average from calculations based on data. However, there are periods of so-called blocking where the momentum transport goes the other way, meaning that the zonal current provides the energy for the waves.

The new theories replaced the older theories proposed by the Norwegian school, in which one assumed that the waves came from the growth of small disturbances on already existing frontal surfaces. The investigations on which this view was based were originally carried out by Haldor Solberg, a pupil and later associate of V. Bjerknes. There is an interesting story in connection with these investigations. It is an example of how the scientific views may change with time.

Solberg's (1928) original purpose was to find whether or not growing disturbances could be found on frontal surfaces. Once again a simple basic state was formulated. Frontal surfaces are sloping surfaces with the heavier, cold air below and the lighter warm air above the surface. Such a surface will therefore normally intersect the ground. Assuming that we can define the top of the atmosphere as a horizontal surface we should have a basic state that is si-

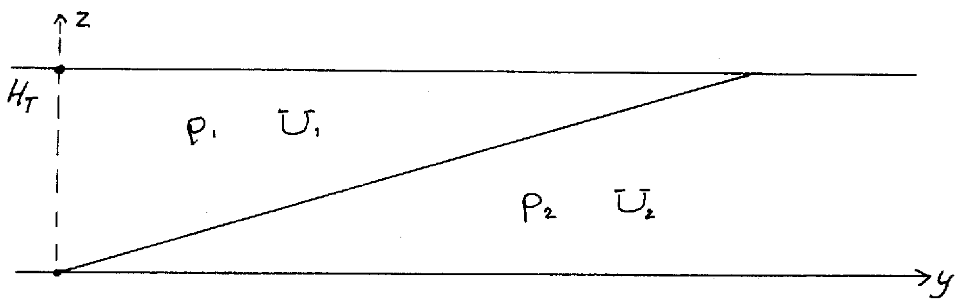


Fig. 4.3: The general case of frontal stability. The atmosphere is bounded below by a flat Earth and above by the 'effective' height of the atmosphere. The sloping line indicates the frontal surface separating the lighter air above and the heavier air below. Boundary conditions in A and B become very difficult in the general case of non-hydrostatic flow.

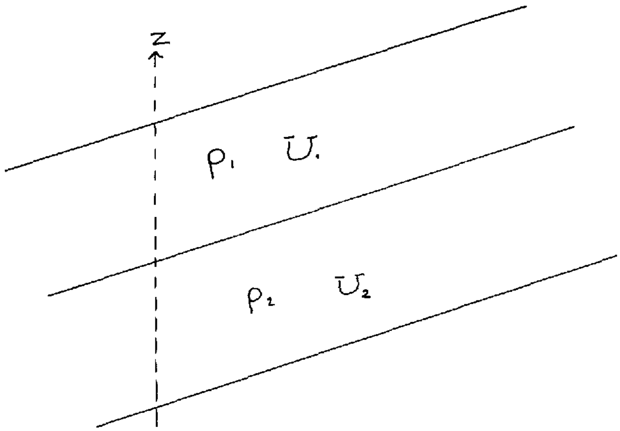


Fig. 4.4: The problem solved by Solberg. The central sloping line indicates the frontal surface. The artificial boundary surfaces are at a finite distance from the frontal surface. The solution is unrealistic, because the resulting disturbances receive their energy from the kinetic energy in the basic state.

milar to Figure 4.3. It is, however, understandable that to investigate growing waves in the setting given in Figure 4.3 will lead to severe mathematical problems particularly due to the conditions that have to be satisfied at the places where the sloping surface intersect the top and the bottom plates. Therefore Solberg replaces the basic state by a new one given in Figure 4.4, where the lateral plates are parallel to the frontal surface. He proceeds to find that in this basic state there are growing waves with a wavelength and a wavespeed of the same order of magnitude as those of the growing observed cyclonic waves. It is no wonder that he felt that he had given a major contribution to the understanding of the birth of the cyclonic waves so important for the whole Bergen School. However, later it was discovered that his mathematical solution was of such a nature that the potential energy increases for these growing waves, and it follows that the waves grow by converting kinetic energy to potential energy in contradiction to what one finds from observational studies.

The original problem was considered again, first by N. Kotchin (1932) and later by Erik Eliasen (1960) who used a basic state as seen in Figure 4.5. He introduced the hydrostatic approximation in the problem, overcame the difficulties with the boundary conditions and obtained a numerical solution to the problem. He also found growing waves with wavelengths and wavespeeds in agreement with observations, but in his case the energy conversion was from the potential to the kinetic energy in agreement with the observational studies.

In view of these results it will be necessary to answer the following two questions: Are the atmospheric disturbances growing on already existing frontal surfaces? Or, are the waves growing due to the vertical and/or the horizontal windshear? If the answer is yes to the first meteorological question, we should also provide a theory for how the sharp frontal surfaces are for-

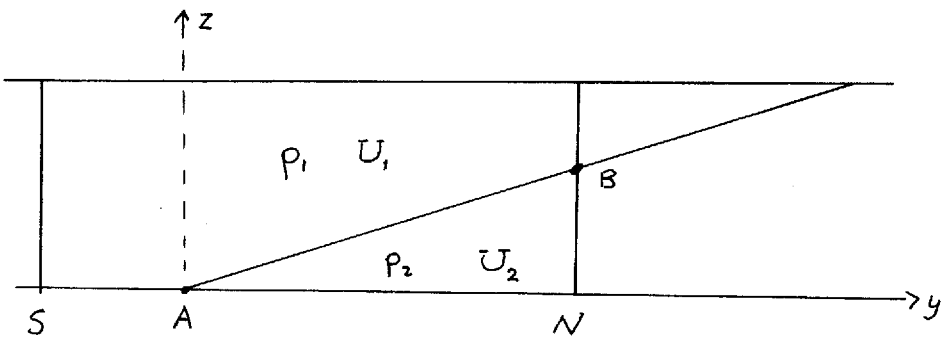


Fig. 4.5: The problem solved by Eliasen. He introduces hydrostatic conditions aa well as a southern and a northern 'wall'. The resulting unstable waves draw on the potential energy of the basic state.

med. On the other hand, if the answer is yes to the second question, we should be able to show that the frontal surfaces that undoubtedly exist in the atmosphere are formed as a result of the baroclinic-barotropic growing wave.

These questions resulted in a number of investigations of the formation of frontal surfaces of which the first one is carried out by Arnt Eliassen (1959), and was later followed by several others. Replacing a description of the various interesting mechanisms proposed for the formation of fronts, which is too far away from our present purpose we may say that numerical simulation of growing waves of the baroclinic-barotropic kind clearly indicates that fronts are formed as a result of the kinematic and dynamical developments in the growing and maturing waves. This empirical evidence combined with the various theoretical considerations shows that we have a theory both explaining the growing waves and the formation of fronts, and we have thus gained considerable new insight and are able to answer the questions posed above.

All the theories are based on simplified situations. The real atmosphere is much more complicated with many processes going on at the same time. The emphasis has also been on growing waves because we were interested in answering the question: How are waves formed in the atmosphere? But real waves have a life cycle: They are born, they grow, they decay, and they die. In order to describe the life cycle it is necessary to go to numerical simulations, and a weather forecasting model is exactly a model that among other phenomena should be able to simulate the whole life cycle of the waves. In a sense we are therefore coming back to the main topic: Weather forecasts. However, most of the studies described above were carried out before or during the very first phases of the gradual development of the field of numerical forecasts. The material covered in this chapter is thus a description of the knowledge available to the meteorological community at the time when it became ready to attack the forecasting problem again after Richardson's imaginative, but unsuccessful attempt.

5. From the complicated to the simple

After World War II the necessary conditions for a reconsideration of numerical weather prediction were closer to being satisfied. Due to the invention of the radiosonde for upper air observations and the requirements for meteorological support to the Air Forces during the war, a hemispheric network of surface and upper air observations had been created. The maintenance and further improvement of the network was necessary to give meteorological support to the steadily growing civil air traffic all over the world.

The radiosonde is an inexpensive device consisting of a barometer to measure pressure, a thermometer to measure the temperature and a hygrometer to measure the humidity. The three instruments are packed in a small box and connected to a radio transmitter that can transmit the three measurements back to Earth in a coded form. The box is attached to a string connected to a balloon that is released from the ground and rises through the atmosphere at a constant rate of ascent. During its flight it will drift with the horizontal winds, and when the path is followed from the ground one may determine the horizontal wind from the displacement of the balloon in the horizontal direction. From the measurements of pressure and temperature one may through the hydrostatic equation, mentioned earlier, compute the height of the balloon. In this way one may determine the height of the standard pressure surfaces, the temperature, and the horizontal wind. On this basis one is able to construct the topography, the isoterms and the windfield in selected pressure surfaces. Such upper air analyses were routinely made by hand in the weather services twice every day.

The network of stations will never satisfy the operational meteorologists who steadily long for more information. It goes without saying that surface and upper air stations may readily be established on land, but it is more expensive to do the same over the oceans. During 1940-50 a number of ocean stations were established by various nations to provide a network for the aircrafts. The Atlantic Ocean was reasonably well covered, but the much larger Pacific Ocean is more difficult to cover. The operational meteorological satellites have gradually replaced the old weather ships that were very expensive to keep on stations continuously. On the other hand it has taken the meteorologists considerable time to learn how to extract useful information from the satellites.

The other necessary condition was the availability of fast computing devices. This requirement was also in the process of being satisfied in the years immediately after World War II. The first electronic computers had been invented in the U.S.A. and in England. The invention of the first electronic computer is often ascribed to John Mauchly, an electrical engineer from Philadelphia. The truth is that many of the basic principles of the first computer were invented by John V. Atanasoff, a professor of physics and mathematics at Iowa State University. He had also a good background in electrical engineering. Together with his associate, Clifford Berry, also an electrical engineer, he built the first computer in the years 1939-41 according to C.R. Mollenhoff (1988). Nevertheless, the invention is often ascribed to John Mauchly, because Mauchly, according to Mollenhoff, »borrowed« many of Atanasoff's ideas for the design and construction of the ENIAC and other computers.

The new electronic computers required planning of the calculations in advance. It is necessary to produce a program for the desired calculations. The well-known mathematician John von Neumann gave very important contributions to the logic design of the computers. The main point is that the program for the calculations is stored in the computer, and that the required data as well as the results of the calculations are stored in the computer and can be used for later calculations using the same program.

Compared to the computers available today for research and operational activities the first computers were physically large, slow and unreliable, but compared to what had existed at earlier times they permitted calculations that had been impossible to carry out on desk-top mechanical or electrical calculators. For the meteorological community it was important that John von Neumann had created a project at the Institute for Advanced Studies at Princeton. He was convinced that the meteorological prediction problem should be included among those to be tried when the so-called Maniac computer was ready for use in Princeton. In the meantime one had access to the Eniac computer located at the Aberdeen Proving Ground in Maryland.

Discussions involving von Neumann and Rossby and several others took place in 1947 and 1948. Jule Charney also participated in at least one planning meeting, but he was on his way to Oslo, Norway for a post. doc. period. Dr. Philip D. Thompson, who was a meteorologist and officer in the Air Weather Service of the U.S. Air Force, had been assigned to John von Neumann in the fall of 1946. He was also a participant in planning the meteorological project. The U.S. Weather Bureau, represented by the Chief, Dr. Reicheldorfer, and the Director of Research, Dr. Harry Wexler, showed an interest in the project which got under way in 1948 with Jule Charney as the leader.

An important issue was the strategy to be used for the new attack on the problem of numerical weather prediction. At the time one had only the ex-

Jule Gregory Charney (1917-1981)

perience from Richardson's tendency calculation, which indicated that something was seriously wrong with such a general approach. Through an exchange of letters between Thompson and Charney one can get an impression of the speculations dominating the strategy discussion. The first letter from Thompson to Charney is written on February 3, 1947 when Charney was still in Chicago. Thompson posed the question: »Why don't perturbations, like say, the travelling cyclones, move at velocities comparable to that of sound, meaning, what new and essentially different physical mechanism limits how fast these disturbances are propagating?« Rather unusual for Charney, he replies as soon as February 12 indicating that he was aware of the problem. In any case, his reply contains a detailed analysis of the waves in a homogenous fluid with a free surface where inertial-gravitational waves as well as Rossby waves may exist. While this analysis had been carried out by Rossby, Charney proceeds to show how the fast gravity type waves can be removed in this simple case. They may simply be removed by introducing the geostrophic wind replacing the real wind in tendency and advection terms, but not in terms dealing with the divergence. By this procedure the fast waves are eliminated as possible solutions. They are so to speak filtered out. It is, however, also clear that Charney has not at this time solved the more general filtering problem for the atmospheric equations. This is seen in a sentence toward the end of the letter where he writes: »... if you accept the consequences of the above reasoning, you will perhaps share my conviction that there is a general type of approximation or transformation or what have you that will eliminate the noise, and the problem is to find it.«

The problem is taken up again in another letter from Charney to Thompson written November 4, 1947 from Oslo. In this letter, which contains many other points of a personal nature, Charney states:«... I have come up with the answer to at least one of the most vexing aspects, namely the practical impossibility of determining the initial vertical velocity and acceleration fields with the necessary accuracy. The solution is so absurdly simple that I hesitate to mention it. It is expressed in the following principle. Assuming conservation of entropy and absence of friction in the free atmosphere, the motion of large-scale systems is governed by the laws of conservation of potential temperature and potential vorticity, and by the condition that the field of motion is in hydrostatic geostrophic balance. This is the required filter!« (Lindzen et al., 1990):

One recognizes in the letter the main elements in the two important papers published a little later, i.e. the paper on the scales of atmospheric motion (Charney, 1948) and the paper on the physical basis for numerical weather prediction of the large-scale motion of the atmosphere (Charney, 1949). In the same letter Charney mentions that he would like to come to Princeton when he leaves Norway. Thompson had discussed this wish with von Neumann. In any case, when Charney came back to the U.S.A., he came to Princeton as the leader of the meteorology project and stayed there until 1956.

Charney had thus outlined a general strategy. One could use his system of filtered equations, later called the quasi-geostrophic system, as a first step in experimental numerical prediction. The system could be formulated in practice by using a number of pressure levels from the bottom to the top of the atmosphere. The number of levels could vary from a minimum of two to an arbitrary number if one wanted to include baroclinic processes. In addition, one had the special case of the barotropic model, originally formulated by Rossby, but now to be used in its nonlinear form. The details of using the general quasi-geostrophic equations for an arbitrary number of levels were worked out by Charney and Norman A. Phillips, who joined the Princeton Project from Chicago, where he had written his Ph.D. thesis on a model, originally proposed by Rossby, containing a number of homogenous layers stacked on top of eachother. The joint paper by Charney and Phillips was published in 1953. The strategy would be to start with the most simple quasi-geostrophic models and gradually move to models with higher vertical resolution as permitted by the available computers.

The filtered equations formulated by Charney gave also at least one reason for the large tendencies in Richardson's experiment. One may say that Richardson's formulation was too general. His system contained not only the desired large-scale meteorological waves, but also the much faster moving external and internal gravity-inertial waves where the word »inertial« refers to the influence of the rotation of the Earth on the gravity waves. Although the hydrostatic assumption eliminated the vertical component of the sound waves, the horizontal component would still be present in the Richardson model. The sound wave is another fast moving wave in the Richardson equations.

The descriptions of the models given above provides an idea of the physical foundation for the new attack on the problems of numerical weather prediction. When the models are to be integrated on the computer one faces a number of problems connected with the numerical procedures that have to be selected. In this regard one could of course to a large extent get ideas from other fields of physics and applied mathematics where equations of the same type had been integrated before. Such integrations had indeed been accomplished for a number of physical problems using either electrical computers or the so-called analog computers that were the forerunners of the electronic computer.

It was, for example, known that the integration with respect to time, which in a numerical integration is a step by step procedure, puts an upper limit on the size of the time step. As one can understand intuitively a small time step should be used if the solution contains high frequency phenomena corresponding to fast moving wave forms. In the simple models employed in the beginning we have really only one type of waves, the so-called Rossby waves. From Rossby's formula one knows that the wavespeed is smaller than the windspeed for typical scales in the atmosphere. One may therefore use the windspeed as an upper limit to the wavespeed.

The actual integration is in one of the methods carried out by defining a square grid on the horizontal plane. Suppose that the distance from one point in the grid to another is d. If the scale of the phenomenon in question is large compared to d, the gridsize, it is possible to obtain a numerical solution with good accuracy. On the other hand, the phenomena having a scale comparable to the gridsize cannot be expected to be computed with accuracy and neither can they be described with good accuracy in the initial state where only the values in the gridpoints are known. Consequently, the gridpoint representation has – just as any other numerical scheme – its limitations. Therefore, the spatial scale of the phenomena that one wants to forecast should be known in advance. Turning our interest to the atmosphere we are in a dilemma because the atmosphere has motion – or, equivalently, kinetic energy – on a wide range of scales including, on the large size, scales comparable to the circumference of the Earth and, on the small size, scales approaching the molecular scales. When it comes to weather forecasting we realize that the available observations will not permit us to resolve scales smaller than scales comparable with the average distance between the observing stations. This means that we must limit our global predictions to the larger scales, say, wavelengths from a couple of thousand kilometers to forty thousand kilometers (the circumference of the Earth). This consideration resulted in horizontal grids with a grid distance of about 300 km.

How large a time step will one be able to use for such a grid? The theory for the determination of the largest possible time step was worked out by Courant, Friedrichs and Lewy (1928). The result of their analysis can be summarized by saying that a particle in a gridpoint may not in one time step arrived in a point outside the four elementary gridsquares surrounding the point. In other words, the maximum time step is determined by the gridsize, d, and the maximum speed of the waves that may be present in the model, i.e. $\Delta t < \Delta s / c$, where Δt is the time step, Δs is the gridsize and c the maximum wavespeed.

Let us look at a couple of examples. If d=300 km and c is the maximum wind speed, say 80 m per s, then the maximum time step is of the order of one hour. Values of this order of magnitude have been used for many integrations of simple atmospheric models. On the other hand, if the model permits the existence of gravity-inertia waves we may have a value of c of the order of 300 m per s. If so the maximum time step wil be of the order of 15 minutes. Very general models, as for example the one formulated by Richardson, will require time steps of this order of magnitude for an integration. However, as pointed out elsewhere, Richardson limited his calculation to an initial tendency calculation.

It will also be understood that as the gridsize has become smaller in recent years, it has been necessary to decrease the time step in order to conserve the numerical stability. Time steps of a few minutes are now common.

Another example of handling the variations in space is to express a given parameter, say the geopotential field, as a sum of functions depending only on the position in space and not on time, while the time dependence appears in the coefficients to these functions. The functions used for this purpose should satisfy certain conditions. Together they shall be able to describe a large variety of scales covering all the scales of interest for the problem. In addition, they should be selected in such a way that they satisfy the necessary conditions at the lateral boundaries for limited regions, and they should have the property of being orthogonal, meaning that one function multiplied by another function belonging to the selected class of functions should integrate to zero over the considered region, while a function multiplied by itself integrates to a non-zero value. As one will understand, we are really talking about a lot of mathematics, and the formulation of a model in this scheme does require additional mathematical work. On the other hand, the formulation has to be done only once.

In this so-called spectral scheme, done for the first time for a meteorological barotropic model in all details by Platzman (1960), the numerical forecast is then limited to the prediction of the time-dependent coefficients of all the functions. When a field representation is required at a given time, it is obtained by adding the contributions from all the functions.

The spectral method has both advantages and disadvantages. Like the gridpoint method it cannot resolve all scales, since it can have only a limited number of functions. The gridsize determines the minimum scale resolvable by the model. Similarly, the last term in the sum of function will determine the minimum scale. One advantage is that it is very easy to formulate it in such a way that the spectral model is energetically consistent. Another advantage is that the field equations used in the gridpoint model are replaced by fewer equations that are time-dependent only. A disadvantage is that the spectral formulation has difficulties in giving an accurate description of rapid horizontal variations as they are experienced for example in crossing a coastline. A gridpoint model is local in the sense that processes may be calculated in the gridpoint itself. The spectral model in its pure form is non-local, because each term in the series represents a certain spatial distribution.

It is therefore not surprising that the present models combine the advantages of both kinds of representation. Such hybrid models have become possible with the increased storage in computers, the increased speed of transfer from one part of the computer to another, and especially the design of mathematical procedures that permit a very rapid conversion from a field representation to a spectral representation and vice versa. These methods, called 'Fast Fourier Tranforms' (FFT), are widely used in the integrations of the model equations.

