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The birth of numerical weather prediction

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ABSTRACT

The paper describes the major events leading gradually to operational, numerical, short-range predictions for the large-scale atmospheric flow. The theoretical foundation starting with Rossby's studies of the linearized, barotropic equation and ending a decade and a half later with the general formulation of the quasi-geostrophic, baroclinic model by Charney and Phillips is described. The problems connected with the very long waves and the inconsistences of the geostrophic approximation which were major obstacles in the first experimental forecasts are discussed. The resulting changes to divergent barotropic and baroclinic models and to the use of the balance equation are described. After the discussion of the theoretical foundation, the paper describes the major developments leading to the Meteorology Project at the Institute for Advanced Studied under the leadership of John von Neumann and Jule Charney followed by the establishment of the Joint Numerical Weather Prediction Unit in Suitland, Maryland. The inter-connected developments in Europe, taking place more-or-less at the same time, are described by concentrating on the activities in Stockholm where the barotropic model was used in many experiments leading also to operational forecasts. The further developments resulting in the use of the primitive equations and the formulation of medium-range forecasting models are not included in the paper.

1. Introduction

Numerical weather prediction, in some form or another, is today a part of the operational activities in most weather services around the world. Global forecasts for a week ahead are made at a few centers around the world, but many regional or even local forecasts for much shorter time periods are produced in many places. It is half a century ago, that the papers, on which the modern era of numerical forecasts is based, were appearing in the scientific literature. In this connection, it was decided to count from 1939–40, when Rossby's (1939) paper on the barotropic vorticity equation was published. As we shall see later, this paper had not only an important influence on the later development in dynamical meteorology, but also in the strategy used in the first numerical forecasts ever made.

When we talk about the birth of numerical weather prediction, we mean of course the production of a forecast by a time-integration of a suitable equation or a set of equations by numerical procedures. As we all know, this method of prediction became available after the invention of electronic computers in the mid 1940s in the USA by John von Neumann and others. But it does not mean that the meteorologists and other scientists of earlier times did not dream about such methods for the making of forecasts. We should of course on this occasion recognize that all of the activities in this field today rest on Newtonian mechanics (Fig. 1) and classical thermodynamics. It is also well-known that the astronomers especially were very successful in solving quite a number of problems in planetary motion using Newtonian mechanics. The process made by the astronomers undoubtedly impressed the meteorologists of earlier times. I have found a statement from the beginning of meteorological activities in Denmark where the Danish Royal Scientific Society were in charge of meteorological observations and their treatment. Thomas Bugge, a professor of Physics, writes in 1781: "If meteorology ever can reach any certainty, if it can ever be included in the natural sciences, and if the meteorologists ever could find

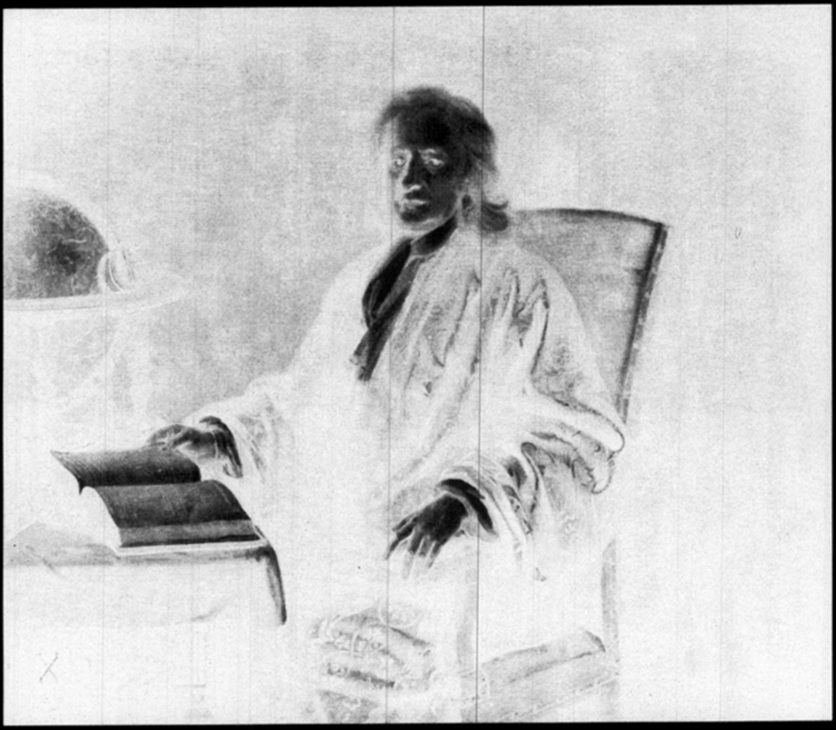

Fig. 1. Sir Isaac Newton (1642–1727), the founder of classical mechanics.

some cycles in the return of the weather, then they might be able to simulate the astronomers to a certain degree by computing the weather for the coming time." (Lomholt, (Royal Danish Scientific Society), 1960). I would expect that similar statements can be found in many other countries. Early contributions were made by Laplace (Fig. 2) and Helmholtz (Fig. 3).