6. The project at the institute for advanced studies

The chief of the whole Princeton Computer Project was John von Neumann. His life is described by Heims (1981), who has written a so-called double biography of von Neumann and Norbert Wiener. Several aspects of the Project are also described by Thompson (1983).

Charney arrived in Princeton in the late spring of 1948 from his visit in Oslo, Norway. At that time Philip D. Thompson and Gilbert Hunt were part of the project in Princeton. It took considerably longer than expected for the Institute's Maniac computer to be finished. It seems that it did not become available before late 1952. In the meantime, the steadily growing group completed a number of studies related to numerical weather prediction, but not requiring the computer. Among these studies one may mention an investigation of the influence of the Earth's topography on atmospheric flow carried out by Charney and Eliassen (1949). The study contained also a one-dimensional forecast carried out using the barotropic vorticity equation along a single middle latitude, but with an assumed lateral scale. A more extensive study was carried out along the same lines by Bolin and Charney (1951).

Another study dealing with the physical processes responsible for the existence of the very long waves in the atmosphere was carried out by Joseph Smagorinsky (1953). He emphasized the role of the heating in the atmosphere, and he showed that the differential heating gave a considerable contribution to the creation of stationary very long waves, especially in the lower part of the troposphere. An interesting part of the study was the fact that while the effect of the mountains in principle is the same in winter and in summer, the heating is mainly located over the oceans in the wintertime and over the continents in the summertime. These facts explain in a qualitative way the seasonal variations in the strength and position of the very long waves. The study became his doctoral thesis at New York University.

Charney and Phillips (1953) formulated prediction models of the quasi-geostrophic type for an arbitrary number of levels. The idea was to do the job once and for all. A main point in the general model was that if the geopotential of the isobaric surfaces were known at, say, equally spaced isobaric levels, then the vertical velocity and the temperature should be carried at intermediate levels halfway between the geopotential surfaces. The most elementary of these models carried the geopotential at 250 and 750 hPa, while the vertical velocity and the temperature was at 500 hPa.

One of the purposes of the Princeton Project was to experiment with various relatively simple models to see if it was possible to produce numerical weather predictions that were at least as good and hopefully better than the empirical forecasts produced at the time. It is thus understandable that the operating weather survices in the U.S.A. would have an interest in the project. In addition, a number of university- related meteorologists were interested in joining the project. As seen above, it had also a program of visiting scientists.

The U.S. Weather Bureau provided three scientists: George P. Cressman, Fred S. Shuman and Joseph Smagorinsky. They were all to be engaged in numerical studies at a later time. Cressman and Shuman stayed with the short-term predictions, while Smagorinsky soon turned his interest from these projects to the problems of simulating the atmospheric general circulation by numerical model integrations. (See also Chapter 8).

The first real forecasts were made on the Eniac computer. They were based on the nonlinear barotropic vorticity equation which was integrated over a limited region dictated by the capacity of the computer. The results were published by Charney, Fjørtoft and von Neumann (1950). Ragnar Fjørtoft was a visitor from Norway. The four forecasts were a success compared to the tendency calculations of Richardson because the 24 hour forecasts looked entirely meteorological although they were of course far from correct. A 24 hour forecast took actually more than 24 hours to be completed on the Eniac Computer.

The decision to employ the barotropic model for the first forecasts could very well have been based entirely on the capacity of the computer. It has been said that the suggestion to use the barotropic model as a first model was given to von Neumann by Rossby. However, in the interview with Jule Charney, conducted by George P. Platzman, it is maintained that the decision to use the barotropic model was made by Charney. The interviewer argues with Charney on this point and mentions that several conversations took place between Rossby and von Neumann. He finds it quite unlikely that Rossby, who after all had worked with barotropic considerations for about 10 years, should not have made the suggestion. However, Charney disagrees and says that Rossby did not think in terms of numerical integrations at all. In support of Charney we may say that whenever he speaks of the meetings between Rossby and von Neumann he mentions Rossby's very general approach, sounding as if Rossby eventually wants the integration of a very general set of equations. However, Rossby maintained himself that he wanted the barotropic forecasts and had suggested this model as the first one.

The surprise was of course that the barotropic model was more realistic than expected. As one would expect from the possibility of growing waves due to the horizontal shear, some cases of cyclogenesis was predicted by the model. Charney states in the interview with Platzman, who himself participated in the Princeton project, that the barotropic model in general was underestimated as a practical and useful forecast model, and that the group in Princeton was surprised by the quality of the forecasts.

Charney mailed a couple of the barotropic forecasts to Richardson who was still living in England. He was apparently quite impressed. Mrs. Richardson thought that the forecasts were closer to the observed state 24 hours later than to the starting state. In other words, the forecasts were better than persistence – meaning forecasting no change. In his reply Richardson congratulated Charney on the results.

The strategy was to move to baroclinic models. It would have been natural to take the two-level, quasi-geostrophic model as the first example, but Phil-

John von Neumann (1903-1957)

lips had worked on a model consisting of two homogeneous layers while he was a graduate student under Rossby in Chicago, and that model in its nonlinear form became the next model to be tested. As one can see from the literature, one of the test cases was a severe development of an east coast storm in November, 1950. It caused severe damage in a number of places. The Finnish meteorologist, Erik Palmén, at the time at the University of Chicago, had made detailed synoptic studies of the development of the storm. It was a very sudden and rapid development of a major cyclone on the Northeast coast of the United States. The two-layer model did not catch the development except for an indication of a rather weak disturbance in the forecast. However, the group then went to a three-level model with a careful selection of the reference levels. This model, which had better information at the low and high levels, made a much better, although not perfect, forecast of the formation and development of the storm. Charney gave a lecture at the U.S. Weather Bureau presenting the forecast of this so-called Thanksgiving storm. He said later that he thought that the presentation impressed the representatives of the Weather Bureau to an extent that they started to think about numerical weather prediction as a possibility for forecasts in the future. This development will be the topic of the next chapter.

As mentioned above the three-level quasi-geostrophic forecast did a good job in some cases, but certainly not always. An analysis showed that the use of the geostrophic assumption in calculations of the vorticity and the advections created some difficulties because of the variation of the Coriolis parameter with latitude. It can be shown that an arbitrary horizontal windfield can be written as a sum of two windfields of which one of them has vorticity and no divergence, while the other has divergence, but no vorticity. What one really wanted in the models was that they should be quasi-nondivergent and not quasi-geostrophic. This means that the horizontal wind should be the part of the total horizontal wind that has no divergence, but all the vorticity. Such a windfield is characterized by a scalar quantity called the 'streamfunction'. The gradient of the streamfunction gives the magnitude of the nondivergent wind, and the vorticity can be calculated as the Laplacian of the very same streamfunction. But how could one obtain the nondivergent wind (or, equivalently, the streamfunction) from the observations? Charney and Thompson derived, independently of each other, an equation, the so-called balance equation, where one could obtain the streamfunction from a knowledge of the height field which of course is observed. And from the streamfunction one could calculate the nondivergent horizontal wind and the vorticity. This generalisation of the quasi-geostrophic models to quasi-nondivergent models resulted in an improvement of the forecasts. Even so, the quasi-nondivergent forecasts were far from perfect. The main assumption in the development described above is that the divergent part of the wind is small as compared to the nondivergent part. This statement is true because the divergent part of the wind is closely related to the vertical velocities, and they are small compared to the horizontal winds.

The Princeton group engaged in a number of other dynamical studies. The most famous of these is Phillips' simulation of some major aspects of the general circulation of the atmosphere using the two-level quasi-nondivergent equation, published in 1956 in the Quarterly Journal of the Royal Meteorological Society. This experiment is the first of many simulations of the general circulation carried out with more complicated models by others at a later time. In order to use the two-level model for a simulation of the general circulation it is of course necessary to add a description of the heating field and the dissipation of the kinetic energy by frictional forces. Phillips used a very simple specification of the heating given by a linear and time-independent function having heating in the southern part of a rectangular region and cooling in the northern part. The dissipation is partly specified by the frictional force in the atmospheric boundary layer and partly by a diffusion term where the latter perhaps is there for numerical rather than for physical reasons. The model contains no moisture so clouds and precipitation cannot be present in the model. Starting from a state of rest the heating will gradually create higher temperatures in the southern half and lower temperatures in the northern half of the region. These horizontal temperature gradients are equivalent to an increase of the wind with height. When the critical level is passed, baroclinic instability will be responsible for the creation of waves in the streamfunction field, and the further developments of the waves results in lows and highs at the lowest surface (corresponding the surface of the Earth).

The model simulates therefore essential parts of the general circulation, and Phillips furthermore shows that the energy levels and the energy conversions are in qualitative agreement with what was known at the time from observational studies. This study was the first numerical study of the general circulation of the atmosphere, and such simulations have been carried out later by many groups using much more general models.

The actual experiment was carried out in two steps. In the first part of the experiment Phillips integrated the model using the zonally averaged equations, thus disregarding all waves. He obtains a zonal windfield with maxima in the middle of the channel and maximum wind velocities of almost 24 m per s at the lower level (750 hPa) and 36 m per s at the upper level. The temperature field varied from 30 deg. C at the southern boundary to -30 deg. C at the northern boundary. The mean meridional circulation had a single cell of the Hadley type with rising motion in the south and sinking motion in the north. The maximum speed in the mean meridional circulation was about 3 cm per s. Starting from a state of rest the model reached the state described above in 130 days or a little more than 4 months.

After the initial experiment he went to the full equations and introduced 'small random disturbances'. Due to the baroclinic nature of the developed zonal flow one would expect the development of atmospheric waves in the model. Such a wave started to appear after about 5 days, and it could be followed to about day 25, after which the model started to behave in a more irregular way necessitating the interruption of the experiment due to a form of numerical instability. The maps of the waves show a good similarity to the observed atmospheric flow. The energy relations in the model also compare favorably with similar calculations using atmospheric observations. Figure (6.1) shows a picture of the surface pressure distribution as obtained in the experiment.

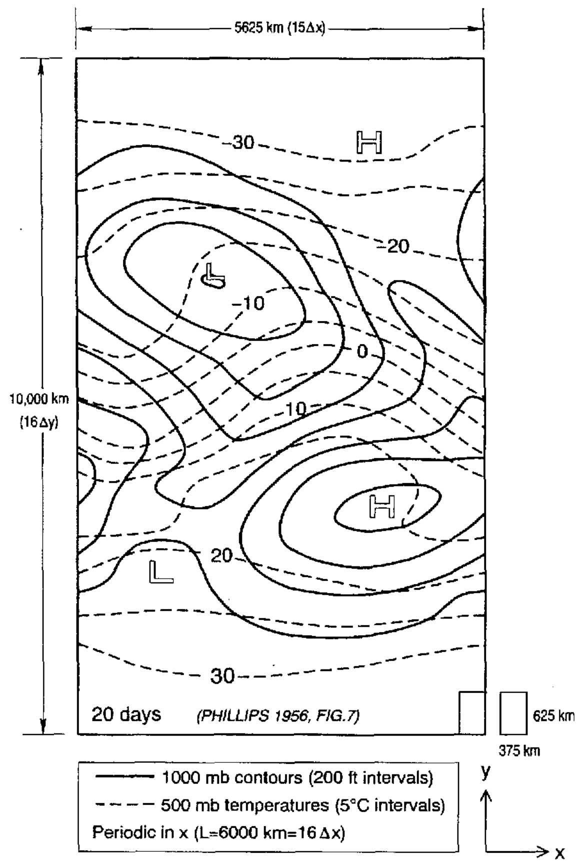


Fig. 6.1.: A picture of the surface pressure distribution as obtained in the experiment

Phillips was thus capable of simulating important aspects of the general circulation of the atmosphere using a relatively simple two-level, quasi-nondivergent model.

The main accomplishment of the Princeton prediction group was to show that numerical weather prediction could be carried out with reasonably good results. It carried the project to a level where the operational forecasting agencies were convinced that numerical forecasting was the method for the future. The dynamic leader of the forecast project was Jule Charney. He had a major impact on the birth of numerical weather prediction through his development of the quasi-geostrophic and quasi-nondivergent models and the testing of these models on quite a few cases.

The Princeton group existed to 1956. At this time John von Neumann had left the Institute for Advanced Studies, had transfered to Washington, D.C. and was engaged in other work as the Chairman of the Atomic Energy Commission. Charney and Phillips were offered positions at the Massachusetts Institute of Technology where they joined the Department of Meteorology. The U.S. Weather Bureau visitors returned to Washington, D.C. to engage in numerical weather prediction on the operational level and in simulations of the general circulation of the atmosphere.

Charney gave very essential contributions to the development of numerical prediction. His major papers are the investigation of baroclinic instability at an early stage and his formulation of the quasi-geostrophic models a little later. After his arrival at the Massachusetts Institute of Technology he engaged in problems dealing with the climate of the Earth and in a number of oceanographic investigations of which the most famous is his Golf Stream investigations. He gave many good ideas to national and international meteorology. It was, however, characteristic for him that as soon as the ideas were accepted by the community, he let other people carry them through the time-consuming and sometimes dull work in the panels and committees. Personally, Charney was a man with political and cultural interests. He worked easily with his graduate students and young researchers in the department as long as these people were of a sufficient high quality and able to follow up on the ideas.

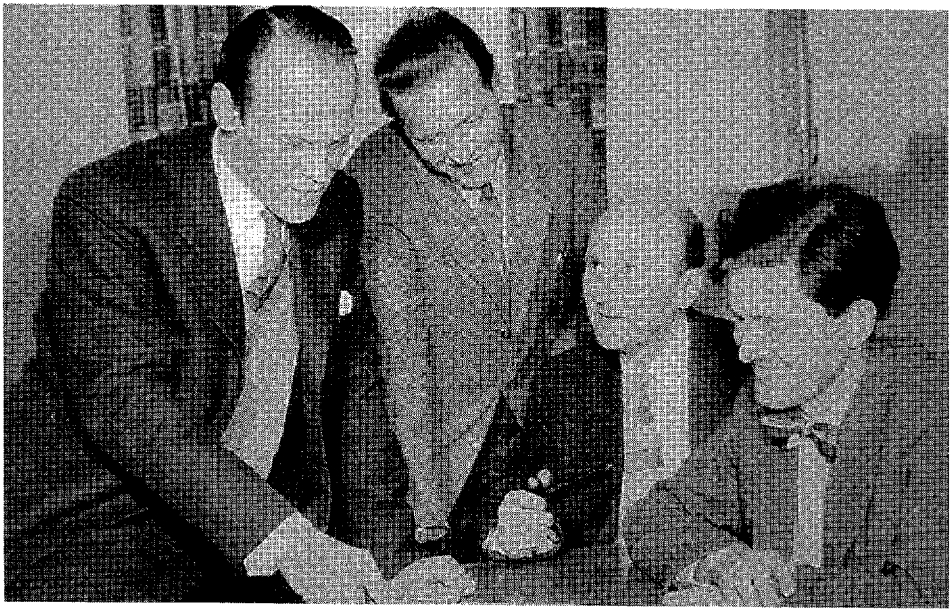
Although Charney and Phillips worked well together and have published joint papers, they were very different as individuals. While Charney on occasion used his personal charm to reach his goal, Phillips appeared to be a more serious person. His approach to science was very methodical and penetrating. In addition to his famous general circulation experiment he has written long papers where he investigates the foundation of the quasi-geostrophic theory in great detail. In the early days he took a great interests in the numerical procedures used in numerical weather prediction and in the models of the general circulation. His interests in numerical weather prediction continued to the end of his scientific career, and he left the department at M.I.T. and joined the research and development department of the National Meteorolgical Center in charge of the daily production of the numerical forecasts.

7. The project at the international institute

Carl-Gustaf Rossby had returned to Stockholm in the late 1940's to accept a professorship at the University of Stockholm, then called Stockholms Högskola, i.e. Stockholms School of Higher Learning. At an earlier time he was requested to advise the Swedish government on a proposal to create a professorship in dynamic or theoretical meteorology in Stockholm. Such a proposal had been made earlier in view of the recent developments in dynamic meteorology, a development to which Rossby had given major contributions. Another reason was the hope that the development of this branch of meteorology would eventually result in practical results of use to the weather forecasters. His advice to the Government was to establish the proposed position. He met with the Minister, Mr. Tage Erlander, who later became the prime minister of Sweden. Rossby summarized his recommendations, and the conversation turned to possible candidates for the professorship. The story is that Tage Erlander suddenly asked Rossby, if he would return to Sweden and accept the chair.

After his arrival in Stockholm he collected a few young students around him. The first two were Bert Bolin and Roy Berggren. As part of their training they took part in an essentially synoptic study of atmospheric wave trains leading eventually to the formation of a blocking situation over the eastern Atlantic and Scandinavia. Both of them got a good training in atmospheric dynamics and both of them came as visitors to the Princeton Project.

To be able to create an exciting environment Rossby created the International Meteorological Institute. The first advantage was that he could secure funds to bring a large number of visitors from other countries to his institute, and the second advantage was that he could secure financial support, essentially from the U.S.A., for the research activities of the Staff. Visitors included almost always a couple of U.S. Air Weather Service officers and a Navy officer from the Navy's meteorological department. In addition, he invited visitors from many countries including the other Scandinavian countries (Denmark, Finland, Iceland and Norway), but also many German meteorologists came to visit such as Hinkelmann, Wipperman and Hollman. Visitors from South Africa included A.P. Burger and later H. van den Boogaard. India and Mexi-



Bo R. Döös (1922-), the author (1924-), Friedrich (Fritz) Defant (1914-1990), Bert Bolin (1925-); (left to right)

co provided C. Ramaswamy and Julián Adem, respectively. In addition to the meteorological project there were sections dealing with convection and cloud physics, oceanography and atmospheric chemistry.

He created projects in dynamic and synoptic meteorology, numerical weather prediction and objective analysis. The physical oceanography was for a time concerned with sea-level predictions for the North Sea in collaboration with Professor Hansen from the University of Hamburg. The latter project brought Dr. Heinz Kreiss and Dr. Günther Fischer to Stockholm. Rossby embarked on an entirely new project on certain aspects of atmospheric chemistry, in the beginning mostly a chemical analysis of precipitation. It will thus be seen that the International Meteorological Institute (IMI) provided an exciting atmosphere for research and development. The author joined IMI in 1955, originally for a short visit of a few months on leave from the Danish Meteorological Institute, but the stay lasted until 1959 due to the fact that Rossby convinced me that I was cut out for research, and the project would provide a (not so large) salary.

The dynamic meteorology program became another pioneering effort in numerical weather prediction. While Roy Berggren took employment with the Swedish Meteorological and Hydrological Institute, Bert Bolin and the visitor, Arnt Eliassen from Norway, became the leaders of the group engaged in numerical weather prediction.

Also in Sweden the computer was unavailable when the project started. The first efforts were then, just as in Princeton, devoted to tendency calculations. The results can be found in the paper: Staff Members (1952). It may be considered as a continuation of the paper by Bolin and Charney (1951), see Chapter 6. The results were expressed as changes of 12 hours permitting a verification with the next observation time for 500 hPa. 14 cases were made, and while one cannot draw any definite conclusions from such calculations, it is evident that the participants were encouraged by the results of these nonlinear calculations. At the same time, they were looking forward to real integrations of the barotropic vorticity equation for a longer period using the step by step procedures. The tendency calculations were cumbersome. While the calculations of the advection term, expressed as a Jacobian, is straightforward, the solution of the final Poisson equation by an iterative procedure to provide the tendency in the geopotential field was time consuming and required a lot of energy, patience and ingenuity. The truly international team consisted of: G. Arnason, Iceland; B. Bolin, Sweden; Phil. Clapp, U.S.A.; Arnt Eliassen, Norway; Karl Hinkelmann, Germany; Ernest Hovmöller, Denmark; William Hubert, U.S.A.; E. Kleinschmidt Jr., Germany; Chester Newton, U.S.A.; Harriet Newton, U.S.A.; H. Schweitzer, Germany; and Charlotte Steyer, Germany. Another tendency calculation for a single case was carried out by S.J. Smebye (1953) using a two-level model designed by A. Eliassen (1952). The results indicated some improvement over the barotropic calculation for the same case.

Eventually, the Swedish computer BESK was ready for use. It was in many regards similar to the Princeton computer, and it was at its creation the fastest computer in the world. A new group, with some overlap with the old one, produced 24 hour forecasts in 24 cases. The results are found in Staff Members, Institute of Meteorology, University of Stockholm (1954). The averaged correlation coefficient between predicted and observed changes became 0.77, an improvement over the same coefficient for the 12 hour tendency calculations that was 0.69. Two of these forecasts (23 and 24 March, 1954) were made on an operational basis by Bo R. Döös and Art Bedient, and it is believed that they were the first forecasts produced so early that the results could be of operational value. This time the team was: G. Arnason, Iceland; H.Bedient U.S.A.; P. Bergthorsson, Iceland; B. Bolin, Sweden; G. Dahlquist, Sweden; B. Döös, Sweden and N. Phillips, U.S.A.

The testing of the barotropic forecasts continued with forecasts up to 72 hours. This forecast experiment was supported by the Weather Service of the Swedish Air Force. The team had now become truly Scandinavian and consi-

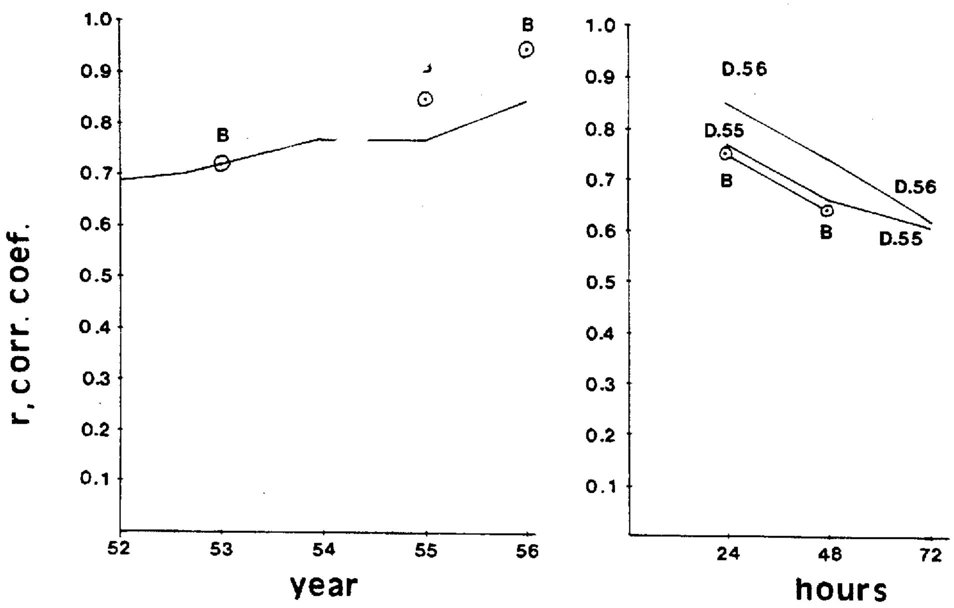


Fig. 7.1: The left-hand side shows the progress in the accuracy of 24 hour, barotropic forecasts as measured by the correlation coefficient between predicted and observed changes as a function of time. The curve applies to standard forecasts, while the points marked B are experimental forecasts carried out by Bolin. The right-hand side shows the decrease in accuracy of 1, 2 and 3 day forecasts. Results are taken from the Stockholm group.

sted of P. Bergthorsson, Iceland; B. Döös, Sweden; S. Fryklund, Sweden; O. Haug, Norway and R. Lindquist, Sweden. The results are published by Bergthorsson et al., (1955).

Bolin (1955) continued the work with the barotropic vorticity equation and extended the forecasts to 3 days (72 hours) for an enlarged forecast region. The larger size was necessary to reduce the errors coming from the artificial boundary conditions. His results were measured by the same correlation coefficient as above and the numbers were 0.85 for the 24 hour forecasts, 0.82 for 48 hours and 0.70 for 72 hours (see Figure 7.1). It is in the same year that the first papers on the balance equation appear. Two papers by Charney (1955) and Bolin (1955) are published next to each other. None of the papers contain examples of numerical solutions, but the latter paper contains a useful discussion of the various terms of the equation. A special point in connection with the balance equation is the following. Equations are normally classified in certain groups. For second order, differential equations in two dimensions one talks about elliptic, parabolic and hyperbolic equations.

The classification is important because the groups do not have solutions of the same type. The worst is if an equation is, say, elliptic in some regions and hyperbolic in others. In that case the normal procedures for the solution of the various types break down. It turns out that the balance equation applied to the 500 hPa height field belongs on occasion to this difficult mixed type in some cases and in certain regions, especially on the south side of a well developed jetstream. What can be done to obtain solutions in these cases? The solution applied at the time is of the type: If you cannot solve a problem, change it so that you can! In practice the height field was changed so that the equation became elliptical in all regions. If the required changes are of a minor nature and fall within the ever present uncertainty in a meteorological analysis, such a procudure may be justified.

During the forecast experiments in both the U.S.A. and in Sweden one had so far obtained the initial field from analyses drawn by hand in agreement with the available data. On such a map it was then necessary to cover the region of interest with a copy of the rectangular grid used in the calculations and to estimate the height in each gridpoint by interpolating by eye between the drawn isolines. This procedure is inaccurate, cumbersome and very time consuming, especially as the grids became larger and larger. The idea to let the computer do all the work arose first in the U.S.A. where Panofsky (1949) used a system of fitting a polynomial in two independent variables to the observations over a rather large area. Such a procedure will necessarily require a function of high degree since it has to be able to catch all the extrema of the height field, i.e. all the lows and the highs. The results were not too promising for a large region. A second attempt to solve the same problem was made by Gilchrist and Cressman (1954). They changed the strategy by fitting the data in a small region around a given gridpoint. Consequently, they could use polynomials of only the second degree. They obtained acceptable results over North America where the data network is good, but how could one apply such a method over a data-sparse region over the oceans? One could not get acceptable results in that case. The third attempt was made by Bergthorsson and Döös (1955), and they introduced a new idea.

What can one do in the sometimes vast areas between the observing stations over the oceans? We have of course the analysis made 12 hours ago, and maybe that could be used as the first guess. However, if a reasonable forecast was made for the 12 hours from the previous analysis, then that forecast would hopefully be a better guess than just the previous analysis. They adopted this strategy. It was assumed that the 12 hour prediction was a good first guess. The prediction was then modified in each point depending on the available data producing eventually the final analysis that could be stored in the machine ready to use. In the beginning the climatological mean map was also used to obtain an analysis that did not contain 'unmeteorological' values, but the importance attached to climatology was always small except close to the boundaries, where the predicted gridpoint values were unrealistic due to the boundary conditions used during the prediction computations. This general strategy was adopted elsewhere in the world to produce the starting fields for numerical forecasts, although various small variations on the same theme were introduced.

The replacement of the hand-drawn analyses by the objective, numerical analyses is one of many cases in which the operational meteorologists realized that some of their standard work on the shifts was going to be done by computers if the new analyses turned out to be accurate. In situations like this there are always two kinds of responses. One is that some of the operational meteorologists appreciate that some of the time-consuming, rather dull work with the maps disappears, and they get more time for more important work. The other point of view, held particularly by the meteorologists with strong interests in synoptic meteorology, was firstly that the very process of producing the analysis by hand was an excellent way of getting into the weather situation and appreciating the changes going on, and secondly that the objective analysis was unable to »draw« the isolines in total agreement with the observations, and finally that the objective analysis was unable to produce the sharp gradients connected with the jetstreams. These points of view were discussed at length and in a heated manner in Stockholm between the meteorologists in the weather service and those responsible for the numerical analysis and the experimental forecasts. Today it is the exception rather than the rule that maps are analysed by hand. On the other hand, the weather forecasters have got another problem. The forecasts produced by various institutions are all available to them. These forecasts are not necessarily in agreement with each other. The forecasters on duty will therefore have to decide which one of the possible developments they have faith in. These matters will be discussed again in a later chapter.

Another effort of making objective, but not numerical forecasts in Scandinavia was going on in Copenhagen where Ragnar Fjørtoft had been appointed as professor of theoretical meteorology after his visit to Princeton. He invented another way of making objective forecasts while the community was waiting for the computers. As a matter of fact he followed the old suggestion by V. Bjerknes in 1904 to solve a nonlinear equation by graphical methods although his procedure was applied to the barotropic vorticity equation only and not to the full set of equations.

The detailed procedure will not be described here. Suffice it to say that he had invented graphical procedures for all the steps necessary to produce a solution to the barotropic vorticity equation including the approximate solution of a simple second order partial differential equation. A person experienced in graphical additions and subtractions could complete a forecast in about 2 to 3 hours.

I got involved in this project when I turned to meteorology after having finished my M.Sc., majoring in mathematics. The year was 1952. The employment was a half time appointment as assistant to Fjørtoft while the other half was in the weather service. The procedures required extensive use of transparent paper of large dimensions for copies of the initial maps. The copies were used to graphically calculate the necessary quantities in the equation. Many forecasts were made by Hans S. Buch and myself under the guidance of Fjørtoft. The results of the efforts were never published because they formed a very inhomogenous sample. Almost every day Fjørtoft got new ideas for improvements in the procedures. On the other hand, it was an excellent project for the young assistants because through our work and through his lectures we got a good eduation in dynamic meteorology.

The project lasted for a couple of years. In 1955 Fjørtoft decided to return to his native Norway where he became Director of the Norwegien Meteorological Institute. Helped by an recommendation given to Rossby by Fjørtoft I was offered to be a visitor at the International Meteorological Institute where I could continue my studies and get further experience. When I came to Stockholm in September, 1955 all the work described so far in this chapter had been completed. During the first semester I took courses in dynamic meteorology and numerical weather prediction from Bo Döös and the general circulation of the atmosphere from Bert Bolin. At the same time I took a first course in computer programming for BESK.

During the first semester in 1955 Rossby asked several of the young visitors to participate in yet another forecast experiment. Once again he had convinced the Swedish Military Air Weather Service to support the experiment which this time would be really operational. At the same time it was to test whether or not the objective analysis was good enough to avoid subjective analyses, which would have to be used if the system broke down.

The team had Bo Döös as its leader. The participants were Rolf Lindquist, Sweden; Hlynur Siggtryggsson, Iceland; Bengt Söderberg, Sweden; Hessam Taba, Iran; Aimo Vaisänen, Finland; and the author. On average each of us was in charge of the production of analysis and forecast using BESK one time per week. The work started in the late afternoon at the Air Weather Service. At this time a paper tape containing all the observations for 1500 GMT had been punched by a young assistant. It was the input data for the analysis. We had also prepared another paper tape containing the first guess field, i.e. the 24 hour forecast from the day before. In the evening we went to the computer and started the analysis and the forecasts. If everything went well without any interruptions we could finish shortly after midnight, but that was seldom the case because the BESK was not the most reliable computer in the world. An engineer was always present, and his job was to get the computer restarted if it broke down. The minimum requirement was to get the analysis and the forecast for the first 24 hours because that secured the continuation for the next day. As the days went by it became more and more difficult to reach this goal because large unnatural flow systems were created by the boundary errors, especially in the meteorologically very active region in the Gulf of Alaska, which was close to one of the corners in our rectangular region. However, in each case we reached the minimum goal.

Döös (1956) summarized the results in a brief note. The correlation coefficient between predicted and observed changes in 24 hours increased from 0.69 in 1952 to 0.85 for the series described here. The 48 hour forecasts gave 0.74 and the 72 hour forecasts 0.62.

I stayed in Stockholm until the end of 1958 when I joined the organization to be described in the next chapter. During 1958 I had completed the design and the programming of a two-level baroclinic model for which the same objective analysis system could be used. While the basic model was of a standard nature a special treatment had been introduced to improve the prediction of the very long waves. It was tested by a group of operational meteorologists from the military Air Weather Service participating in a course of supplementary education. We ran the model for about 2 weeks and had every day a discussion of the forecasts that should be valid on that day. The model was never published, put together as it was of standard elements but it served for a time as the prediction model for the Swedish Meteorological and Hydrological Institute where it later was replaced by a model with three information levels.

It will be seen that the efforts in Sweden resulted in operational forecasts by 1955 with the barotropic and in 1959 with a baroclinic model. In addition, the quasi-nondivergent model was introduced at about the same time in Stockholm and in Princeton, and the objective analysis problem was solved in a very practical way.

Rossby did not take any personal interest in the meteorological and numerical aspects of the efforts in NWP, but he nevertheless gave a lot of inspiration to the young people gathered around him. His personal interests had turned away from dynamic meteorology to atmospheric chemistry. But also in the latter field he found an application for the numerical forecasts. The chemical analyses of precipitation carried out at the Institute showed that precipitation contained chemicals which could not originate in Sweden. To determine the origin he needed computations of air parcel trajectories backward in time indicating the origin of the air in which the precipitation was formed.

He gave this problem to Dusan Djuric, a visitor from Yugoslavia, and the author. Rossby had a way of convincing his co-workers of the importance of the task which he requested. We looked at some of the procedures for the construction of trajectories in the meteorological textbooks, but none of these methods were well suited to numerical methods. Inspired to a large degree by Pierre Welander, who had demonstrated the severe deformation fields in the atmosphere, we found a Lagrangian technique well suited for incorporation in the barotropic predictions as an extra subprogram. The main idea is that in addition to the square grid defining the points in which the computations are carried out, we defined a grid which at the initial time coincided with the computational grid, but which moved with the winds in the horizontal plane. Such a grid will be severely reshaped, but the advantage was that we could easily make hundreds of trajectories for each original gridpoint. Our calculation of trajectories were used in a later modification of the integration method itself, the so-called Lagrangian and semi-Lagrangian methods.

As it turned out I became the very last licentiate (Ph.D.) student studying under Rossby. The dissertation was delivered a few days before he died in his office of a heart attack. He was a very charming person, and when he after a while had proposed that one should drop the formalities, such as always addressing him as 'Professor', the relationship became a lot easier. He had a reputation for not awarding any higher academic degrees to a student before the examination became a formality, when he had decided in advance that the student had reached the required level. In any case, he was never terribly impressed by academic degrees, but looked always for significant papers published by the researcher. He would tell a group of graduate students at the afternoon coffee that fortunately there were students in the group who would not need a Ph.D., but unfortunately also some who would never manage without it. When he congratulated a new Ph.D., he would add that in the best of cases the degree would help to open the door to the first job, but thereafter one would again have to live on ideas and contributions.

Bolin was Rossby's favorite co-worker. He put him in charge of the projects in Numerical Weather Prediction. As described above Bolin gave good contributions based on the barotropic model in several forms and on the use of the balance equation. When Bolin became Rossby's successor in the professorship in meteorology in Stockholm, he decided to change his field of interest to other areas of the atmospheric sciences.

Döös moved to the Swedish Meteorological and Hydrological Institute for some years, but his later career took place in international programs such as the Global Atmospheric Research Program (GARP) where he served at the World Meteorological Organization (WMO) as head of the GARP office sponsored jointly by WMO and the International Council of Scientific Unions (ICSU). Still later in life he worked as Deputy Director for the International Agency for Applied Systems Analysis (IAASA) located close to Vienna.

8. From experiments to operational predictions

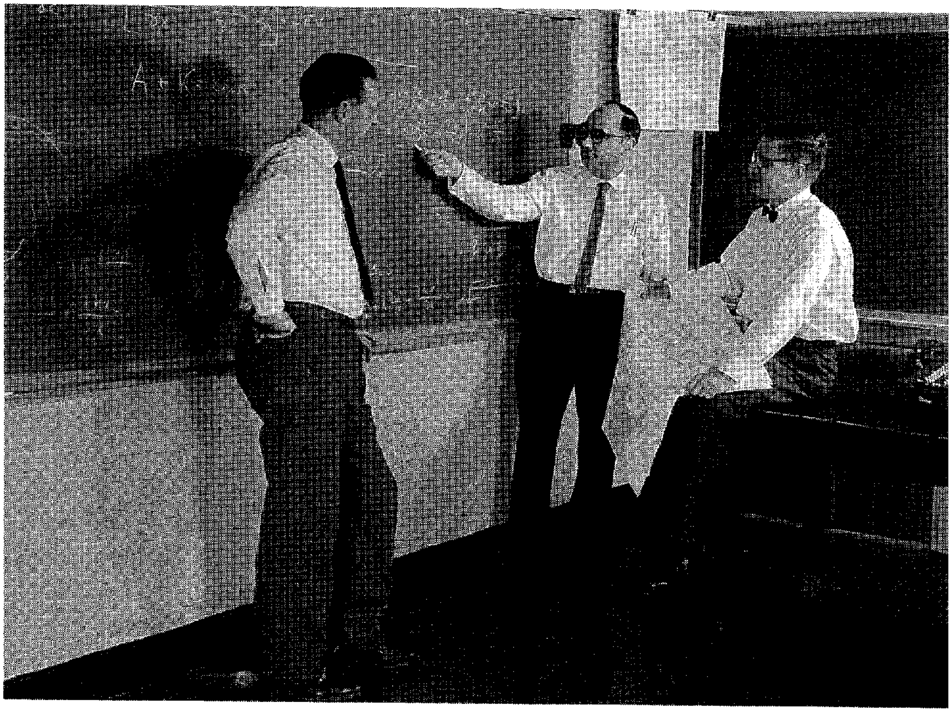
As indicated in the chapter on the meteorology project at the Institute for Advanced Studies, Charney and his various co-workers had succeeded in barotropic as well as baroclinic forecasts of which particularly the best forecast for the Thanksgiving Day storm of 1950 had impressed the U.S. Weather Bureau. It would be natural for the U.S. Weather Bureau to create a project with the goal of introducing numerical forecasts in the operational routines.

The negotiations resulted in the very reasonable decision that the three weather services (the U.S. Weather Bureau, the Air Weather Service of the U.S. Air Force and the Navy Meteorological Section) jointly would support a unit containing three sections: an operational section, a research and development section and a computation section. This decision lead to the formation of the Joint Numerical Weather Prediction unit, normally called JNWP. It was established in Suitland, Maryland in Federal Office Building No.4 in 1954 with George Cressman as director, Joseph Smagorinsky as head of the operational section, Philip Thompson as head of the development section and Art Bedient as head of the computer section.

Cressman, Thompson and Smagorinsky were very familiar with the Princeton Project having stayed with the project for various lengths of time. Bedient had been assigned to the Institute in Stockholm, so they were all well prepared.

Not surprisingly, they started with the barotropic model. The region for the forecasts was prepared as an octagon on a map with the North Pole in the center. The projection was from the South Pole onto a plane cutting the Northern Hemisphere at a constant latitude, a polar stereographic projection. The octagon on the map did not reach the equator because the tropical observations were not plentiful, but more importantly, it was assumed that the barotropic model did not apply in this region. The basic gridsize in the square grid was 381 km, sometimes called 'One Bedient'. These forecasts were the first in which it was possible to have a representation of the whole spectrum of waves in the zonal direction, including the very long waves.

One should perhaps have anticipated that the waves on the very largest scale would result in large errors since the Rossby formula predicts that these waves will move with a large speed from east to west, and because the first forecast model did not contain any forcing and dissipation. A generalisation of the Rossby wavespeed formula to the spherical domain carried out by



The author, Frederick G. Shuman (1919-), George P. Cressman (1919-); left to right

Haurwitz shows that the contribution from the rotation of the Earth, the so-called beta-effect, in itself will result in an east to west velocity of 360 degrees of longitude per day corresponding to one time around the Earth in one day for the very longest wave (wave number 1), while wave number 2 will move with a speed of 120 degrees of longitude per day. It was indeed found that wavespeeds of this order of magnitude were found in the forecasts although the westerly winds normally present in the middle latitudes would decrease the wavespeed by a minor amount as a counteracting effect.

On the other hand, the real very long waves consist of a stationary part and a transient part. The stationary part is supposedly a result of the forcing due to the heating of the atmosphere and to the influence of the large mountains on the surface of the Earth, and these two effects were not included in the most simple barotropic model. The effects of the mountains are rather easily incorporated in the barotropic model, but are not in themselves sufficient to remove the errors in the forecasts.

The first remedy for the large errors on the large scale was invented by Wolff (1958). His method is really a stop-gap procedure because he first determined the amplitudes and phases of the three longest waves in the initial field. The initial long waves were then periodically inserted in the forecasts replacing the same long waves as predicted in the forecasts. This procedure is essentially equivalent to keeping the three longest waves stationary during the forecast. Since a relatively large fraction of the longest waves are stationary, he obtained an improvement in the scores.