Apart from such imprecise statements, we have apparently to come to the beginning of the present century to find a really meaningful statement of the prediction problem. Vilhelm Bjerknes (Fig. 4) (1904) gives a clear description of the possibilities of forecasting, at least in principle, in a short paper: "Weather Forecasting as a Problem in Mechanics and Physics." In this paper, he points out that the knowledge is available to formulate as many equations as there are variables in the atmosphere, i.e., the three equations of motion on the rotating Earth, the thermodynamic equation, the continuity equation, the gas equation and the equation for a measure of the water vapor in the atmosphere. He also judges that to solve the equations by any numerical methods is out of the question, but he has apparently some hope that graphical methods may be of some use. As we know, he would never make any real attempt to make any predictions using the equations in their nonlinear form. The following work of Vilhelm Bjerknes is not the topic of this paper, but those interested will find a detailed account in the recently published book by R. M. Friedman (1989).

Another person, L. F. Richardson (Fig. 5), accepted the challenge to try to compute the future state of the atmosphere. Sydney Chapman writes in the introduction to the paperback edition (1965) of Richardson's book: "Weather prediction by numerical process" (1922): "He had the hardihood, perhaps at that time too idealistic and unpractical, to attack the important and formidable problem of weather prediction by direct assault, based on known physical laws applied to a partly known initial state." We need to note here that Richardson's failure in a sense discouraged

Fig. 2. Pierre-Simone Laplace (1749–1827), who believed in unrestricted predictability.

all attempts by other people to repeat similar exercises. There was probably general agreement among those who took an interest in this matter that the approach was doomed to failure unless radically changed and unless both the observations and the computing machinery were greatly improved, but such a statement could easily be made with the advantage of hindsight and not be true, because the truth of the matter is probably that Richardson's work went rather unnoticed and was soon forgotten. We shall later touch on the question of whether or not the experiences of Richardson had any impact on the people who implemented numerical prediction in the 1950s and later. Suffice it here to say that those who want to study Richardson's work, will find many com-

Fig. 3. Hermann L. F. von Helmholtz (1821–1894), physicist, mathematician and physiologist, known in meteorology from the waves named after him and from the Helmholtz equation.

ments in the extensive review made by Platzman (1967) of the paperback edition. The readers who take an interest in Richardson's life, will find it well described in the biography written by Ashford (1985).

In the following sections, we shall follow the developments of the modern era of numerical weather prediction. Although the developments in the USA and in Europe, particularly Sweden, are closely connected in many ways, we shall nevertheless describe them separately, making cross references whenever possible. In Section 2 we shall briefly discuss the theoretical background created in the 1940s and of great use to the

Fig. 4. Vilhelm Bjerknes (1862–1951), who formulated the atmospheric prediction problem in principle.

Fig. 5. Lewis Fry Richardson (1881–1953), who made the first attempt to predict the weather by numerical processes.

pioneers of numerical predictions. Section 3 will contain the developments in the USA, mostly on the experimental side, and we shall finish the description with the formation of the unit for operational forecasts. The activities on the European side of the Atlantic will be given in Sections 4 and 5, and Section 6 will contain the concluding remarks.

2. The theoretical background

It is probably fair to begin the story around 1940 as mentioned in the introduction. The solutions of the linearized barotropic vorticity equation presented by Rossby (Fig. 6) (1939) form a complete break with the older studies of the Norwegian Bergen School, where the emphasis was on the possible instabilities of disturbances on a frontal surface. Two remarkable facts are connected with Rossby's study. The first is the simplicity of the model describing horizontal, nondivergent flow. The second is the importance of the beta effect, which is brought out in all clarity, because it is the only physical effect left in the model. There is no doubt that Rossby's paper started a whole new way of considering the dynamics of the free atmosphere. Other more cumbersome solutions to the barotropic vorticity equation were presented by others later in the 1940s, but the most important further development came from Rossby himself, who pointed out that the phase speed of the very long waves was altered significantly, if the model was generalized to a homogeneous fluid layer with a free surface. In such a model, divergence is possible due to the changes in the depth of the fluid, and the divergence is very effective in slowing down the very long waves.

The next very significant paper is Charney's (Fig. 7) (1947) study of the dynamics of long waves in a westerly current. This paper, which is based on the author's doctoral dissertation at University of California, Los Angeles (UCLA), is the basic study of baroclinic stability, but contains

Fig. 6. Carl-Gustav Rossby (1898–1957), who provided the first theory of the atmospheric long waves named after him.

Fig. 7. Jule Gregory Charney (1917–1981), the creator of quasi-geostrophic theory and the leader of the meteorology project in Princeton.

at the same time many of the elements of the quasi-geostrophic model. The basic model is one in which the zonal current increases linearly with height. The thermal stratification is characterized by a constant lapse rate. The frequency equation is of the confluent hypergeometric type. The main result is that instability sets in for a given wavelength for a sufficiently large vertical wind-shear, corresponding to a certain south-north temperature gradient. This paper is in the opinion of the present author the most significant paper in atmospheric dynamics produced so far in this century. Charney has in an interview conducted by George W. Platzman in 1980 commented on his own opinion of the paper. The report of the interview, published in 1987, will hereafter be referred to as GWP. It is noteworthy that Charney in GWP states that the above study is the most significant paper he ever made, "-- and everything has been downhill ever since". The citation should not be taken too seriously, because Charney produced in the following years other publications, which form the very foundation for the birth of numerical weather prediction.

After his dissertation, Charney spent some time in Chicago at the Department of Meteorology, headed at that time by Rossby. Charney was really on his way to Norway having received a fellowship from the National Research Council and was supposed to be in Chicago for only a couple of weeks. Rossby suggested that he should stay in Chicago at least until the following spring and as part of the inducement, he would obtain an invitation for Charney to participate in a meeting called by John von Neumann (Fig. 8) in Princeton.