Later Cressman (1958) replaced this method with a modified barotropic model which was formally based on an early analysis of the wavespeeds in a homogeneous fluid with a free surface conducted originally by Rossby and used later by Charney and Phillips. Contrary to the pure barotropic model the free surface model has divergence and convergence closely related to the changes in the depth of the fluid on the local level and thus to vertical velocities. Furthermore, it turns out that the dynamical effect of the divergence has an influence on all waves, but the effect is particularly large on the longest waves which are slowed down in the rapid movement from east to west. Formally speaking, the change amounts to the replacement of a Poisson equation with a Helmholtz equation when the changes in the streamfunction are obtained from the already computed right hand side of the equation. This is actually an advantage because the latter equation is solved by the same iterative procedure as the first, but the convergence of the iterative procedure used to solve the equations is faster for the Helmholtz equation.

One may also say that a new parameter, measuring the intensity of the divergence, has been introduced in the model used by the JNWP. The question is: What is the numerical value of the parameter? Cressman determined this value empirically by running the same forecast for various values of the parameter and selecting the value that gave the minimum error in the forecast. The same problem has been considered by Bolin (1956) who used a model with a stratosphere and a troposphere in such a way that the motion in the stratospheric layer was neglected, and by the author (Wiin-Nielsen, 1959 and 1991). In the latter paper a procedure has been given to compute the value of the parameter in advance. It is in good agreement with the empirical value determined by Cressman (1958).

The first baroclinic model was the so-called thermotropic model designed by Thompson (1953). The experiences at JNWP were the same as elsewhere,namely that the two-level models did not give better results at 500 hPa than the modified barotropic model. An intermediate model used for a time consisted of making barotropic forecasts at 500 hPa and using the streamfunction so predicted in the thermal equation to obtain forecasts for the thermal field which in turn could be used to obtain forecasts at other levels. One neglected in other words the baroclinic effects at 500 hPa, or the feedback mechanism from the thermal field on the 500 hPa field.

As implied by the discussion above one had, like everybody else, replaced the quasi-geostrophic formulation with the quasi-nondivergent one. In addition, Cressman incorporated all the experiences in a three-level quasi-nondivergent model which served with good results for a long time, awaiting only the introduction of forecasts based on the primitive equations, which will be treated later.

The author joined JNWP at the very beginning of 1959 after it had existed for more than four years. The reason for this assignment was that Philip D. Thompson as an Air Force officer from time to time had to have an assignment outside the U.S.A. He was promised a stay at the International Meteorological Institute in Stockholm if someone from there would come to JNWP. It seems that I was the only one available since Bo Döös in 1958 was in the U.S.A for a year-long stay at Florida State University, but also with a trip to the meteorological department at U.C.L.A. I arrived at a time when most of the developments described above had taken place except that Cressman was still in the design phase of his three-level model. Cressman was in my opinion an excellent head of JNWP. He stayed in close contact with the members of the development section and discussed the various projects with us. I have never been asked by him to do specific projects, but we told him what we were engaged in and as long as it was relevant work he did not try to influence our choices. Norman A. Phillips was at M.I.T., but acting as a consultant to JNWP from time to time. He helped me a great deal to get started on some projects and invited me to visit the department at M.I.T. during some weeks in the first summer. We stayed in a cottage at Marblehead on the coast, and I drove to the meteorological department every day with the synoptician, Professor Fred Sanders. During the visit Phillips got me started on an investigation of the structure of the very long waves. The project was finished at JNWP.

My assignment was in the development section headed by Dr. Fred Shuman who was very interested in the numerical procedures employed in the models. Although he was the immediate boss, he hardly ever spoke to us about any progress on our projects in the first couple of years, supposedly because he was so deeply involved in his own work. His own speciality was finite differences, i.e. the way in which one approximates the differentials in a model. He developed this particular field in many forms combining differences with averages and in one particular version he needed at least four indices to indicate the procedure.

For the first many months I shared an office with John A. Brown, Jr. He was a captain in the Air Weather Service at the time and had recently completed his master's degree in meteorology at M.I.T. He came from Louisiana and spoke the language of the deep south. In the beginning I had great difficulties in understanding him, but after a few months it became easier. We had a common project where we designed a scheme to calculate the atmospheric heat sources directly from the analyses on a daily basis. We felt that this was important because the prediction models did not yet contain forcing by the heat sources and dissipation due to friction, and our calculations provided an estimate of the total heating and cooling. At a later time John went to Boulder, Colorado where he was employed by the National Center for Atmospheric Research. At the same time he was a Ph.D. student at the University of Colorado where he wrote an excellent thesis on a version of the baroclinic stability problem with Philip D. Thompson as the real advisor. We continued to have joint projects for some years concentrating on diagnostic calculations of the various atmospheric energy generations and conversions. In this work we cooperated also a great deal with Miss Margaret Drake who was a expert in handling the very large data resources necessary for such calculations. Margaret came from Massachusetts, was a devoted Catholic and had acted as teacher for the Indian children in New Mexico on the reservations. After his Ph.D. degree John Brown returned to the National Meteorological Center, the successor to JNWP, where he became head of the development section. Unfortunately, both John and Margaret passed away at a relatively young age, both of them from heart trouble.

The Princeton and Stockholm projects were certainly the major efforts in the early stages of numerical weather prediction, but other institutions gave contributions as well. Fjørtoft's graphical methods tested in Denmark and Norway played a role in the early 1950's.

The German weather service arranged a symposium on numerical weather prediction in 1957, and the proceedings from the symposium give a good impression of the work carried out by various groups working on special problems. In addition to descriptions of the operational forecasts produced by JNWP (Thompson) and the Swedish Military Weather Service (Herrlin) we find reports on objective weather map analysis by Döös, testing of the barotropic and thermotropic models by Gates, testing of the Sawyer-Bushby model by Knighting in the United Kingdom, testing of the graphical procedures by Haug from Norway and Brandejs from Czechoslovakia. Gambo from Japan presented a case study of the forecasts of a cyclogenesis in the Far East and the associated precipitation. The replacement of the quasi-nondivergent model was discussed. Several contributions discussed the importance of nongeostrophic motion such as the lectures by Thompson, Berkovsky, Hinkelmann, Hollman and Edelmann. These papers can be considered as the first practical steps in returning to the use of the Richardson equations, also called the primitive equations. Furthermore, physical processes had started to enter the operational forecast models as indicated by reports by Smagorinsky on the introduction of moist-adiabatic processes and a report from Wippermann on the inclusion of orography. We may thus say that the research on

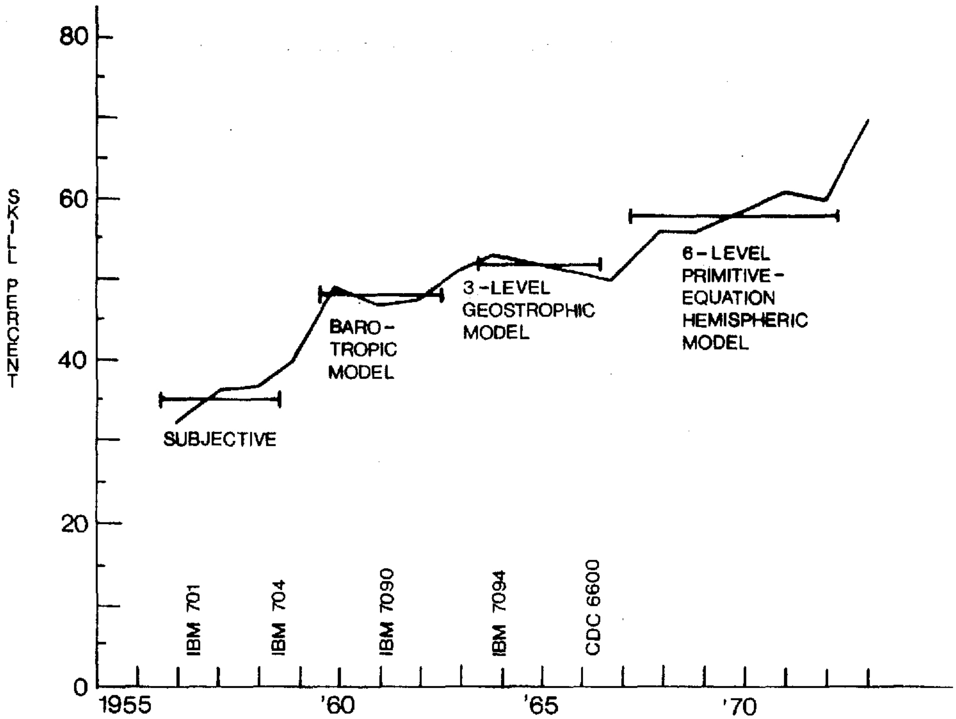


Fig. 8.1: The increase in accurary of forecasts made by various models at the Joint Numerical Weather Prediction Group as a function of time. The names along the time coordinate are the names of the various computers used for the forecasts.

numerical weather prediction and the production of operational forecasts were spreading around the world because of the practical applications of the numerical weather prediction models. In Europe we notice efforts in Sweden, Norway, United Kingdom, West Germany and Czechoslovakia, and it would not be long before other countries converted to numerical weather prediction.

But back to the JNWP. It became clear that one had to try to return to the primitive equations. It had at that time been shown by Joseph Smagorinsky and his group working on a simulation of the atmospheric general circulation that the primitive equations could be used for this purpose. It is necessary to understand the difference between this problem and the prediction problem. In the general circulation problem one may start from a state of rest in the atmosphere, turn on the heating of the atmosphere and gradually develop the full circulation of the atmosphere. In other words, one does not have a serious problem with the initial condition in a simulation of the atmospheric general circulation. In weather predictions one does have such a problem because a prediction has to start from the initial conditions given by the meteorological data at a particular time. As suspected already by Richardson the uncertainty of his initial conditions, which were highly artificial due to the lack of upper air observations, could be the reason for the failure of his forecast.

One was therefore in need of a starting procedure and an accurate numerical scheme for the integrations of the same type of equations used earlier by L.F.Richardson, i.e. a return to the original Navier-Stokes equation containing, however, the hydrostatic approximation. This set of equations is called the primitive equations. It was thus necessary once again to face the problems connected with the fact that the primitive equations have several type of wave solutions in addition to the meteorological waves for which a forecast is desired. During the integration the non-meteorological waves would be present in principle. How could one prevent them from destroying the forecast?

In the early phases of the work on this problem, Fred Shuman at JNWP worked mostly on the numerical aspects of the integration procedures. He developed rather complicated schemes, all based on finite differences, and he mixed finite differences and averaging procedures to prevent the gravity and other fast waves from disturbing the development of the important slowly moving meteorological waves. He was able to keep the integrations stable for integrations over a few days, but he did not consider the initial state problem in detail. The use of the primitive equations for prediction purposes was studied by a number of different research groups during the later part of the 1950's and the 1960's. The integration of these equations will be the subject of the next chapter.

Figure (8.1) shows the increasing skills obtained at JNWP from 1956 to 1973 with various models and computers. The measure, called the skill score, is 100% for a perfect forecast. The figure indicates the improvement found with the barotropic model over the earlier subjective forecasts as well as the increase created by better vertical resolution as the number of levels increased. Further increases are found with the primitive equations.

9. Back to Richardson's general approach

The quasi-geostrophic (or quasi-nondivergent) system developed by Charney in the late 1940's and tested extensively by a number of research groups in the 1950's had not lived up to the expectations of being really superior to the barotropic forecasts when compared at 500 hPa. These results were disappointing, but the models could certainly be used and were used for years in various weather services. The three-level version as developed first by Charney and later put in operational form by Cressman and used by JNWP was slightly better than the barotropic forecasts at 500 hPa, but in addition it gave a useful forecast for 1 to 2 days for the lower and the upper levels. It became, however, clear that it was desirable to use less restrictive assumptions in the models, and that meant a return to Richardson's approach using his basic form. For this to be accomplished it was necessary to understand in detail why Richardson's single tendency calculation gave such unrealistic results.

By the work of many researchers the picture gradually became more clear. It was realized that a major difference between the meteorological waves, i.e. the disturbances in the atmosphere connected with the weather, and all the other possible wave types was found in the amount of divergence connected with the waves. The synoptic scale meteorological waves are characterized by small divergences compared to the divergences connected with the other wave types that do not influence the weather to any appreciable degree. The second group of waves are therefore only wanted in the forecasts if they influence the development of the meteorological waves. In the former more simplified models these waves had been totally excluded. They now had to be reintroduced for interaction purposes, but not in such a way that they had a unwanted influence on the meteorological waves.

The hydrostatic assumption already introduced by Richardson was retained, partly because it is the very best assumption compared to all the others, but mainly because it is introduced in the upper air meteorological observations when the observed pressures and temperatures are used for the calculation of the height of the isobaric surfaces. The initial state is therefore in hydrostatic equilibrium, and the computed states will also be in hydrostatic equilibrium when the assumption is incorporated in the prediction equations.

An early and very important contribution by Hinkelmann (1951) describes an analysis of the behavior of a homogeneous fluid with a free surface in which he finds the two wave types: the Rossby wave and the external gravity waves. The gravity waves have wavespeeds that are much larger than the speed of the Rossby waves. After a detailed calculation of the nature of these waves and their development in the general case, he proceeds to consider how one may avoid an unwanted influence of the gravity waves by starting from a *geostrophically balanced* state. Although this investigation considers only the most simple case where the only space variable is the x-direction, the investigation nevertheless gives a procedure which can be used also in the most general case.

With respect to the windfield, a radical assumption would be to assume that the initial divergence and its rate of change would be zero. A less radical approach would be to use a quasi-nondivergent model initially and at that time only. From the balance equation one would obtain the streamfunction which in turn is used to solve for the vertical velocity from the so-called omega-equation i.e. an equation formulated in such a way that it is in agreement with the quasi-nondivergent, hydrostatic model. Finally, an estimate of the divergent part of the wind may be obtained from the continuity equation. All this is very technical, but the main point is that the earlier prediction models are restricted to the initial time only. Thereby one obtains an initial state that is characteristic for the large-scale quasi-geostrophic motion and not for all the other waves.

Karl Hinkelmann and his group in the research and development department of the German weather service showed already in the late 1950's (Hinkelmann, 1959) that it was possible to integrate the primitive equations for up to three days with good results. They used a somewhat idealized model in which the initial state was relatively simple, defined mathematically and not based on observations. It consisted of a single wave superimposed on a zonal current. No noisy data was therefore part of this integration. They made two different integrations. The first started from an initial state with no divergence and no vertical velocity. The second initial state had the same nondivergent field as the first, but in addition they computed the vertical velocity from the proper equation as discussed above and obtained finally the divergent part of the wind, all obtained from the quasi-nondivergent model.

The model had no heating, no friction and no mountains. It was in other words free of external influences and therefore limited to short-range integrations of, say, 3 days, which is small compared to the dissipation time estimated to be about 10 days. The horizontal windfields and temperatures were predicted at 100, 300, 500, 700 and 900 hPa, while the vertical velocity and the geopotential were represented at the levels: 0, 200, 400, 600, 800 and 1000 hPa. Simple boundary conditions were applied at the bottom and the top of the atmosphere and at the southern and northern edges of the region, while the model was periodic in the west-east direction.

The results of the two experiments, particularly the second, showed a very strong development of a low and a high, and they studied especially the effects of the role of the divergent windfield in the advection processes and found that this windfield is especially important in the maturing of the initial disturbance, the so-called occlusion process. Another important conclusion from the experiments was that non-meteorological waves, in their case the internal gravity waves, remained with a small amplitude during the whole integration. These waves have of course a zero amplitude initially, but since the primitive equations are used, gravity waves may form during the integration.

They do so, but remain at amplitudes so small that they do not influence the forecast to any appreciable extent. Finally, the main conclusion is that the forecast based on the better initialisation using the full quasi-nondivergent model to estimate also the vertical velocities and the divergent part of the windfield initially performs better than the forecast containing no divergence and no vertical velocities initially. They are therefore justified in concluding that »the primitive equations too can serve as useful tools for predicting large-scale weather developments.«

The Hinkelmann experiments do not provide solutions to all the problems in the use of the primitive equations although they represent a major step forward. The major limitations are:

1. The model is used in a channel flow with a constant value of the Coriolis parameter, i.e. no beta effect.
2. The channel is restricted to the middle and high latitudes thereby avoiding the tropical problem where the atmosphere is less quasi-geostrophic.
3. The lower boundary condition is especially simple removing not only the vertical component of the sound waves, but also external gravity waves.
4. The mathematically defined initial fields avoid the cumbersome treatment of the noisy real data.
5. No energy sources and sinks are included.

It should be noted that Hinkelmann first defines the streamfunction and then uses the balance equation to obtain the geopotential. In practice, the situation is reversed. It is the geopotential field that is known from the observations, and it is the streamfunction that has to be computed. As we have mentioned earlier Hinkelmann's formulation will always work from a mathematical, numerical point of view, while the opposite problem does not always result in a solution because the criterion for ellipticity fails to be satisfied in certain regions, especially on the south side of strong jet streams. Furthermore, his procedure assumes that the important fields are the streamfunction and the geopotential, and that the winds to a very good degree of accuracy may be computed from the streamfunction. While such assumptions are justified to a good degree of accuracy in the middle and high latitudes, the same assumptions are generally not satisfied in the regions close to the equator.

In the tropical regions the windfield is much more important than the geopotential field. If we look at the tropics only, say a channel with the equator in the center and reaching to, say, 30 degrees south and 30 degrees north, one would in practice want to have good windfields from the observations. From the winds one would compute the vorticity and from that quantity one would obtain the streamfunction which in turn gives the nondivergent wind. Having the streamfunction one may obtain the geopotential from the balance equation.

When we subtract the nondivergent wind from the total wind, we obtain the divergent part of the wind which may be used to calculate the divergence itself. From the field representation of divergence one may then obtain the velocity potential. These practical problems are not part of Hinkelmann's procedure, but it would have to be faced by researchers wanting to formulate global models.

Following Hinkelmann and his group, many other researchers were very interested in the primitive equations and the numerical integrations of this set. The group continued its work to remove some of the special assumptions in the very first experiments. In additiom, they started to use real data. In those days as now it was a problem to find suitable computers that could handle the integrations as fast as possible. The original integrations were carried out in Paris, while some of the later integrations with a more general formulation were performed at JNWP because this organisation had obtained a new and faster machine at that time. They came to JNWP during my employment there, and we had a good opportunity to learn about their ideas for the future models. The group was often called »Die Männer«, the German word for »The Men«, because the most outstanding scientists in the group were Hinkelmann, Hollmann, Wippermann and Edelmann. Around 1970 I visited Germany and the German Meteorological Institute in Offenbach. The group was no longer in existence. Hinkelmann was a professor at the University of Mainz, Wippermann a professor at the Technical University in Darmstadt, while Edelmann was employed by the German Weather Service.

It is also worthwhile to note that the original Richardson model was integrated by Kasahara and his co-workers at the National Center for Atmospheric Research in Boulder, Colorado, and their work shows that Richardson's approach was completely sound as long as the initial conditions were well treated.

As mentioned before the primitive equations were first used for simulation of the atmospheric general circulation by Smagorinsky (1963) and his group. If the people engaged in numerical forecasting were to extend their forecasts as far into the future as possible, it became clear that the models had to include both energy sources and sinks in a way at least as complicated as the general circulation models. Naturally, the reason for this is that the models should be able to forecast the whole life cycle of the atmospheric disturbances from its birth over the growing phase to the phase of dissipation. Although the two kinds of models originally were quite different they became more and more alike. Today one can hardly discover any basic differences except for the fact that the prediction models have to be equipped with the various programs designed to produce a suitable initial state for the forecast.

The initialization problem was very difficult to handle. It took years to obtain a satisfactory solution. The integration of the primitive equations was attempted in several quarters already in the late 1950's. To understand the problem we can imagine a start from an unadjusted initial state. The raw analysis based on the observations is in a statistical sense an optimal initial state. However, if it is applied directly as the initial state for a forecast one will observe spurious oscillations in the forecast model because the fast waves will dominate. To avoid this behavior it is necessary to modify the initial analysis so that the fast waves do not appear. It is this process that is called initialization.

The initialization will produce changes in the analysed fields. In a statistical sense we are then degrading the analysis, but it is necessary to reduce the spurious oscillations and at the same time change the analysed field as little as possible. Using a mathematical technique one separates the slow and the fast waves. The fast waves are then modified in such a way that they initially have no divergence and no tendency to develop divergences. This scheme, proposed originally by Machenhauer (1977), has been used widely and is called the normal-mode initialization.

Another scheme is more direct. The idea is to integrate the model adiabatically backward for half the time span under consideration. A forward forecast is then made containing the diabatic forcing for the total time span in such a way that the diabatic forecast is centered on the initial time. This forecast will contain fast oscillations which are then removed using a so-called digital filter designed to get rid of the high frequency oscillations and leave the low frequency oscillations essentially untouched. The filter can be designed in a number of ways (see Huang and Lynch, 1993), but recently Lynch (1996) rediscovered a special filter, originally designed by Dolph (1946) in connection with some antenna problems, and this filter will most likely make the initialization procedures simpler. A comparison of the two techniques can be found in a paper by Huang et al. (1994).

As will be seen from the above very brief descriptions the initialization procedures have made it possible to integrate the primitive equations for prediction purposes in such a way that the only major physical assumption in the models is the hydrostatic relation. Numerical Weather Prediction has come a long way since the first integrations of the barotropic vorticity equation less than half a century ago.

10. Limited predictability

Weather predictions have always been wanted for durations as long as possible. In the following we are going to limit the discussion to forecasts trying

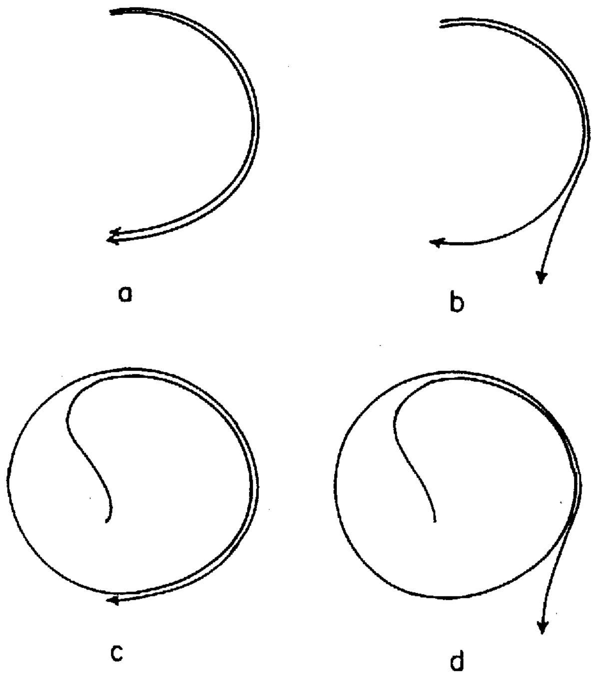


Fig. 10.1: A schematic diagram indicating the various possibilities for the behavior of forecasts starting from almost identical initial states. (a) shows unlimited predictability and (b) limited predictability. (c) indicates the behavior of a single forecast for a situation in which a stable limit cycle exists, while (d) shows what happens if the limit cycle is unstable.

to predict the individual atmospheric systems. Forecasts for a mean state and deviations from it will be treated in Chapter 12.

The operational meteorologists, the weather forecasters, have always known that the weather was predictable in a limited sense only. It was, however, difficult to convert this knowledge into numbers before the advent of numerical weather prediction. The first forecast models such as the barotropic and simple baroclinic models could easily be compared with what really happened, and using these models one could say that the forecasts were limited to 1 to 2 days or in special cases perhaps 3 days. The very limited capabilities of the models were generally ascribed to their simplicity, and the model designers were optimistic that the next model would permit forecasts for longer periods.

Let us first consider the general possibilities for predictability. Figure (10.1) shows in a schematic way a number of logical arrangements for what could happen if we were to start two forecasts very close to eachother. In Figure (a) we are in the best of all possible worlds. The two forecasts remain very close to eachother. A small error in the initial state is rather unimportant in this case because the small initial error remains small throughout the forecasts. On the other hand, in Figure (b) we have illustrated what really happens. While the two forecasts remain close to eachother for a while they start rather suddenly to deviate more and more from eachother. The reason for this behavior is that the atmospheric equations belong to the class of nonlinear equations that are sensitive to small changes in the initial state of the atmosphere. In Figures (c) and (d) another couple of cases are illustrated. Note that these figures contain only one forecast. Figure (c) aims to illustrate that if we have a periodic behavior one could imagine that once the system has arrived in the periodic cycle it would continue to move around in almost the same path. We call that behavior a stable limit cycle. Figure (d) contains also a limit cycle, but after a while the path goes away from the limit cycle. This case is called the unstable limit cycle.

It turns out that the atmospheric behavior is similar to the cases (b) and (d) and far from the two possiblities illustrated in (a) and (c). The implication is that the atmosphere suffers from limited predictability. How did we come to this conclusion?

In 1957 Philip D. Thompson pointed to the role of the accuracy of the initial state, i.e. the analysis, as a factor in the limited predictability of the atmosphere. His interest was not only the extent to which the atmosphere was predictable in practice, but rather the answer to the more general question: How far into the future is it possible to predict the state of the atmosphere if we have a theoretically complete knowledge of the physical laws that govern it? Or put another way, he was interested in knowing if the forecast limit was determined by practical and/or economic incapacity to observe and predict, or whether it was due to some irreducible minimum of indeterminacy that lies beyond human limitation.

One can immediately understand the importance of the question. Already the first experiences with numerical forecasts showed that often one could trace a major error in a forecast to a data-sparse region. Thompson investigated from a theoretical point of view the error growth in barotropic and simple baroclinic models, and he managed to find formulas that could be used to make realistic estimates. He also showed that the error growth depends not only on the error in the initial state, but also on the scale difference between the initial wind error field and the scale of fluctuations in the true initial windfield, the averaged magnitude of the vertical wind shear, i.e. the baroclinicity, determining the growth rate of disturbances, the averaged static stability, and a measure of the wind variation at 500 hPa with respect to the averaged wind at that level. It will thus be seen that while we may think of the predictability in an averaged sense there will be differences in the predictability from different initial states, a fact that has been verified by all later experiences to be described later.

With the advantage of hindsight one would certainly expect that these factors would enter in an error growth estimate, but the power of Thompson's analysis lies in the quantitative estimate that became possible from his analysis. It is seen from the analysis that there are differences in the predictability depending on the magnitude of the important factors appearing in the final formulas. He also applies his mathematical technique to prediction of the zonally averaged flow and arrives at the result that the zonal average is more predictable than the total flow.

One of the important conclusions from this excellent work is that it will pay to increase the density of the observations and the accuracy of each individual observation. On the other hand, it is realized that it will never be possible to observe the atmosphere in such a way that the uncertainty in the initial state will disappear completely. This is explained by the fact that most of the processes driving the atmosphere occur on a molecular scale, and it is of course impossible to observe the physical state of each of the molecules. We shall therefore have to live with the limited predictability forever since the uncertainty in the initial state cannot be reduced to zero.

An estimate of the averaged predictability of the atmosphere is made in Thompsom's paper. The result of the estimate is that the atmosphere from a practical point of view should be predictable for 7.7 days. It is a curious fact that this estimate is close to the present ability as obtained from the present most advanced models.

After this long description of the very important work by Philip D. Thompson it may be pertinent to give a brief description of the person. He had come into meteorology during the World War II as many other young american scientists. Originally, it was his intention to become a mathematician, but contrary to so many others, he stayed in meteorology and in the military service, where he climbed through the grades and eventually retired as a full colonel. His interest in numerical weather prediction started, when he was assigned by the U.S. Air Force to the Institute for Advanced Studies in Princeton at a time, when John von Neumann was looking for problems suitable for the expected electronic computer. While he was not a member of the staff in Princeton, when the first forecasts were made, he conducted together with Dr. L. Gates a series of tests using the so-called thermotropic model designed by himself. In later stages of his scientific career he stayed mostly with theoretical studies, as for example described above. He was a very good writer. This can be seen from his scientific papers, but also from an elementary textbook in numerical weather prediction. He worked mostly alone. It appears that he has not published a scientific article with any co-author.

The atmospheric predictions are based in part on Newton's law saying that the acceleration of a particle of unit mass is equal to the sum of the forces on the particle, where each of the forces are calculated per unit mass. The second basic law applied in the prediction problem is the thermodynamic law saying that heat added to a particle of unit mass is used partly to increase the temperature and partly to expand the particle relative to the surroundings. The latter process requires work and therefore energy. In addition, we employ the so-called continuity equation saying that the increase of mass is equal to the net inflow of mass due to the motion. Finally, we need the proper gas law for the medium, in our case the atmosphere, relating pressure to density and temperature, and a budget equation for water vapour in the atmosphere.

All the information comes from classical physics. It is characteristic for this form of physics that it is deterministic. By this expression we mean that from a given initial state we can, using the whole system of laws, predict one and only one future state. It is probably the deterministic nature that gradually made classical physics uninteresting to physicists. In the beginning of the 20'th century they were mainly interested in the developments of the special and general relativity, the atomic and particle physics and quantum mechanics. Classical physics was considered to be completely developed, and its role was mainly in applications to problems such as geophysical and engineering problems.

This view has changed radically during the second half of the present century, partly due to the investigations by Thompson and partly to the contributions by Saltzman and Lorenz. The change comes from the discovery that although classical physics *is* deterministic, it displays a sensitivity to small changes in the initial state as described above in the summary of P.D.Thompson's paper. Not all systems show limited predictability. The prediction of the motion of a single planet around the Sun is very predictable (the so-called two-body problem), but as soon as one wants to predict the individual paths of several planets around the Sun (the many-body problem), it turns out that the general results are sensitive to small changes in the initial states of the various planets relative to the Sun. This discovery was made by Henri Poincaré and his co-workers between 1890 and 1900, when they worked on the three-body problem. Since the general three-body problem cannot be solved in a closed mathematical form, Poincaré suggested that it should be handled numerically, and it was in the numerical integrations that it was found that small changes in the initial states of the three planets could give large deviations in the predicted paths. These results did not have a major impact on classical physics at the time of their discovery. The reason could very well be that the field was very special and that the results came at a time when the physicists in the first couple of decades of the present century were much more interested in the development of the modern physics.

While the results of Thompson's investigations are very clearly outlined in his paper, he applied a sophisticated mathematical analysis to arrive at his results. This is perhaps the reason that his paper did not inspire other meteorologists to continue the work on atmospheric and other predictability problems. John M. Lewis (1996) has written a paper on some major scientific contributions by P.D. Thompson.

A second meteorological example of limited predictability was produced in the next decade. Barry Saltzman, then at the Travellers Research Center, had become interested in convection. The problem may be described as follows. Consider two horizontal plates where the lower plate is kept at a constant temperature that is higher than the also constant temperature of the upper plate. Experiments show that if the temperature difference between the two plates is very small then the heat will be carried from the lower to the upper plate by conduction in the air between them. For a somewhat larger temperature difference it is observed that convection cells start to appear. They consist of rather narrow regions in which the air moves rather fast from the lower to the upper plate and broader regions of sinking air, where the velocity is smaller than in the rising air resulting in a net transport of heat from the lower to the upper plate. When the temperature difference is even larger the upward and downward flows are more complicated containing many scales of motion. The flow is called fully developed convection.

Saltzman (1962) decided to describe the flow in a model in which he used a spectral formulation similar to the one described earlier in connection with the numerical procedures used in the atmospheric prediction models although he had to use a spectral formulation in the vertical direction as well. His numerical model described the various flow regimes as described above. He noticed that the largest scales of motion appeared to have their own behavior different from the more unruly dynamics of the smaller scales of motion. He discussed this behavior with E.N. Lorenz, who was a member of the staff of the MIT meteorological department, where Saltzman had taken his degrees. Lorenz became interested in the problem of the behavior of the largest scales and started to look at a low order model describing only the largest scales. Saltzman suggested, on the basis of his experience, the values of the parameters that gave the most interesting results, and Lorenz (1963) started to make numerical integrations of the low order systems.

For such low order systems it is in general possible to get some feeling for the behavior by finding the possible steady states. Once the steady states are determined, it becomes possible to investigate the stability of each of these states. In essence, one starts from a state very close to a steady state, and if the system falls back into the steady state in question then the state is *stable.* On the other hand, if the system moves away from the steady state if disturbed slightly the steady state is *unstable.*

The case studied by Lorenz has in general three steady states, and for the chosen values of the parameters it has one stable and two unstable states, but

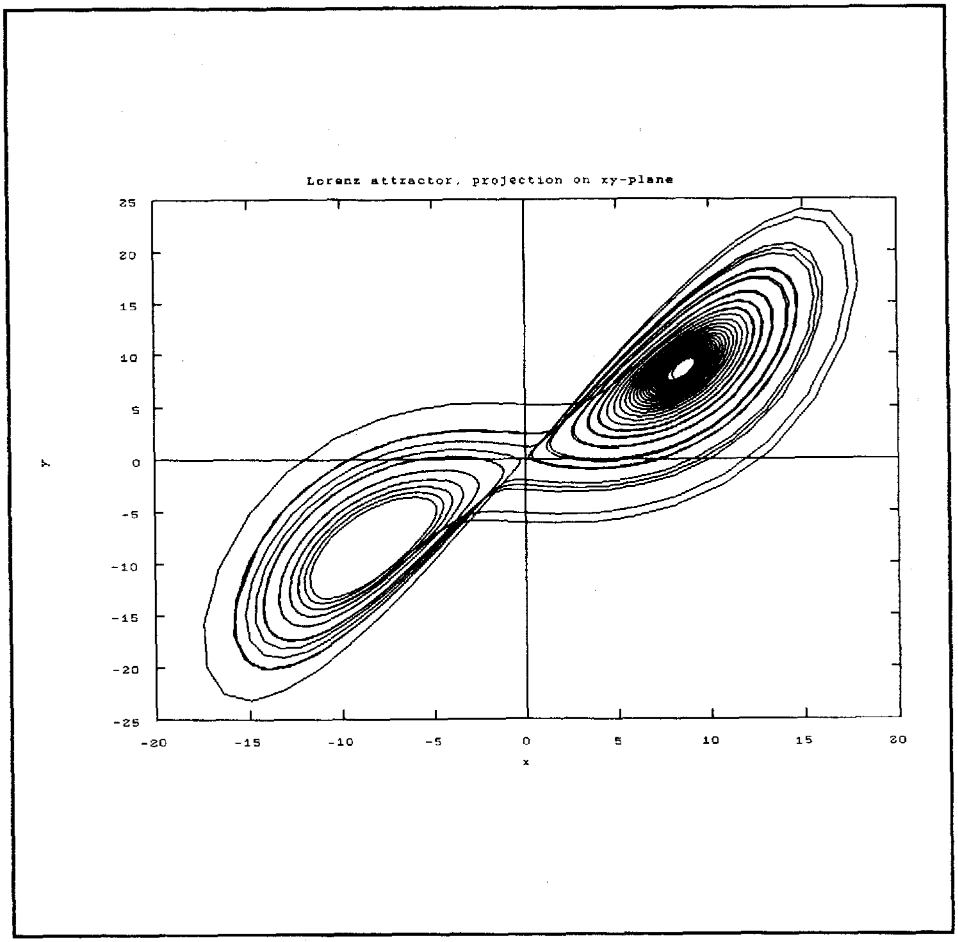


Fig. 10.2: The projection of the Lorenz attractor on the xy-plane.

the stable state is the uninteresting state of no motion. This means that the system has no place where it can come to rest. On the other hand, it is also possible to demonstrate that the system was stable »at large«. This means that the system will stay within a finite distance from its state of no motion or, in other words, if the system should pass across a certain critical surface in the three-dimensional space of amplitudes for the three components, it will be forced back inside the critical surface. But this means that the path of the system must move around in the limited space forever. The numerical integrations can be used to make pictures of the path of the system, and these pictures show figures that have a certain similarity to a flying butterfly.

Lorenz could also show that the system suffers from limited predictability

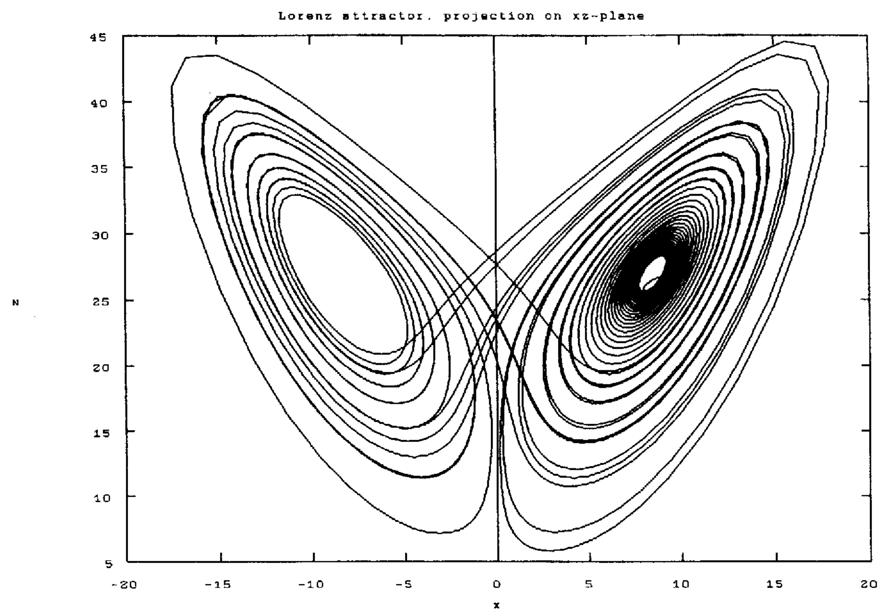


Fig. 10.3: The projection of the Lorenz attractor on the xz-plane.

in the same sense as described before, i.e. two initial states very close to each other will after a while be quite distant although the distance can be no more than the dimensions of the critical surface. The fact that the system stays within a limited region can also be expressed by saying that it looks as if the system is attracted to some point in the middle. It is therefore called an attractor. The other part of the behavior, i.e. the limited predictability and the lack of periodicity, was later given the name of 'a strange attractor', but that name has been provided by D. Ruelle and F. Takens (1971).

The Lorenz attractor has been described in many places in the literature. For our purpose of illustrating limited predictability it will suffice to show a couple of pictures of the attractor which of course is in the three-dimensional

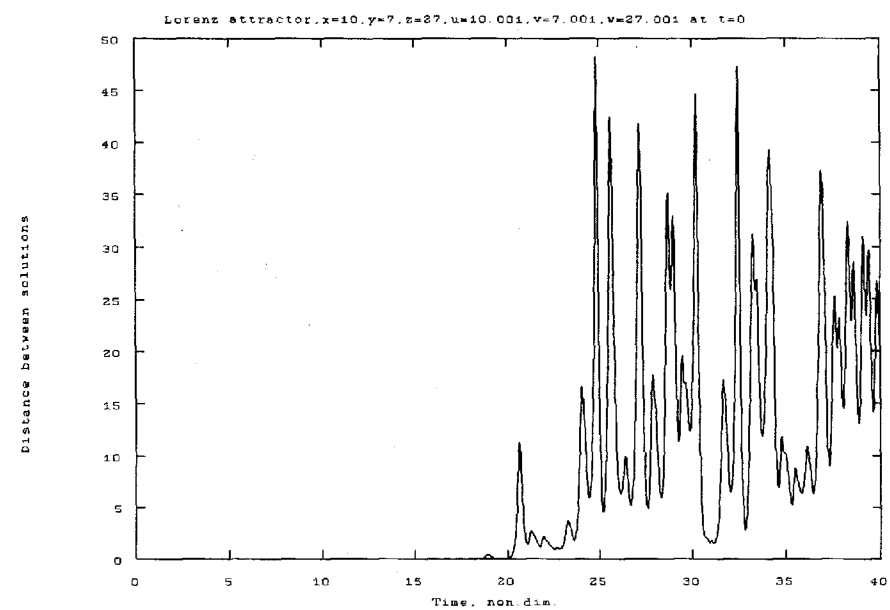


Fig. 10.4: The distance between two predictions using the Lorenz attractor. The initial state for one of the integrations is: x=10, y=7, z=27, while the initial state for the other integration are: u=10.001, v=7.001, w=27.001. The separation of the two forecasts occurs after about 20 time units.

space. Fig. 10.2 shows the projection on the xy-plane. A prediction starting on the left loop well inside the attractor will first move in the left loop for a while, but it will eventually, as shown on the figure shift to the right loop. The point then runs around this part of the attractor moving gradually a longer distance from the center of this loop. It then shifts to the left loop and so on. Figure 10.3 shows in a similar way the projection on the xz-plane.