This meeting took place in August 1946. At that time, the computer group at Princeton already knew what kind of computer they would be able to produce, so the meeting was more to gain the support of the meteorological community in the USA. The meeting first discussed some numerical problems such as the Courant-Friedrichs-Levy criterion for numerical stability. Later Rossby gave a talk with emphasis on the fact that the numerical prediction project was not only a mathematical problem, but especially a physical one. At this point, Rossby apparently wrote the turbulent equations for the atmosphere and pointed out that the various elements in the stress tensor were not known. It was thus necessary to work on the physical aspects. Charney attended the meeting, but was as he says himself rather unknown at the time. Nevertheless, he recalls that he had a conversation with Von Neumann, who asked him about his interest in the meteorology project. Charney replied that he was interested, but that he had a fellowship and was going to Norway. Charney made good use of this rather informal conversation at a later stage.

Fig. 8. John von Neumann (1903–1957), one of the designers of the electronic computer and a participant in the production of the first numerical, barotropic forecasts.

At this point, a new person enters, namely Philip D. Thompson. He had been assigned by the US Air Force to the Institute of Advanced Studies in Princeton where he worked under John von Neumann. This assignment started in the fall of 1946. In February of 1947, while Charney was still in Chicago, Thompson wrote him a letter. This letter is apparently dated 3 February, 1947, and the main question posed in the letter is according to GWP: "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 propagated?". Rather unusual for Charney, he already replies on 12 February, which must indicate that he was aware of the problem. In any case, his reply reproduced in GWP, contains a detailed analysis of the waves in a homogeneous fluid with a free surface, where inertial-gravitational waves as well as Rossby waves may exists. While this analysis had been carried out by Rossby in his earlier paper, Charney proceeds to show how the fast waves can be removed in this simple case. It is, however, also clear from the letter that Charney has not yet solved the general filtering problem. He states toward the end of the letter: "..., if your 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 a second letter from Charney to Thompson written 4 November 1947 from Oslo, Norway. In this letter, which contains many other points of a more 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 and geostrophic balance. This is the required filter!". We recognize here the elements of the two important papers, which were published 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 in the atmosphere (Charney, 1949). The same letter contains another important point of an entirely different nature. Charney tells Thompson that he would like to go to Princeton, and Thompson has apparently suggested such an arrangement with Von Neumann. In any case, when Charney left Norway, he went to Princeton to the meteorology project, where he stayed until 1956.

We may therefore say that by 1949 we have the necessary scientific background to start the experimental work on numerical weather prediction, since one could use the quasi-geostrophic theory as a first step. In Section 3, we shall look at some of the aspects of the meteorology project in Princeton.

3. The Princeton Project

The Princeton Meteorology Project was directed by John von Neumann who used the problem of meteorological forecasting by numerical processes as a test of the newly developed electronic computer. 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 Princeton Project are described by Thompson (1983).

Charney arrived in Princeton in the late spring of 1948. At that time, Philip Thompson and Gilbert Hunt were there. However, it took considerable time before the computer for the Institute of Advanced Studies was completed. It seems that the completion was made as late as 1952. In the meantime, the group completed many studies that did not require the computer. But a smaller computer was available at the Aberdeen Proving Grounds in Maryland, and it was on this computer that the first barotropic forecasts were made resulting in the well-known paper by Charney, Fjørtoft and Von Neumann (1950). Charney had arranged, with the good help of Sverre Petterssen, who was the chief of the forecasting services in Norway, that first Arnt Eliassen and later Ragnar Fjørtoft visited the Princeton project each for a period of several months.

It has always been my impression that when the decision to start with the barotropic vorticity equation was made, it was Rossby who had suggested this to Von Neumann. However, Charney maintains in GWP that this is not so, and that is was his decision to start with a model as simple as that. Even, George W. Platzman, who made the interview, argues with Charney on this point. He knows that there were repeated conversations between Rossby and Von Neumann, and finds it quite unlikely that Rossby, who after all had worked with the barotropic vorticity equation for almost 10 years at that time, should not have made this suggestion. However, Charney disagrees and says that Rossby did not think at all in terms of numerical integrations. In support of Charney's view, we can 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. It is also true that Charney arrived in Princeton with the quasi-geostrophic equations "in his pocket" with the intention of integrating a hierarchy of models adding, so to speak, one new process or parameter at a time. It could also be that when they had to go to the smaller machine at Aberdeen, it was quite natural to select the most simple model as a "training" model. Support for this point of view can be found in Charney's statement that he wanted rapidly to move to baroclinic models, which in his expectation, would be more realistic models.

The surprise was, of course, that the barotropic model was much richer than suspected. Some types of cyclogenesis were predicted with the simple model. As a matter of fact, Charney states that the barotropic model was generally underestimated as a practically useful model, and that the group in Princeton were surprised that the forecasts were as good as they were. Charney mentions the true story that he mailed some of the forecasts to L. F. Richardson who still lived in England. Richardson replied that he was very pleased to have the forecasts, which he had discussed with his wife. Mrs. Richardson made the statement that in her opinion, there was a little greater similarity between the forecast map and the verification map than between the forecast and the initial field. A somewhat limited praise, but still on the positive side. It should perhaps also be mentioned that these first barotropic forecasts did not use a Liebmann relaxation method to solve the Poisson equation, but rather obtained the solution using a Fourier resolution on the limited grid.