In the following we shall give some examples of limited predictability using

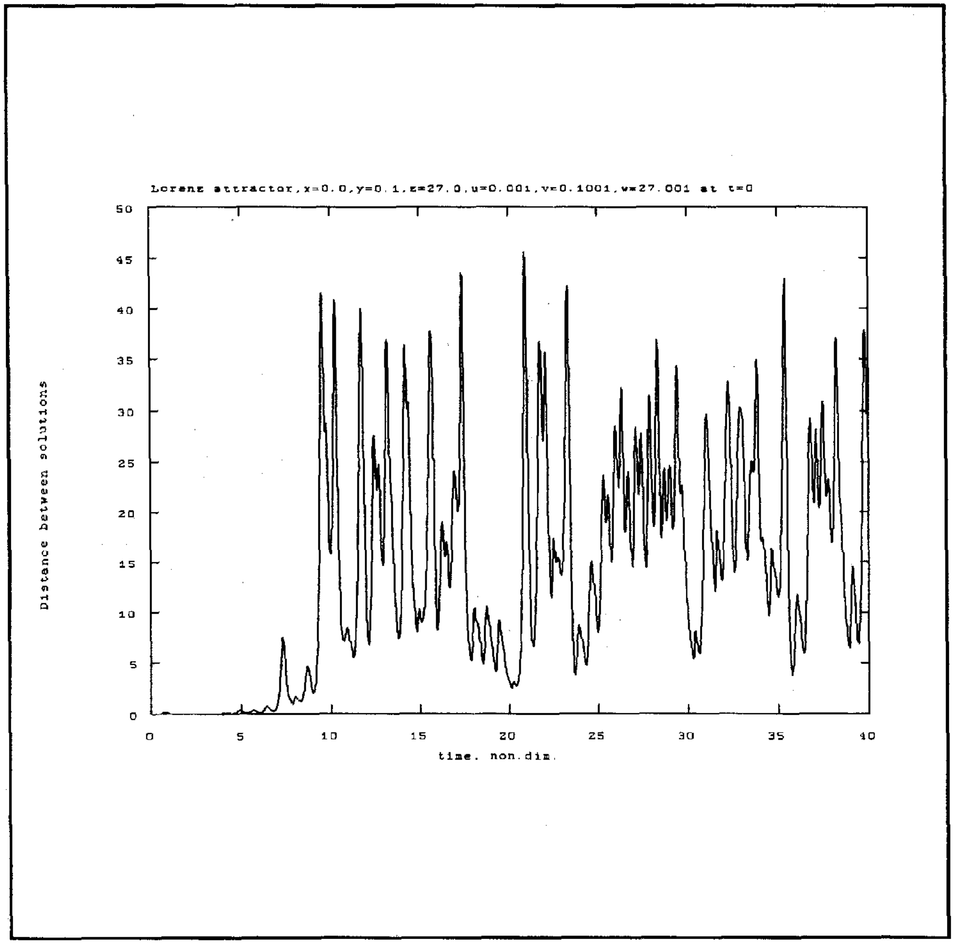


Fig. 10.5: Similar to Fig. 10.4, but the starting positions are taken from another part of the attractor. They are: x=0, y=0, z=27 and u=0.001, v=0.001, w= 27.001. The predictability is much shorter than in the case illustrated in Fig. 10.4.

the Lorenz equations. We start therefore two predictions in points close to each other. In the first example we select the two starting positions well inside one of the loops in the attractor using the initial values x=10, y=7 and z=27 for one prediction and these values plus 0.001 for the second prediction. We calculate then the distance (d) between the two forecasts as a function of time. The result is shown in Figure 10.4. It is seen that the distance remains very small up to and a little beyond the (nondimensional) time of 15 units. A little after 20 units we see that the distance suddenly becomes very large as a clear indication that predictability has been lost. We would expect a rather

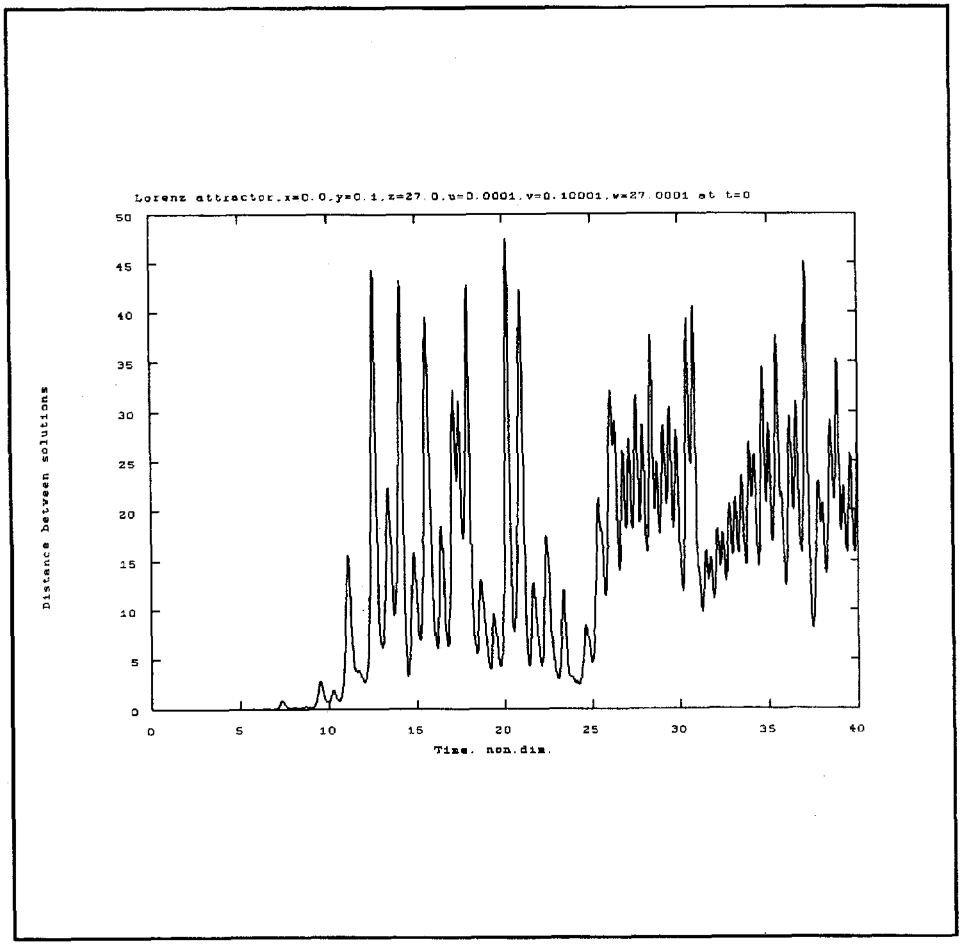


Fig. 10.6: Similar to Fig. 10.5, but the initial states are 10 times closer. They are: x=0, y=0, z=27, u=0.0001, v=0.0001, w=27.0001. The predictability time is about twice as long as in the previous case. It pays therefore to have accurate initial states.

large value as the limit of predictability because we in this case intuitively would expect that it takes a long time before the two predictions will be on different loops on the attractor.

In the next case we select the starting positions equally close (0.001) to eachother (x=0, y=0.1 and z=27) but selected close to the dividing line between the two »wings« of the attractor. In this case, shown in Figure 10.5, we find that the distance becomes large a little after 5 time units, or, in other words, the predictability limit is only about a quarter of the value in Figure 10.4.

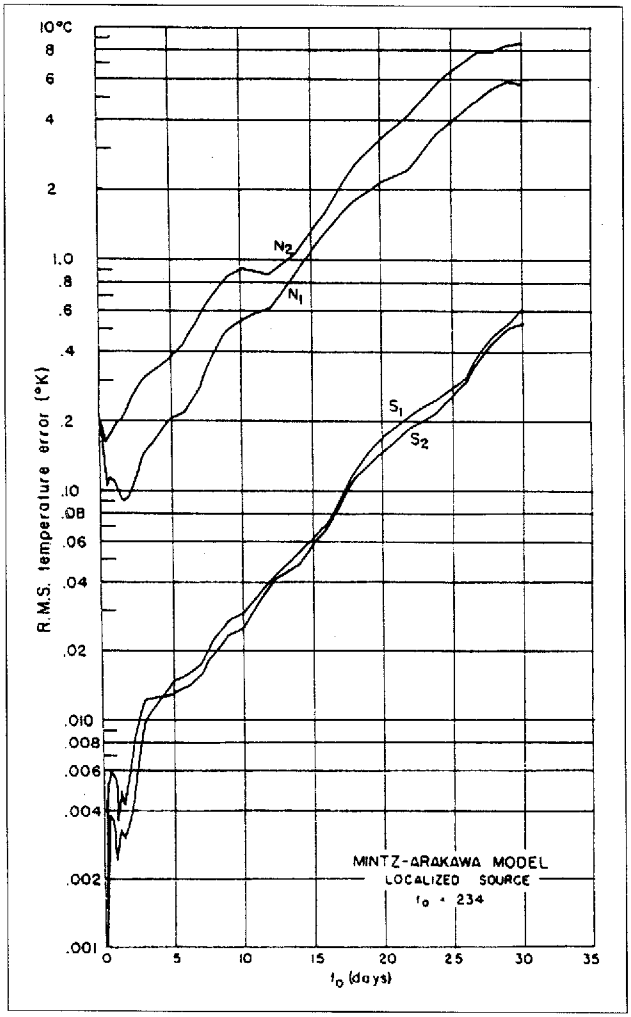


Fig. 10.7: The root-mean square temperature difference between two global integrations starting from two initial states that are identical except for a local small temperature difference introduced on day 234. N and S refer to the northern and the southern hemispheres.

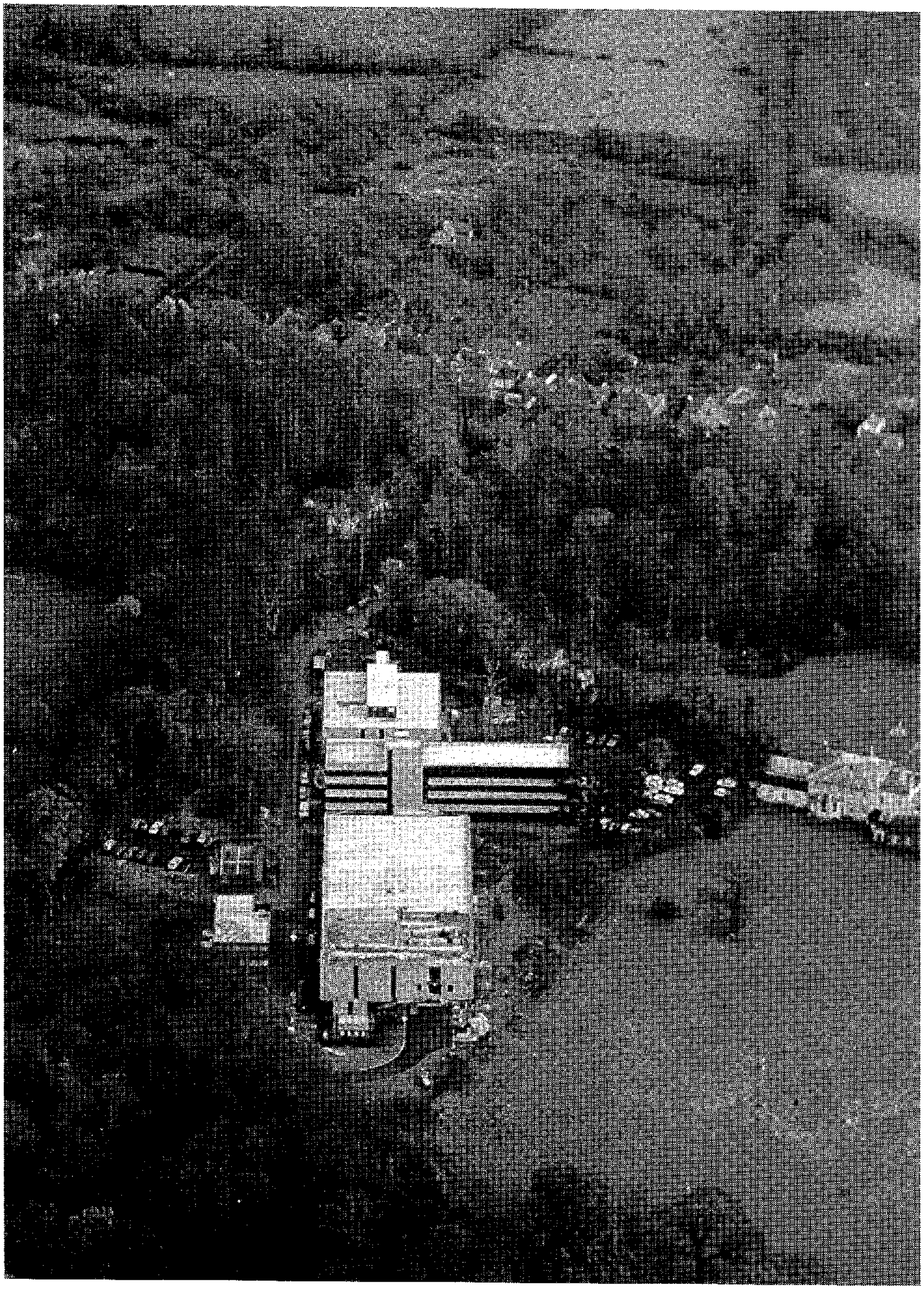
In the final example, we shall illustrate that it helps to obtain good observations. Figure 10.6 shows the distance between the two integrations when the second initial condition is selected 10 times closer to each other, i.e. 0.0001 is the difference in the coordinates of the initial states. As one can observe we gain several time units in predictablity. However, the gain is small compared to the increase in accuracy.

Lorenz's paper was published in 1963 in the meteorological literature. As such it was known in the meteorological community. Most of the researchers found it an interesting paper, but thought that it was after all a very special example of a low order convective system, and the conclusions could not immediately be transfered to the predictions of the large-scale atmospheric flow. However, J.G. Charney (1969), who at the time was interested in the Global Atmospheric Research Program (GARP) and the planning of its future activities, realized that the ideas in Lorenz's investigation could be tested by using the available general circulation models. He convinced Mintz, Leith and Smagorinsky to use the global models available to them to investigate the growth of differences between two integrations starting from small differences in the initial states. A special problem appeared in some of the models. They had for numerical reasons such large diffusion coefficients that the initial small differences were rapidly removed. However, models integrated with more realistic numerical schemes produced error growth patterns that indicated the limited predictability as one indeed should expect from the results obtained earlier by Philip D. Thompson (1957). The integrations can be considered as an expansion of Thompson's research to more general global models.

Based on the error growth results from the integrations of the global general circulation models (see, Figure (10.7)) it can be estimated that the upper limit of predictability of the atmosphere is of the order of one month. This estimate is somewhat uncertain and is based on assumed very small differences between the two initial states. In addition, the estimate is obtained using particular models, and there is no guarantee that by using other models one would arrive at exactly the same number. However, the order of magnitude is probably correct. It has therefore been called the theoretical limit of predictability. The estimated limit refers to integrations when one wants to follow individual atmospheric systems such as highs and lows and the movements of atmospheric waves. The estimate does not apply to attempts to predict the time-averaged state of the atmosphere. Such predictions are a special problem that will be described later.

We may therefore conclude that by 1970 the meteorological community was aware of the atmospheric limited predictability. The work by Lorenz became very well known in the broader areas of research in nonlinear processes in many other branches of physics. These cross-fertilizations are described in Lorenz's book »The Essence of Chaos« (1993) and can also be seen in some of the books consisting of collections of published papers on chaotic processes (see for example Hao Bai-Lin, 1984).

As presented above the limited predictability is a sensitivity to small changes in the initial state. However, it can just as well be presented as a sensitivity to small changes in the forcing. As we know the forcing of the atmosphere is the total heating per unit mass and unit time. Many different processes contribute to the total heating which depends on radiation, condensation and precipitation, transfer of heat and moisture between the underlying surface



The buildings of the European Centre for Medium-Range Weather Prediction under construction (about 1976) of the Earth, and the formation and maintenance of many different kinds of clouds. Most of these processes take place on a small (molecular) scale, but the net result has to be expressed as changes in the meteorological parameters in the models. The latter process is called parameterization, and the conversion of the net result of all small scale processes to the tendencies of the model parameters cannot be an accurate process. Just as there is an uncertainty in the initial state there is also an uncertainty in the specification of the energy sources of the atmosphere at any time. This is the second contributor to limited predictability.

11. The medium-range problem: The european centre

The theoretical limit of predictability is not the same as the practical or operational limit. The operational forecasts prepared by integrations of steadily more complicated, but also more realistic, models in the decades of the 1950's and 1960's were generally limited to forecasts for two or three days. As we recall the models were mostly limited to a few vertical levels and energy sources and sinks (dissipation) were not part of the models in the beginning. The effects of orography (mountain effects) appeared in the models rather early, but it took much longer before a realistic parameterization of the various heat sources and sinks became available for use in the prediction models.

On the other hand, simulations of the atmospheric general circulation had started as early as 1956 with N.A. Phillips' work using a two-level quasi-non-divergent model in a rectangular region on a beta-plane. The heating in the model was rather schematic consisting of a prescribed linear heating function with heating in the southern and cooling in the northern half of the region. The dissipation was restricted to an estimate of the dissipation of kinetic energy in the atmospheric boundary layer and to a horizontal diffusion with a constant coefficient. In spite of the simplicity in the formulation the model was capable of simulating the creation and the growth of atmospheric waves. The extrapolation to the surface of the Earth clearly showed major aspects of the growth of surface disturbances including at least some indications of the formation of fronts and the beginning of the occlusion process although numerical problems eventually prevented the continuation of the time-integration of the model. The energy generations by heating, the energy conversions from available potential to kinetic energy and from the zonal flow to the eddies or vice versa, and the dissipation of kinetic energy were calculated with results that were later confirmed by data studies of the same processes.

Smagorinsky (1963) and his group had generalized the simulations to the

Mr. Jean Labrousse (1932-) and Dr. Lennart Bengtsson (1935-)

spherical domain and had formulated the two-level model using the primitive equations. In the model they attempted to be more realistic with respect to the formulations of the parameterizations of the heating and the dissipation.

In order to determine the practical limit of predictability Kikuro Miyakoda (1969), who was on the staff of J. Smagorinsky's group, decided together with other staff members to use the latest of the general circulation models to make a set of prediction experiments and to verify the results. He found that the predictions were without practical value after about 3.5 days. To arrive at such a result it is of importance to compare predictions with reality using a common scheme. It is now standard procedure to make the verification in the following way. One computes the correlation coefficient between the predicted and the observed deviations from the climatological mean. This measure is called the anomaly correlation. It is close to unity in the beginning of the prediction, but will in general decrease as the prediction is extended into the future. Based upon the judgement of operational forecasters it was decided that when the correlation coefficient goes below the value 0.60, the operational limit of predictability has been reached. It is in this way that the limit of 3.5 days was determined.

The experimental predictions performed and evaluated by Miyakoda became very important for the creation of the institution now known as the »European Centre for Medium-Range Weather Forecasts« (ECMWF). In the late 1960's the European Commission had requested the Member States and the countries associated with the Commission to submit proposals for common European projects in the broad areas of science and technology. The European meteorological institutes decided to propose a center equipped with the largest and fastest computer. The original purpose was to acquire a machine that would permit the European countries to engage in meteorological calculations requiring both large computing speed and excellent storage facilities. Among the proposed projects were prediction experiments and simulation of the general circulation.

The project was well received, and a planning committee with representatives from the meteorological institutes was formed. They came relatively fast to an agreement about the scientific and technological aspects, but – as is so often the case in Europe – it was more difficult to agree on administrative and staffing matters and above all: the official languages to be used. The project stalled for a while. About this time Miyakoda's results became known in Europe, and it was then proposed that the main purpose of the center should be to prepare medium-range predictions on a daily basis for the use of the member states. It should still maintain the secondary purpose of providing computing power for individual, national or European meteorological projects. This proposal, originating from the Netherlands, was approved. The new and very practical proposal created the proper atmosphere in the high level planning committee to get the new institution under way. The language problems were solved by adopting Dutch, English, French, German and Italian as the official languages. Dutch was selected to provide a language from one of the smaller European countries.

The site of ECMWF was eventually selected to be Shinfield Park close to Reading located in Berkshire, west of London. The agreement should naturally be officially ratified by the Parliaments of the Member States which were Finland, Sweden, Denmark, United Kingdom, Ireland, West Germany, Holland, Belgium, France, Austria, Switzerland, Greece, Yugoslavia, Italy, Spain and Portugal (16 states). The ratification would take some time. In reality it took more than two years, but it was decided that the creation of ECMWF from a practical point of view should proceed. The United Kingdom provided temporary space in Bracknell awaiting the completion of the permanent headquarters in Shinfield Park. By January 1974 the author was appointed as the head of the planning staff. The idea was to work with the British architect on the design of the building, to acquire temporary computing facilities for the first experimental predictions, to prepare for the selection of the first operational computer for the headquarters and to select members of the planning staff of such a quality that they most likely could be selected for the permanent staff.

The energy crises dominated the world at this time. The problems were severe in England. At the very beginning, in January and February of 1974, I worked alone in the temporary quarters at Fitzwillam House in Bracknell since the European staff had not yet been appointed. It was not permitted to use the electricity in the daytime hours. Since these winter months could be rather dark, and since I had about 40 offices at my disposal I worked on the eastern side in the morning and moved to an office facing the west in the afternoon.

There were some amusing aspects of the regular meetings with the architect, Mr. Kidby, assigned by the British Governmant to take care of the construction of the new headquarters. At an early date we were asked some rather impossible questions. What will the total number of staff members be? How many of these will be women? How much room is required for the computing facilities? These questions, requiring almost immediate answers, helped a very great deal in making our own plans as solid as possible. Especially the size of the computer hall created many problems because we had no idea in 1974 which computer we eventually would get. We therefore based our answers on a survey of the larger member countries.

Due to the energy crisis and possible interruptions of the electrical network it was recommemded that the new headquarters should be equipped with two stand-by generators that would be able to provide the electrical power for the computers in emergency situations. They were installed in a separate house and tested at least once a month. To my knowledge they have never been used in a real emergency.

It was also recommended that we should have a so-called no-break system which would be able to provide an uninterrupted flow of meteorological observations to the analyses and forecasts. The system consisted of a number of large batteries that would go into operation automatically if the network should fail. We told the architect that the equipment should be installed in the basement under the computer hall. Answer: »We do not do basements in England«. The architect was told that dry basements could be made even in areas below sea-level, and examples were mentioned from The Netherlands and Denmark. Remark: »We do not do basements«. The solution was found during a week-end visit to the designated site. By turning the whole complex a certain angle, the computer wing could be located on a slope permitting an extra story on the low side, where the no-break system was installed on the ground floor. No basement!

Generally speaking we had a very good cooperation with the architect, and he saw to it that the building was finished on time. The planning process must have been in order. Only twenty years later was it necessary to add a new library building. Good meeting facilities with instantaneous translating facilities for the five official languages were made. An excellent lecture hall was provided for the special conferences and the annual courses for the meteorologists from the Member States.

When an international organization is established in one of the participating countries it is customary to sign a Headquarters Agreement. A member of the staff of the Foreign Office was assigned to take care of our problems. She drafted the Agreement supposedly based on similar agreements for other international institutions. It said among many other matters that the headquarters would be provided on land provided by the Queen and leased to the ECMWF for a period of 999 years. The annual rent would be »one peppercorn«. The building would be erected at the expense of the United Kingdom, but should be given back to the host country in good shape at the end of the 999 years. We asked the architect how we should interprete this statement. His answer was that one did not any longer build for the centuries and added that we could treat the matter in a lighthearted way since none of us would be around to face the consequences.

The staff moved into the building in 1977, and it was officially opened by the Prince of Wales.

We return now to the meteorological and technical problems. When a very good planning staff was created by selecting the very best European meteorologists with experience in numerical weather prediction we faced the problems of deciding on the first atmospheric prediction model and to explore the market for a sufficiently powerful computer.

There was agreement on a model based on the primitive equations. It was equally clear that the model would have to include parameterizations of all important heat sources and sinks as well as good parameterizations of the frictional processes. It was also agreed at an early stage that the operational model would have to be global so one could avoid all artificial boundary conditions. The planning staff was in the enviable position that all proposed parameterization schemes could be tested, and we could select the best existing scheme for our purposes, or we could invent a new scheme, if we had any bright ideas.

We made an excellent agreement with Dr. Smagorinsky and his group entailing that a version of their model was made available to us provided that a member of the planning staff could be located at the Geophysical Fluid Dynamics Laboratory to learn all the details of the model. A similar agreement was made with Dr. Yale Mintz at the University of California in Los Angeles.

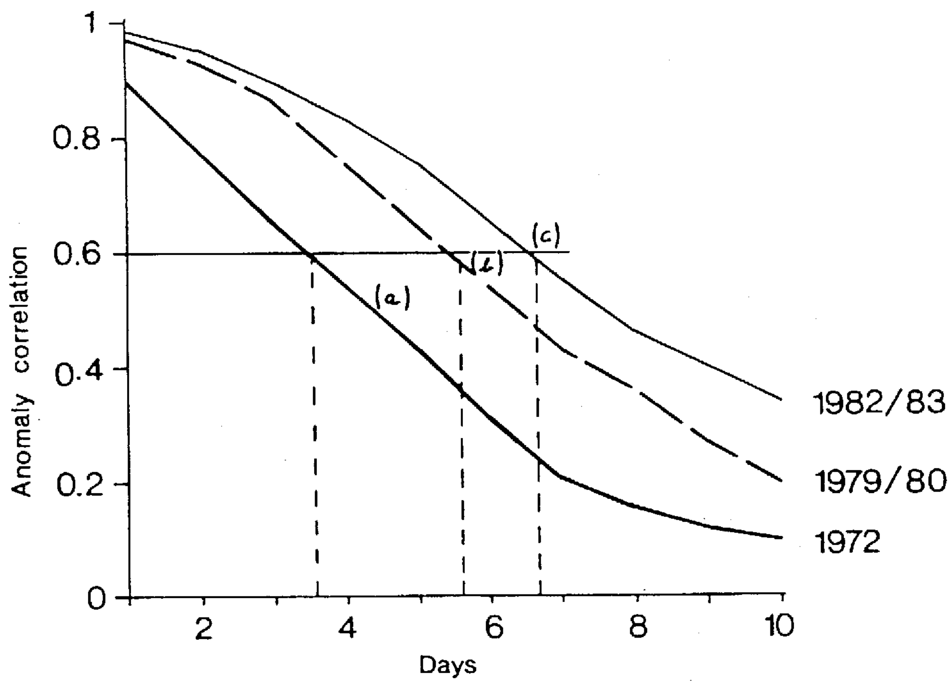


Fig. 11.1: The three curves show the anomaly correlation coefficient as a function of time, measured in days. The limit for practical predictability is in each case the point where the curve intersects the value of 0.6. Curve (a) refers to the Miyakoda forecasts with a predictability of about 3 $^1/_2$ days, curve (b) is for the first operational ECMWF forecasts with a predictability time of about 5 days, while curve (c) indicates the improvements in the ECMWF forecasts in 1982/83, when the predictability time is about 6 $^1/_2$ days.

Temporary staff members (Dr. A. Hollingsworth and Dr. R. Sadourny) were sent to both of these places, and both models were used in the experimental prediction program. These models were also very useful since many of the new staff members had no experience with general circulation models.

We made an agreement with Control Data Corporation to rent one of their computers for the experimental forecasts. It was installed in another building within walking distance of the temporary quarters in Bracknell.

Having solved all these preliminary problems some of the staff could turn their attention to the requirements of the first operational computer. Certain conditions had to be fulfilled. We were planning to make one medium-range forecast every 24 hours. We had to include the data processing of the incoming observations and the initial analyses in the estimates, but it turned out that the

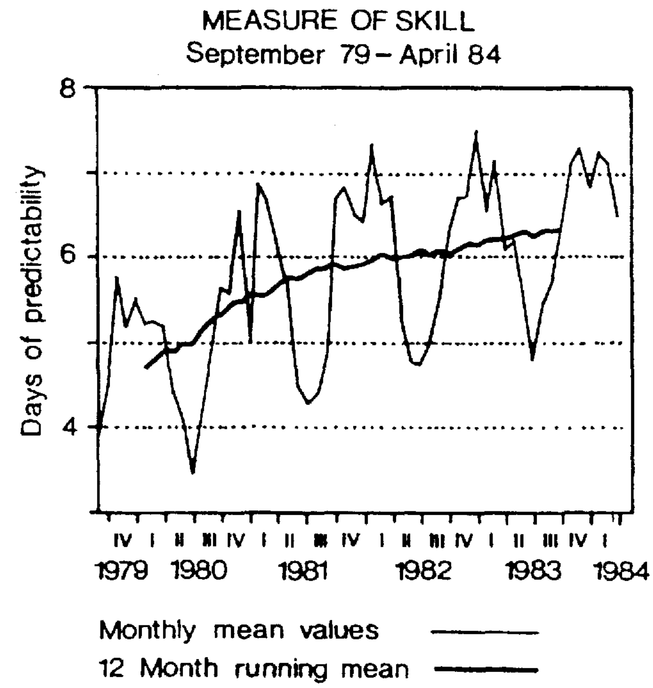


Fig. 11.2: The heavy curve shows the improvements in the ECMWF forecast as a function of time by plotting 12 months' running means, while the lighter curve is the plot of the monthly means. On the horizontal coordinate each year is divided in quarters. Note the smaller predictability for the summer season for these forecasts.

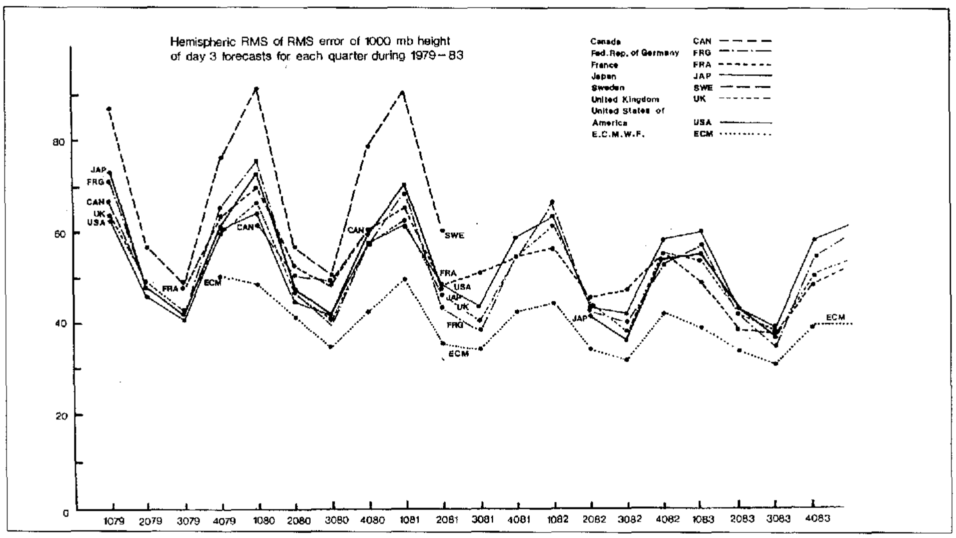


Fig. 11.3: A comparison between forecasts made by various organizations as a function of time indicated by year and quarter (1079 is the first quarter of the year 1979 etc.). The accuracy is this time measured by the root-mean-square error in forecasts for the surface of the Earth approximated by the 1000 mb= 1000 hPa surface.

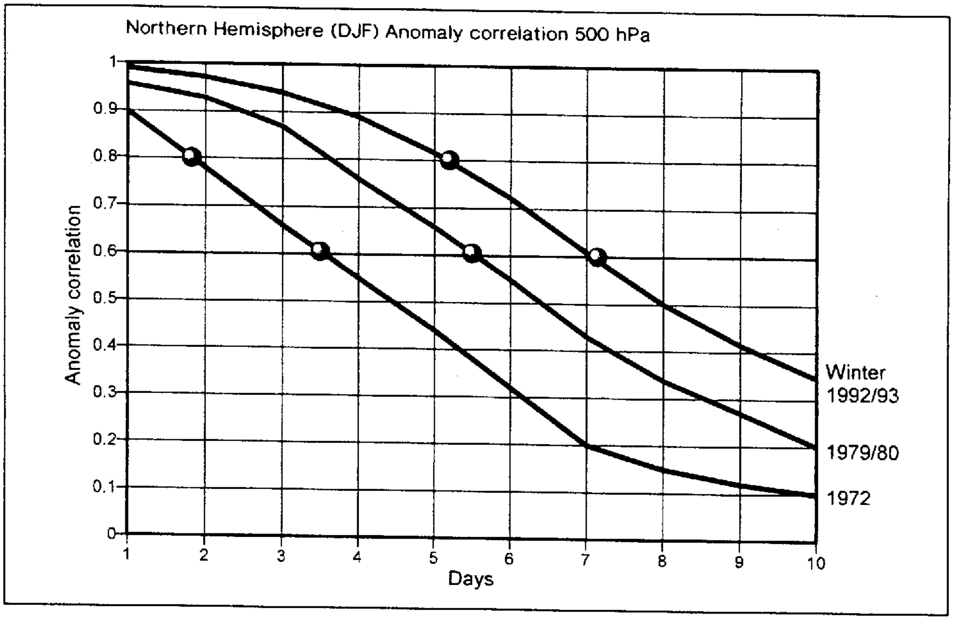


Fig. 11.4: This figure is similar to Fig. 11.1, but is updated to 1992/93 at which time the practical predictability exceeds 7 days. Since then the predictability has increased by about a half day.

bottleneck was running the 10 days predictions and ensuring that the predictions could be made available to the Member States in good time every day.

One may ask why we were planning to run forecasts for 10 days when the practical limit of predictability was 3.5 days at the time. The reason was that the Council, consisting of the meteorological directors (or their representatives) from the Member States, had *defined* 10 day predictions as the final goal. Everybody realized that it would be a long and difficult task to reach that goal, but a necessary requirement was that the 10 day forecasts were made and thus permitted a study of the deterioration of the forecasts as a function of time.

A number of estimates of the computer requirements were prepared under various assumptions and the result was that the new computer had to be able to perform about 50 million instructions per second (= 50 MIPS) which was the unit used at the time. No such computer was available on the market to our knowledge. The well known computer manufacturers (CDC, IBM, Honeywell, Ferranti etc.) did not offer anything close to the requirements. To our surprise it turned out that a small firm, headed by Dr. Seymour Cray, expected to be able to satisfy our wishes in time. Dr. Cray had left CDC to

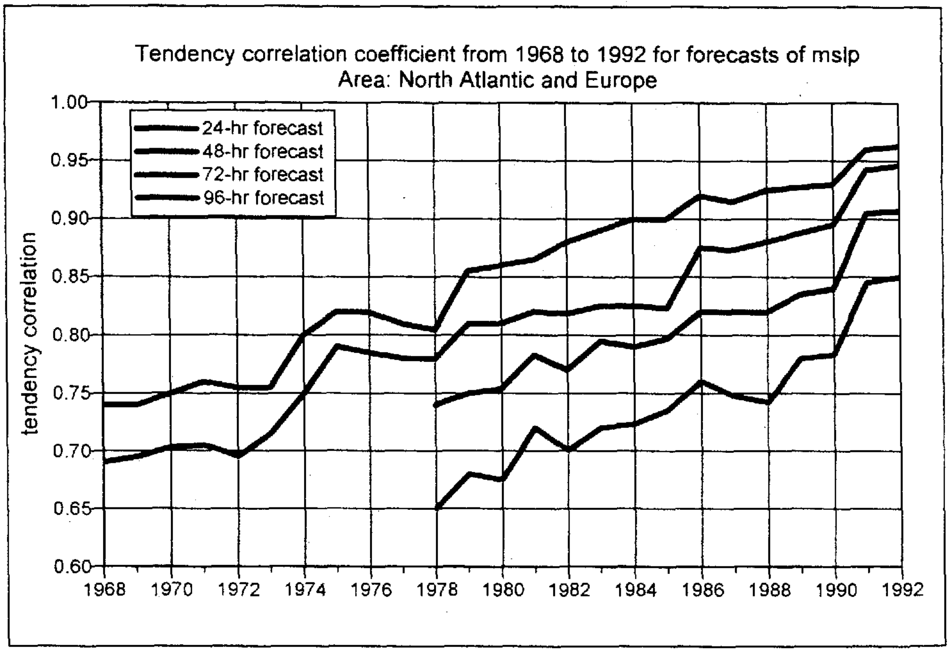


Fig. 11.5: The correlation coefficient between predicted and observed changes for 1, 2, 3, and 4 day forecasts covering the period 1968 to 1992. Note for example that a 4 day forecast in 1992 is better than a 1 day forecast in 1978.

concentrate on the design of his own computer. As soon as we received this information we visited Dr. Cray in the woods of Wisconsin where the first computer of an entirely new design was under construction. The main unit was small with a horizontal cross section similar to a horse shoe. The inner part of the horse shoe was quite small, but a slim person could get in and perform the wiring of the computer. Dr. Cray was quite unorthodox in many ways. The wiring was for example made by local high school students working on hourly wages in the afternoon.

Eventually we got the approval of the Council to purchase a CRAY-I computer. As a first installment we obtained the prototype, but it was very soon replaced by a custom-made computer. The CRAY had to be linked to other computers taking care of the incoming observations and still others which took care of the post-processing. Not surprisingly, we had reserved much too much space for the computer not knowing about the Cray-concept. On the other hand we had underestimated the space required for the storage of data and past predictions. All in all the space turned out to be adequate.

In 1977 the ECMWF could start the first experimental forecasts on the

CRAY-1 machine. The new headquarters could be used from October 1978. An improved computer – the CRAY-1A, serial number 9 -was delivered at ECMWF. It was the first CRAY computer in Europe. As soson as the programming could be done it was used to test the proposed operational model, and the first operational medium-range forecasts were delivered to the Member States on 1 July, 1979.

During the coming years the model was improved several times. The number of days of predictability went from less than 5 days to more than 6 days from 1979 to 1983. Figure 11.1 illustrates this statement. The figure also shows that the predictability is smaller in the summer than in winter. The reason was probably that the scale of motion is smaller in summer than in winter. With the resolution used in the initial model it was difficult to resolve the smaller systems in the summer resulting in larger errors. The same is illustrated in Figure 11.2 in a different way. This figure shows the forecast time at which the prediction error amounts to 75% of the persistence error, i.e. a different way to measure the predictability. Measured in this way the predictability becomes more than 7 days. Another illustration of the accuracy of the ECMWF predictions is given in Figure 11.3 measuring the RMS-error at 1000 hPa for 3 day (72 hour) compared to the same score for various other countries. It is seen that the Centre's errors are considerably smaller than any of the 3 day forecasts produced by other institutions.

We return to the old measure of the anomaly coefficient where it is considered that the predictability is measured by the time at which the coefficient becomes smaller than 0.6. The three curves in Figure 11.4 are the results of the Miyakoda experiments with a predictability time of about 3.5 days, the predictability of about 5.5 days when the Centre started to issue medium-range predictions, and the curve for the winter 1992/93 having a predictability of slightly more than 7 days.

Finally, in Figure 11.5 we look at the tendency correlation coefficients for 24, 48, 72 and 96 hour forecasts of mean sea level pressures for the North Atlantic and European regions. The forecasts have steadily improved. For example it is interesting to note that the forecast for 4 days (96 hours) in 1992 is of about the same accuracy as the 1 day (24 hours) forecast was in 1979.

I cannot mention all the excellent staff members at ECMWF and their various contributions. However, I do want to recognize the support that I have received from Lennart Bengtsson and Jean Labrousse who were heads of the research and the operational departments, respectively. Without their ingenuity, flair for the important questions at any particular time and their leadership in the departments, ECMWF would not have been ready to issue operational forecasts of such good quality at the promised time. Both of them became directors of ECMWF at a later time, first Jean Labrousse and later Lennart Bengtsson. None of them are presently with the Centre, whose director is Dr. David Burridge.