While the Princeton Group waited for the computer being built at the Institute to be finished, they continued to explore the barotropic and the baroclinic model by making a rather large number of tendency calculations by hand. This method consists of first calculating the right-hand side of the equations (in the adiabatic case without friction, it means the calculations of one or more Jacobians). The second step is to solve the Poisson or Helmholtz equation by hand using a relaxation method. Since these calculations were quite time-consuming, they needed help, and some of the wives were employed for this purpose, i.e., Mrs. Ellen Eliassen, wife of Arnt Eliassen, and Mrs. Margaret Smagorinsky, the latter being the wife of Joseph Smagorinsky who had joined the project from the US Weather Bureau.

Another prominent staff member who joined the project was Norman A. Phillips. He had been a graduate student at the University of Chicago under George Platzman and for his dissertation he had applied the quasi-geostrophic formulation as explained in Charney's papers to a 2-layer incompressible fluid model used by Rossby in his lectures. Phillips (1951) had an opportunity to present his research in an informal way to Charney during a visit which he made to Chicago. This model, which Phillips could convert to atmospheric conditions using the dispersion relationships, had an impact on the Princeton project, because it was the first baroclinic model used after the barotropic calculations. As pointed out by Phillips in GWP, it was the original plan to use an advective baroclinic model (i.e., with the static stability set to zero) as the next step, but this step was replaced by Phillips' model. The general quasi-geostrophic model with an arbitrary number of layers was formulated by Charney and Phillips (1953). It was used later in operational predictions with three or four layers.

Many visitors came to Princeton. In addition to Eliassen and Fjørtoft from Norway there were visits by Bert Bolin and Roy Berggren from Sweden, but we shall return to these persons later in the next section. The US Weather Bureau showed quite an interest in the Project. In addition to Smagorinsky, the Weather Bureau detailed George P. Cressman and Fred Shuman to Princeton. The idea behind these visits was that the Bureau considered that the project could produce such results that it would be possible to start operational numerical predictions within the Bureau. According to the record in GWP, the Bureau became convinced of the potential of the numerical procedures when the Princeton group eventually produced a reasonable three-level baroclinic forecast of the so-called Thanksgiving Day storm. It turned out that the first attempt to forecast the East coast storm was not very good. The development in the model was much weaker than in reality. However, in models with a low vertical resolution, one has some considerable freedom to locate the various pressure levels used in the model. According to Charney, the forecast became much better when the lower level came to 900 hPa. In any case, when Charney presented these improved results in Washington DC, it was apparently enough for the Bureau to consider its own program in numerical weather prediction.

The result of these considerations became the formation of the Joint Numerical Weather Prediction Unit (JNWP) created in July, 1954. It was a joint undertaking by the US Weather Bureau, the Air Weather Service of the US Air Force and the meteorological service of the US Navy. George Cressman became the Director of the Unit, Philip Thompson was the Chief of the Development Section, which was staffed by Fred Shuman, Art Bedient, Paul Wolff and William Hubert; the applications section had Joseph Smagorinsky as its first head and the staff was G. Arnason, C. Bristor, L. Carstensen, and H. Zartner. The first head of the analysis and operations section was Ed. Fawcett. Philip D. Thompson (1983) has described the activities of JNWP in the early days. There was a rather rapid change of staff, particularly the personnel coming from the Air Force and the Navy.

From the very beginning, JNWP used a barotropic model for operational forecasts. As long as this model is used in its pure form, one will experience a strong retrogression of the very long waves. Wolff (1958) designed a stop-gap method to decrease the considerable error caused by the retrogression. The main idea was to obtain the amplitude and phase of the longest waves (wave numbers 1, 2 and 3) from the initial field and then, periodically during the forecast, to replace these waves by the initial waves. The purely empirical method amounts in essence to keeping the very long waves stationary during the forecast. Cressman (1958) replaced this method at a later time by a modified barotropic model, which was formally based on the early model by Rossby consisting of a homogeneous fluid with a free surface. It turns out that the free surface effect creates a divergence with a pronounced influence on the very long waves, but with little effect on the shorter waves. Formally, the Poisson equation for the tendency is replaced by a Helmholtz equation, and Cressman determined the coefficient in this equation by numerical experiments to give a minimum error in the forecast. The same problem had been treated by Bolin (1956) and at a later time Wiin-Nielsen (1959) showed that the same model can be obtained as a special case of the baroclinic, quasi-geostrophic equations, if it is assumed that the atmosphere is equivalent barotropic (see also the Appendix of this paper).

The first baroclinic model, the so-called thermotropic model, was a two-parameter model designed by Thompson (1953), but it was later replaced by a three-level model which Cressman had worked on in Princeton. The latter model was put in operation in 1955, but the first experiences with baroclinic models for operational forecasts were not too good. It turned out in many instances that the 500 hPa baroclinic forecast was worse than the barotropic forecast. An intermediate model, which was used operationally for a time made barotropic forecasts at 500 hPa, while the thickness forecasts for the layer between 850 and 500 hPa used the forecast already made at 500 hPa.

It should also be mentioned that the disadvantages of using the strict geostrophic assumption were taken care of by using the balance equation, which is obtained by setting the total time derivative of the divergence equal to zero in the divergence equation. The use of the balance equation provided better 500 hPa forecasts, but the solution of this equation could from time to time create difficulties, because the ellipticity criterion occasionally was not satisfied in all points of the initial analysis. The major papers on the balance equation are those by Charney (1955), Bolin (1955, 1956) and by Thompson (1956).

A lot of new developments have taken place since 1956, but since this paper is restricted to the birth of numerical weather prediction, I shall abstain from a desription of the use of the primitive equations, the gradual introduction of "physics" into the prediction models and the resulting extension of the predictability time.