12. Problems of long-range predictions

It should be emphasized that the entire theory of predictability applies to forecasts starting from an initial state and attempting to predict the behavior of individual systems as they appear on the verification charts.

Forecasts beyond this limit must therefore be of another kind. What we have in mind is a forecast that for a given time interval describes the deviation from a reference state that could be a climatic map for the period. The forecasts would consist of maps describing the deviations of important meteorological parameters such as temperature or precipitaion from the reference state. The idea of producing this kind of forecasts goes back to the 1930's and 1940's, and such forecasts were for example produced in preparation of important military events during World War II. The most famous and most discussed forecast of this type was produced as a preparation for the landing of the Allied Forces in Normandy, France on 6 June, 1944. The preparation of the forecast has been described by J.M. Stagg (1971): »Forecast for Overlord« and by others. The discussion that is taking place more than 50 years after the event indicates what may happen when the basis of a forecast is subjective input from a number of different forecasters each relying on his own empirical ideas. J.M. Stagg, as the spokesman for the American and British forecasters, convinced General Eisenhower that the invasion should be postponed for 24 hours. They also told him that the winds over the channel would decrease to a level where the invasion would be possible. The winds did decrease, but only to a level where the landings were very difficult.

Forecasts for a period of a month and for three months were prepared in the 1940's and 1950's by groups in the U.S.A., United Kingdom, Germany, the Soviet Union and possibly also in other countries. Various methods were used. Some forecasters prefered the analogue method resting on the assumption that if it was possible to find a situation in the past very similar to the present, then the development in the future would be similar to the sequence observed by looking at the maps from the historical record. The proposed methodology seems to be logical, but the weak part is how one compares the present and the former meteorological situations. How close should they be to each other to assure similar developments in the two cases? There has probably never been two global meteorological states which are identical, but many that are quite similar.

Other methods started from the initial anomalies and used mean maps to displace these anomalies to future positions. The German meteorologists relied heavily on the descriptions of the various types of circulation as classified in the descriptions of »Grosswetterlagen«. In any case, the methods were totally empirical and statistical and not based on any model calculations. As far as the author knows long-range forecasts are no longer prepared on a regular basis anywhere in the world because a systematic verification of them resulted in unsatisfactory scores. However, some weather services prepare forecasts of this type for customers who are willing to pay the price.

Forecasts of the type discussed above would be of great economic and social importance if they could be prepared in a reliable way.

We need a forecast of a time-averaged state of the atmosphere. Time averages of observed maps show mainly an atmospheric flow on a large space scale because the travelling smaller disturbances will have a tendency to average to zero. The first question would be if one could formulate a model predicting only the largest scale motion. A low-order model of this type is relatively easy to formulate, but the use of the model would rest on the assumption that little or no interaction takes place between the largest scales of motion and the smaller transient disturbances. Diagnostic studies have shown that these two wave groups communicate, and that the smaller disturbances feed energy into the larger scales. The basic assumption appears therefore to be unjustified.

This situation is in a sense akin to the parameterization problems faced in deterministic forecasts. The parameterizations of cumulus convection, large-scale precipitation or air-sea interaction processes assume that we can make a prescription where the net effect on the larger scale of many events on the small scale can be expressed empirically in terms of the large- scale parameters. Is it possible to formulate such a parameterization of the effect of the transient disturbances on the time-averaged large scale flow? If so, we have not yet found a way to do so. The problems of long-range forecasts remain unsolved. But it is still being investigated by a number of groups.

One possibility is to make a number of forecasts from initial states that differ slightly from each other. A single one of these forecasts cannot be used because of the limited predictability. From the group of forecasts one can take the average and hope that the large-scale features of the mean would be a good guess of a future state. Some difficulties remain. How shall the various initial states be selected? Does the methodoly work only in certain, but not in other, meteorological situations?

13. Summary and concluding remarks

The field of numerical weather prediction has experienced a rapid development over the almost five decades from 1949 to 1997. The simple barotropic one-level model that was the basis for the first experiments in one-day predictions was soon replaced by baroclinic models having an increasing number of levels in the vertical direction in order to test the performance of multi-level baroclinic models based on the quasi-nondivergent assumptions. The experience from these models was somewhat disappointing because the various models gave results that in general were of almost the same accuracy as the barotropic forecasts at 500 hPa. On the other hand, the multi-level models provided predictions at both low and high tropospheric levels, and these forecasts were of operational importance. Nevertheless, it became clear that it was desirable to replace the quasi-nondivergent models with more general models based upon the basic atmospheric equations, the so-called primitive equations, containing only the single physical assumption of hydrostatic equilibrium.

The return to the original approach of L.F. Richardson was not without difficulties. The more general set of equations contain not only wave solutions corresponding to the meteorological waves that we want to forecast, but also waves of the gravity-inertial type. We want the general models to secure the interactions among the various wave types, but we do not want the fast gravity-inertial waves to dominate the forecasts. They will do so unless we make suitable slight modifications to the initial fields to prevent the domination of the fast waves in the first part of the prediction. It is thus necessary to pay attention to the initialization of the analysed fields obtained from the observations. Initialization procedures of various kinds have been developed. Some of the early schemes were based on the idea of using the quasi-nondivergent models at the initial time only to calculate the vertical velocities and the divergences. Later it turned out that a much more practical procedure was to make short-range (say, 3 hours) forward and backward forecasts from the uninitialized analyses and thus containing the fast waves, but then to remove the undesired fast waves by suitably defined numerical filters. The filtered fields could then be used as the initial state for the integration of the primitive equations. It turns out that one of the most elegant and most effective filters was invented by C. Dolph (1946).

Due to the improved models and the more effective initialization schemes it became gradually possible to extend the limit of operational predictability from about 3.5 days to about the double, i.e. one week. The improvement of these medium-range forecasts was, at least in the beginning, in part due to the better models and in part to the increased resolution in the integrations of the models. The latter steps were made possible by the design and production of much faster computers. The most recent analysis of the reasons for the latest extention of the predictability by a fraction of a day indicates that a major part of the increase is due to better data. The development has thus confirmed that forecast improvements are obtained by a better definition of the starting position and by more realistic models describing the physics of the atmosphere in a more realistic way.

All these results have been accomplished by the whole community of operational and research meteorologists helped on occasion by experts in applied mathematics and data processing.

Today the great majority of national weather services base the weather forecasts on numerical predictions produced either locally or received from the few centers for medium-range forecasts. In some countries the short-range forecasts are produced locally using high resolution, limited area models (HIRLAM) although the developments of the models sometimes have been done by cooperative efforts involving several countries. An example is the HIRLAM-project initiated originally by the Scandinavian countries and later expanded to include the Netherlands and Ireland. The limited area models are not independent of the global medium-range models since the latter provide boundary conditions for the former.

The European Centre for Medium-Range Weather Forecasts (ECMWF) has played a leading role in pushing the operational limit of prediction to about a week in a strong and mostly friendly competition with the national weather services in the large countries of the world. Especially, the competition and interaction between the National Meteorological Center (NMC) in the United States and ECMWF has undoubtedly helped both organizations to continue the efforts to close the gap between the operational and the theoretical limits of predictability. These developments have been made possible by the computer industry. Steadily improving computers with the necessary speed and capacity have permitted the increases in horizontal and vertical resolutions and the improved parameterizations of all the sub-grid scale physical processes.

Some individuals have made their special marks by providing a new foundation for the models or for the way in which the models can be handled from a numerical point of view. Jule G. Charney provided the equations for and the initial testing of the quasi-geostrophic models. Without these barotropic and baroclinic models based on his theory it would have been impossible to start numerical experimentation as early as 1949. Many other contributions within the framework of quasi-geostrophic theory and practice were made by Bert Bolin, Fred. Bushby, George P. Cressman, Arnt Eliassen, Norman A. Phillips, J. Sawyer, Fred. Shuman and Philip D. Thompson. P. Bergthorsson and Bo R. Döös developed the analysis schemes for the initial states. The basic principles of the objective analyses are still used today.

George Platzman, based on earlier work by Silberman, developed the spectral method for the integration of the vorticity equation. The method was tested and compared with the finite difference method by several researchers. Contributions to the solution of the initialization problem have been made by E. Eliasen, B. Machenhauer and R. Daley in the early days, while P. Lynch and X.-Y. Huang gradually have developed very effective numerical filter techniques. The use of the hybrid method, in which one repeatedly goes from a spectral to a field representation and vice versa, has been made possible by the Fast Fourier Transform method (FFT). The use of this method has influenced the choice of the spectral resolution in the models.

The implementation and testing of these ideas, of which only some of the good ones have been mentioned, was carried out by the staff of the operational centers around the world. Before a new model should be used in daily operational forecasts it is a definite requirement that it goes through extensive testing by making many experimental forecasts and then to follow up with a critical analysis of the resulting forecasts. Unfortunately, there are examples of modified models that have been accepted for operational use without sufficient testing.

A still unsolved problem of great importance is the production of reliable predictions of the mean state of the atmosphere beyond the limit of detailed predictions. Some encouragement can be found in the observed longer period oscillations found in data studies. The oscillations with several observed periods (30-35, 40-50 and 70 days) occur from time to time apparently in situations where the forcing of the atmosphere by heating exceeds certain critical limits. The period of the oscillation is supposedly determined by the geographical distribution of the heating patterns, but much more needs to be learned about these oscillations before prediction experiments can be initiated. Predictions for the coming seasons fall also in the class of unsolved problems.

Before closing one may ask the question: Is it likely that the operational limit of predictability will be extended significantly beyond the present level? The limited predictability is partly due to uncertainties in the initial state. The most recent results indicate that it pays to continue the struggle to decrease the uncertainty by improving the quality and the quantity of the various kinds of observations. However, the limited predictability of the atmosphere is also due to the imperfections of the models. In this regard one faces some almost unsolvable problems because of the necessary parameterizations of the sub-grid scale physical processes. These problems will not go away, but will stay with us forever due to the fact that the physical processes take place on a scale that cannot be observed or resolved in a global or limited area model. It seems that the only way is to continue the efforts to improve the various parameterizations in the models. In any case, it will be a difficult task to reach the original goal of reliable predictions for the coming 10 days.

References

Ashford, O.M., 1985: Prophet or Professor? The life and work of L.F. Richardson, Adam Hilger Ltd., Bristol and Boston.

Bai-Lin, Hao, 1984: Chaos, World Scientific Publishing Co. Pte. Ltd, 576 pp.

Bergeron, T., 1959: The young Carl-Gustav Rossby, The Atmosphere and the Sea in Motion, Oxford University Press, 51-55.

Bergthorsson, P., B.R. Döös, S. Fryklund, O. Haug, R. Lindquist, 1955: Routine forecasting with the barotropic model, Tellus, 7, 272-274.

Bergthorssson, P. and B.R. Döös, 1955: Numerical weather map analysis, Tellus, 7, 329-340.

Bjerknes, V., 1904: Weather forecasting as a problem in mechanics and physics, Meteor. Zeitschr., 21, 1-7.

Bolin, B., 1953: The adjustment of a non-balanced velocity field towards geostrophic equilibrium in a stratified fluid, Tellus, 5, 373-385.

Bolin, B., 1955: Numerical forecasting with the barotropic model, Tellus, 7, 27-49.

Bolin, B., 1956: An improved barotropic model and some aspects of using the balance equation for the three-dimensional flow, Tellus, 8, 61-75.

Bolin, B. and J. Charney, 1951: Numerical tendency computations from the barotropic vorticity equation, Tellus, 3, 248-257.

Burger, A.P., 1958: Scale considerations of planetary motions in the atmosphere, Tellus, 10, 195-205.

Byers, H.R., 1959: Carl-Gustav Rossby, the Organizer, The Atmosphere and the Sea in Motion, Oxford University Press, 56-59.

Cahn, A., 1945: An investigation of the free oscillations in a simple current system, Jour. of Meteorology, 2, 113-119.

Charney, J.G., 1947: The dynamics of long waves in a baroclinic westerly current, Jour, of Meteorology, 4, 135-162.

Charney, J.G., 1948: On the scale of atmospheric motion, Geofys. Publ., 17 (2), 17 pp.

Charney, J.G., 1949: On a physical basis for numerical prediction of the large-scale motions in the atmosphere, Jour. of Meteorology, 6, 371-385.

Charney, J.G., 1955: The use of the primitive equations of motion in numerical weather prediction, Tellus, 7, 22-26.

Charney, J.G., 1966: The feasibility of a global observation and analysis experiment, Nat. Acad. of Sci., Nat. Res. Council, 172 pp.

Charney, J.G. and A. Eliassen, 1949: A numerical method for predicting the perturbations of middle latitudes westerlies, Tellus, 1 (2), 38-54.

Charney, J.G., R. Fjørtoft and J. von Neumann, 1950: Numerical integrations of the barotropic vorticity equation, Tellus, 2, 237-254.

Charney, J.G. and N.A. Phillips, 1953: Numerical integration of the quasi-geostrophic equations for barotropic and simple baroclinic flows, Jour. of Meteorology, 10, 71-99.

Courant, R., K. Friedrichs and H. Lewy, 1928: Über die partiellen Differential Gleichungen der mathematischen Physik, Math. Ann., 100, 12-74.

Cressman, G.P., 1958: Barotropic divergence and very long atmospheric waves, Mo. Wea. Rev., 86, 245-250.

Dolph, C.L., 1946: A current distribution for broadside arrays which optimizes the relationship between beam width and side-lobe level, Proc. I.R.E., 34, 335-348.

Döös, B.R., 1956: Automation of 500 mb forecasts through successive numerical map analyses, Tellus, 8, 76-81.

Eliasen, E., 1960: On the initial development of frontal waves, Publ. of the Danish Meteorological Institute, No. 13, 109 pp.

Eliassen, A., 1959: On the formation of fronts in the atmosphere, The Atmosphere and the Sea in Motion, Oxford University Press, 277-287.

Eliassen, A., 1952: Simplified models of the atmosphere, designed for the purpose of numerical weather prediction, Tellus, 4, 145-156.

Fjørtoft, R., 1952: On a numerical method of integrating the barotropic vorticity equation, Tellus, 4, 179-194.

Friedman, R.M., 1989: Appropriating the weather (Vilhelm Bjerknes and the construction of a modern meteorology), Cornell University Press, 251 pp.

Frisinger, H.H., 1977: The history of meteorology: to 1800, Science History Publications, New York, New York, 148 pp.

Gilchrist, B. and G.P. Cressman, 1954: An experiment in objective analysis, Tellus, 6, 309-318.

Gold, E., 1954: Obituary notices of Fellows of the Royal Society, 9, 217-235.

Heims, S.J., 1981: John von Neumann and Norbert Wiener. From Mathematics to the Technologies of Life and Death, Mass. Inst. of Tech. Press, 547 pp.

Hinkelmann, K., 1951: Der Mechanismus des meteorologischen Lärmes, Tellus 3, 285-296.

Hinkelmann, K., 1959: Ein numerisches Experiment mit den primitiven Gleichungen, The Atmosphere and the Sea in Motion, Oxford University Press, 486-500.

Huang,X.-Y. and P. Lynch, 1993: Diabatic digital-filtering initialization: Application to the HIRLAM model, Mo. Wea. Rev., 121, 589-603.

Huang, X.-Y., A. Cederskov and E. Källén, 1994: A comparison between digital filtering initialization and nonlinear normal-mode initialization in a data assimilation system, Mo. Wea. Rev., 122, 1001-1015.

Kotchin, N., 1932: Über die Stabilität von Marguleschen Diskontinuitätsflächen, Beitr. zur Physik der freien Atmosphäre, Band 18, 129.

Kuo, H.L., 1949: Dynamic instability of two-dimensional, non-divergent flow in a barotropic atmosphere, Jour. of Meteorology, 6, 105-122.

Kutzbach, G., 1979: The thermal theory of cyclones, A history of meteorological thought in the nineteenth century, Historical Monograph Series, American Meteorological Society, 255 pp.

Lewis, J.M., 1996: Philip Thompson: Pages of a scientist's life, Bull. of the American Meteorological Society, 77, 107-113.

Lindzen, R.S., E.N. Lorenz and G.W. Platzman, 1990: The Atmosphere – A Challenge. The science of Jule Gregory Charney, American Meteorological Society, Historical Monograph Series, 321 pp.

Lorenz, E.N., 1963: Deterministic nonperiodic flow, Jour. of Atmos. Sci., 20, 130 141.

Lorenz, E.N., 1993: The essence of chaos, University of Washington Press, 226 pp.

Lynch, P. and X.-Y. Huang, 1994: Diabatic initialization using recursive filters, Tellus, 46A, 583-597.

Lynch, P., 1996: A simple filter for initialization, Technical Report No. 24, HIRLAM, Norrköping, Sweden, 13pp.

Machenhauer, B., 1977: On the dynamics of gravity oscillations in a shallow water model with applications to normal mode initialization, Beitr. Atmos. Phys., 50, 253-271.

Miyakoda, K., J. Smagorinsky, R.F. Strickler and G.D. Hembree, 1969: Experimental extended predictions with a nine-level hemispheric model, Mo. Wea. Rev., 97, 1-96.

Mollenhoff, C.R., 1988: Atanasoff, Forgotten father of the computer, Iowa State Univerity Press, Ames, Iowa, 274 pp.

Panofsky, J., 1949: Objective weather map analysis, Jour. of Meteorology, 6, 386-392.

Platzman, G.W., 1967: Bulletin of the American Meteorological Society, 48, 8, 514-552.

Platzman, G.W., 1960: The spectral form of the vorticity equation, Jour. of Meteorology, 17, 635-644.

Richardson, L.F., 1922: Weather prediction by numerical process, Cambridge University Press, 236 pp. (Also by Dover Publications, Inc., New York, 1965 with foreword by S. Chapman).

Rossby, C.-G., 1938: On the mutual adjustment of pressure and velocity distributions in certain simple current systems, Part II, Jour. of Marine Research, 1, 239-263.

Rossby, C.-G., 1939: Relation between variations in the intensity of zonal circulations of the atmosphere and the displacement of the semi-permanent centers of action, Jour. of Marine Research, 2, 38-55.

Rossby, C.-G., 1940: Planetary flow pattern in the atmosphere, Quart. Jour. of the Roy. Met. Soc., 6 (Supplement), 68-87.

Ruelle, D. and F. Takens, 1971: On the nature of turbulence, Comm. math. Physics, 20, 167-192.

Saltzman, B., 1962: Finite amplitude free convection as an initial value problem, Jour. of Atmos. Sci., 19, 329-341.

Smagorinsky, J., 1953: The dynamical influence of large-scale heat sources and sinks on the quasi-stationary mean motions of the atmosphere, Quart. Jour. of the Roy. Met. Soc., 79, 342-366.

Smebye, S.J., 1953: Tendency computations with a continuous two parametric atmospheric model, Tellus, 5, 219-223.

Solberg, H., 1928: Integrationen der atmosphärischen Störungsgleichungen, Geofys. Publ., 5, No. 9.

Staff Members, Univ. of Stockholm, 1952: Preliminary report on the prognostic value of barotropic models in the forecasting of the 500 mb height changes, Tellus, 4, 21-30.

Staff Members, Institute of Meteorology, University of Stockholm, 1954: Results of forecasting with the barotropic model on an electronic computer (BESK), Tellus, 6, 139-149.

Stagg, J.M., 1971: Forecast for Overlord, Ian Allen, London.

Thompson, P.D., 1953: On the theory of large-scale disturbances in a two-dimensional baroclinic equivalent of the atmosphere, Quart. Jour. of the Roy. Met. Soc., 79, 51-69.

Thompson, P.D., 1957: Uncertainty of initial state as a factor in predictability of large-scale atmospheric flow patterns, Tellus, 9, 275-295.

Thompson, P.D., 1983: A history of numerical weather prediction in the United States, Bulletin of the American Meteorological Society, 64, 755-769.

Wiin-Nielsen, A., 1959: On barotropic and baroclinic models with special emphasis on ultra-long waves, Mo. Wea. Rev., 87, 171-183.

Wiin-Nielsen, A., 1991: The birth of numerical weather prediction, Tellus, 43A-B, 36-52.

Wolff, P.M., 1958: The error in numerical forecasts due to the retrogression of ultra-long waves, Mo. Wea. Rev., 86, 245-250.

Submitted to the Academy May 1997.
Published October 1997