4. The Stockholm projects

A good part of the early developments in numerical weather prediction took place in Stockholm. A major reason for this fact is that Rossby had returned to Sweden in 1947, and he was, as we have seen in Sections 2 and 3, closely affiliated with the developments in the USA. He was not only very familiar with the theoretical progress by Charney and others, but also an advisor to John von Neumann. Another major reason could very well be that it was decided at a rather early stage that an electronic computer would be built in Stockholm where considerable experience with relay computers existed. The electronic computer became operational in early 1953 and known under the name of BESK.

Rossby had at the beginning in Stockholm two young students, Roy Berggren and Bert Bolin (Fig. 9). The three of them (Berggren, Bolin and Rossby, 1949) had already cooperated in an important observational study concerning the breakdown of the zonal flow to a blocking situation. Both of the students spent some time at the meteorological project in Princeton, and especially Bert Bolin got engaged in studies directly connected with numerical weather prediction in the very early stages. He became interested in the influence of the Earth's orography on the atmospheric flow and published a paper (Bolin, 1950) on these matters. In addition, he became engaged in the barotropic tendency calculations carried out in Princeton, and he and Charney (Bolin and Charney, 1951) published a joint paper describing the results. Consequently, he was well equipped to

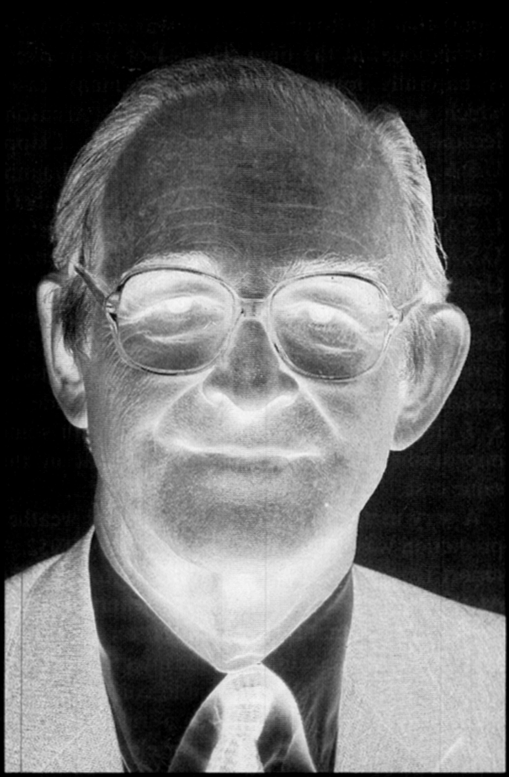

Fig. 9. Bert Bolin, born 1925, the leader of the efforts in numerical weather prediction in Stockholm and the first to use the balance equation in barotropic forecasting.

engage in similar studies when he returned to Stockholm.

At that time, Rossby had returned to Sweden, more-or-less permanently, and was in the process of creating the International Meteorological Institute. He gathered a number of visitors to the Institute, and as a common project, they engaged in barotropic tendency calculations. The first report (Staff Members, 1952) was published in *Tellus* and can be considered as a continuation of the study by Bolin and Charney (1951). The tendency calculations are nothing more than the very first step in a numerical integration. They were expressed as changes over a 12-period and compared with the corresponding observed changes. From such cases, of which the first report contains 14, one cannot draw final conclusions, but it is evident from the results that the participants were encouraged and were looking forward to real integrations of the nonlinear vorticity equation. It is also easy to see from the report that the Rossby Institute was already truly international at the time. The list of participants is naturally long considering the many cases which were included. They were: G. Arnason, Iceland; B. Bolin(*), Sweden; Ph. Clapp, U.S.A.; A. Eliassen(*), Norway; K. Hinkelmann, Germany; E. Hovmöller, Denmark; W. Hubert, U.S.A.; E. Kleinschmidt, Jr., Germany; C. Newton, U.S.A.; H. Newton, U.S.A.; H. Schweitzer, Germany; Ch. Steyer, Germany. The project leaders are marked with an asterisk. It is interesting to note that tendency calculations were also carried out for a 2-parameter baroclinic model designed by Eliassen (1952). They were made by S. J. Smebye (1953) for a single case with some improvement over the barotropic result in the same case.

A very useful conference on numerical weather prediction was held in Stockholm in May, 1952. A report is published by Bolin and Newton (1952). At the conference, there were presentations of a meteorological nature on models, on numerical and graphical procedures for the integration of the equations, and on the design and construction of Swedish computers of which a relay machine already existed (BARK) and an electronic computer (BESK) was being built. The design of BESK was very much influenced by the computer at the Institute for Advanced Studies in Princeton. Many of the papers presented at the conference were later published in *Tellus*. Especially useful for the progress in numerical weather prediction and for students entering the field was the very next issue of *Tellus* (vol. 4, no. 3, 1952) containing Eliassen's vertically integrated model, Eady's $2\frac{1}{2}$-dimensional model, Fjørtoft's graphical methods, and Platzman's remarks on high-speed automatic computers and their use in meteorology.

It is quite clear that one of the aims in Stockholm was to develop numerical prediction to the point where it could be used in operational forecasting. We shall first follow this line and later return to the more theoretical problems. The next step was to use the newly finished BESK to obtain a series of barotropic forecasts. They were obtained, and the results can be found in a paper by Staff Members, Institute of Meteorology, University of Stockholm (1954). A series of 24 24 h forecasts were made. The averaged correlation coefficient for these cases was 0.77, an improvement over other forecasts of similar nature, but especially over the 12 h tendency calculations which gave 0.69. Apart from the obvious errors due to truly baroclinic developments and erroneous boundary conditions, the report recognizes errors due to inaccurate analyses, particularly over the Atlantic region, and small-scale developments where the scale of the disturbances is only a few times the grid size. It is especially noteworthy that two of the forecasts, i.e., those for 23 and 24 March 1954, 03 GMT, were made on an operational basis. It is believed that these two cases represent the first forecasts finished in time to be of use in operation. The team was this time somewhat different from the earlier team. The participants were: G. Arnason, Iceland; H. Bedient, U.S.A.; P. Bergthorsson, Iceland; B. Bolin, Sweden; G. Dahlquist, Sweden; B. Döös, Sweden; N. Phillips, USA.

The work on the barotropic vorticity equation and its integration continued in the following year, where Bolin (1955) with an enlarged region for the forecasts extended them to 72 h with encouraging results. The usual correlation between computed and observed changes were for the 24 h forecasts 0.85, for 48 h 0.82 and for 72 h 0.70 for a region covering Western Europe. The various error sources in the barotropic forecasts are discussed in the paper. The balance equation appears for the first time in this issue of *Tellus*. Immediately preceeding

Bolin's paper, Charney (1955) had published a paper presenting the balance equation as well. None of the papers present examples of numerical solutions, but Bolin discusses the nature of the equation in some detail pointing out that the ellipticity criterion is not fulfilled in all regions of the map. The routine forecasting with the barotropic vorticity equation also continued in 1955, producing forecasts up to 72 h. These forecasts were made for the Weather Service of the Swedish Air Force by a team consisting of P. Bergthrosson, Iceland; B. Döös, Sweden; S. Fryklund, Sweden; O. Haug, Norway; and R. Lindquist, Sweden (Bergthorsson et al., 1955). A graphical presentation of the results is given in Fig. 10.

During the forecast experiments in the USA and in Sweden, it had become quite apparent that one would need a system of objective analysis for the forecasts. The problem already arose in the first period of the meteorology project in Princeton. A first attempt was published by Panofsky (1949) who used a system of fitting a two-dimensional polynomial to the observations over a rather large area. Such a procedure will require a fairly high degree of the polynomial, since it has to be able to describe all extrema within the region. A second attempt was made by Gilchrist and Cressman (1954). They changed the strategy by trying to fit the data in a small region around a given gridpoint. Consequently, they could restrict the polynomial to degree 2. They obtained acceptable results over the North American continent. When the problem of objective analysis was taken up by Bergthorsson and Döös (1955) in Stockholm, they concluded that the previous methods could not work in a satisfactory way if the observing stations are far apart as is the case over the oceans. The new idea which was introduced by them was to use a forecast from a previous time as a first guess to the analysis. The first guess would then be modified by the data resulting eventually in a final analysis. It is clear that the analysis in a large data-free region will be influenced very much by the first guess, but if the forecast is of reasonable accuracy,

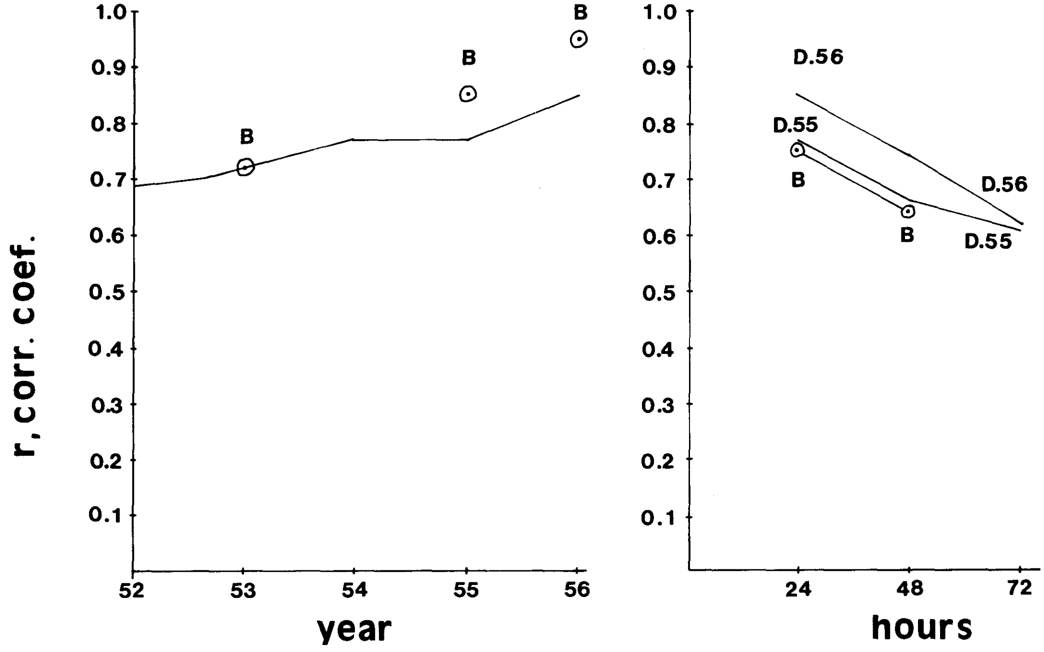

Fig. 10. The left-hand side of the figure shows the progress in the accuracy of barotropic forecasts as measured by the correlation coefficient between predicted and observed changes as a function of time for 24 h forecasts. The curve applies to routine forecasts, while the points marked B are research forecasts made by Bolin. The right-hand side shows the decrease in accuracy of 1, 2 and 3 day forecasts for different years. The curves are drawn from published data from the Stockholm group.

this is much better than just using, for instance, climatology in that region. This general idea of using a forecast as a first guess was later adopted in most objective analysis schemes in the world. The test of the analysis scheme in conjunction with operational forecasts were carried out during November and December in 1955, where the goal was to run without any subjective analysis for a full month. Döös (1955) has reported on the result of this experiment, but it was apparently quite difficult to reach the goal.

Bolin was the leader of the work in numerical weather prediction in Stochholm during the early 1950s. He gave several very significant contributions to the development of numerical prediction of which some have been mentioned above. He was particularly active in this field up to about 1956 (Fig. 11), whereafter he turned his interest to problems of a different nature. After the first tests of the barotropic equation, Bolin (1953a) turned his interest for a while to the baroclinic models. He derived the equations for a 2-parameter model in such a way that he could obtain a solution by graphical methods. A single forecast was made with a result that showed a slight improvement over the barotropic forecast for the same case. In the same year, he published his contribution to the adjustment problem, a study which must have been inspired by Rossby who had worked on the problem himself (Rossby, 1938). Bolin (1953b) generalized the investigation to a stratified fluid while earlier studies (Cahn, 1945) had been dealing with homogeneous fluids.

The problem of solving the balance equation and using the solution in the barotropic vorticity equation was taken up by Bolin (1956). He gives examples of the improvements of the forecasts as compared to the use of the geostrophic wind. In another section of the same paper, he gives his version of reducing the speed of the very long waves. This is done by formulating a model consisting of a troposphere with constant density and a stratosphere on top with a smaller, constant density. This model has as its final result a forecast

Fig. 11. A part of the numerical weather prediction group during the later stages in Stockholm. From the left: Bo R. Döös, the author, Fritz Defant and Bert Bolin.

equation which formally is the same as the one used by Cressman (1958). As we recall, Cressman determined the value of the coefficient in the Helmholtz equation by numerical experiment, while Bolin's coefficient depends on the assumptions made regarding the densities in the two fluid layers. The two coefficients are, however, of the same order of magnitude.

5. Other efforts

The Princeton and Stockholm Projects as described above were certainly the major efforts in the early stages of the development of numerical weather prediction, but is goes almost without saying that other research institutes gave contributions as well. Fjørtoft's (1952) graphical methods played a role in the period of the early 1950's. These procedures were first tested in the Norwegian weather service, and later, when Fjørtoft served as Professor of Meteorology at the University of Copenhagen, he continued a project concentrating on these procedures. Having worked myself on the project, I recall the many pieces of semi-transparent paper which were necessary to complete even a simple barotropic forecast. To my knowledge, he never published the results on the accuracy of the method, in spite of the many cases which were completed.

It is also evident that the research department in the German Weather Service, headed by K. Hinkelmann, gave very significant contributions. It is, however, characteristic for this group that it turned its attention at a very early stage to the integration of the primitive equations (Hinkelmann, 1959).

One may obtain a relatively complete picture of the status of numerical weather prediction research and development in the middle of the 1950s by noting the papers given at a conference on the subject held in Frankfurt in 1956. The abstracts of the presentations can be found in a publication issued by *Deutschen Wetterdienst* (1957), and in the following, reference is made to this publication and not to the papers published elsewhere. Reports were given on operational numerical forecasting by Philip D. Thompson for the USA and by O. Herrlin for Sweden, while B. R. Döös reported on the objective analyses. Extensive testing of models were carried out by W. L. Gates on the barotropic and thermotropic models in the USA, by E. Knighting on the Sawyer-Bushby model in the UK, and by O. Haug and S. Brandejs et al. on the use of Fjørtofts graphical methods in Norway and Czechoslovakia, respectively. K. Gambo et al. presented a case study of the prediction of a cyclone in the Far East and the associated precipitation. The major part of the German research group (Hinkelmann, Hollman and Edelmann) were concerned with various aspects of non-geostrophic motion. Papers on the same subject were delivered by L. Berkofsky and P. D. Thompson. J. Smagorinsky reported on the inclusion of moist-adiabatic processes, while F. Wippermann spoke on the subject of the orography.

It is thus clear that numerical prediction had spread to many research centers and meteorological services at this time.

6. Concluding remarks

We may conclude that during the period from 1940 to the middle of the 1950s, the research community accomplished a paving of the road for operational numerical weather predictions. In this paper, we have concentrated on the developments which took place in the USA and Sweden, where a great deal of the pioneering work was done, both in theory and in practice, but this is not to say that scientists from other countries did not participate as can be seen from Section 5. We have seen that the two projects which have been described in some detail were international in nature. It is evident that the two Norwegians, Eliassen and Fjørtoft, gave important contributions on both sides of the Atlantic. As we can see from the lists of participants, the various projects had a very liberal attitude with respect to international visitors. The German meteorologists participated from the start, and they gave new and original contributions. Especially, Hinkelmann's (1951) early paper on meteorological noise broke new ground. After the middle of the 1950s, there were efforts in many countries to start numerical predictions on an operational level. The brief history given above is thus a description of a revolution in both meteorological research and operational forecasting, carried out over a short span of years.

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Appendix

A historical note

As mentioned in the main text, it was a problem in the early days of short-range numerical forecasting to correct for the rapid retrogression of the very long waves in a purely nondivergent, barotropic forecast (Wolff, 1958). Bolin (1955, 1956) and Cressman (1958) used somewhat different interpretations, but in both cases, the basic model consisted of two homogeneous layers of which the motion in the upper layer was neglected. The resulting equation is, however, in both cases of the form:

$$\frac{\partial}{\partial t}(\zeta - q^2\psi) + u\frac{\partial \zeta}{\partial x} + v\frac{\partial \zeta}{\partial y} + \beta v = 0. \tag{1}$$

The interpretation of a model consisting of two homogeneous layers is difficult in terms of the actual atmosphere. The two authors used quite different methods to arrive at the numerical value of q^2. Bolin interpreted his two layers as the troposphere and the stratosphere. The expression for the coefficient is in his formulation:

$$q_B^2 = \frac{f_0^2}{\nu g D_0}, \tag{2}$$

where f is the Coriolis parameter. g the acceleration of gravity, D_0 the mean tropopause height, and ν a factor equal to the difference between the densities of the two layers divided by the density of the lower layer. No attempt is made to calculate mean densities for the two layers, but for ν, Bolin selects a value to reflect the changes in the tropopause height. It is done by noting that the total variation in D, the 500 hPa height, is 600 to 800 m and that the corresponding variation in the tropopause height is 5 to 10 times this value. By such order of magnitude arguments, he arrives at the value $\nu = \frac{1}{8}$. Setting $f_0 = 10^{-4}\,\mathrm{s}^{-1}$, $g = 9.8\,\mathrm{ms}^{-1}$, $D_0 = 8000$ m, he obtains

$$q_B^2 = 1.0 \times 10^{-12}\,\mathrm{m}^{-2}. \tag{3}$$

Cressman, on the other hand, decides to determine the value of q^2 experimentally by performing a number of forecasts from the same initial data and selecting the value of the coefficient giving the smallest error. Such a procedure is operationally efficient, but physically unsatisfactory, because it assumes that the divergence effect is the only source of error in the equivalent barotropic forecast. In this way, he obtained

$$q_C^2 = 0.7 \times 10^{-12}\,\mathrm{m}^{-2}, \tag{4}$$

or a somewhat lower value of the coefficient.

Wiin-Nielsen (1959) tried to calculate a value of the coefficient by using the equivalent-barotropic assumption and noting that the advection of temperature in the thermodynamic equation vanishes in this case. However, also this approach to the problem leads to uncertainties in the way in which it was carried out. The reason is that it was assumed that the vertical variation of the geopotential and the vertical variation of the static stability are independent of each other.

With the advantage of hindsight, we shall in the following part of this note make a new derivation of the numerical value of the coefficient. It is obtained by noting that the equivalent-barotropic atmosphere is a special case of the baroclinic, quasi-geostrophic model. This model is governed by the conservation of quasi-geostrophic, potential vorticity, which Arnt Eliassen has proposed should be called the "Charney vorticity". We adopt this proposal in the remaining part of the note. The Charney vorticity is:

$$Q = f + \zeta + \frac{\partial}{\partial \pi}\left(\frac{f_0^2}{\sigma p_0^2}\frac{\partial \psi}{\partial \pi}\right), \tag{5}$$

where $\zeta = \nabla^2\psi$ is the relative vorticity, ψ the streamfunction, $\sigma = -\alpha(\partial \ln \theta/\partial p)$ the static stability and $\pi = p/p_0$ the nondimensional pressure, and $p_0 = 1000$ hPa. Using the fact that Q is conserved in the horizontal, nondivergent flow and using the equivalent barotropic assumption which may be written in the form:

$$\psi = A(\pi)\,\bar{\psi}, \tag{6}$$

where the bar means a vertical average with respect to π. Finally, an assumption concerning the vertical variation of the static stability parameter is introduced:

$$\sigma = \sigma_0 \pi^{-\delta}. \tag{7}$$

We apply the standard derivation of the equivalent, barotropic forecast equation which again takes the form of eq. (1). However, this time the coefficient is given by the expression:

$$q^2 = -\frac{f_0^2}{\sigma_0 p_0^2} (\mathrm{d}A/\mathrm{d}\pi)_1. \tag{8}$$

The remaining part of this note is concerned with the determination of the numerical value of the coefficient in (8). We start by noting that the hydrostatic equation is

$$\pi \frac{\mathrm{d}\phi}{\mathrm{d}\pi} = -RT. \tag{9}$$

We integrate (9) from 0 to 1, performing the left-hand side of the equation by integration by parts. The result is:

$$\bar{\phi} = R\bar{T}. \tag{10}$$

Assuming as usual that $\psi = \phi/f_0$, we obtain from (6) that

$$\frac{\partial \phi}{\partial \pi} = (\mathrm{d}A/\mathrm{d}\pi)\, \bar{\phi} = -\frac{RT}{\pi}, \tag{11}$$

which for $\pi = 1$ gives:

$$\left(\frac{\mathrm{d}A}{\mathrm{d}\pi}\right)_1 = -\frac{T_1}{\bar{T}}. \tag{12}$$

The only remaining problem is to evaluate $\bar{T}$, but this is easily done from (7) which may be written in the form:

$$\pi \frac{\mathrm{d}T}{\mathrm{d}\pi} - \kappa T = -\frac{\sigma_0 p_0^2}{R} \pi^{2-\delta}, \tag{13}$$

where $\kappa = R/c_p$ and c_p is the specific heat for constant pressure. Since we are interested in the vertical mean value only, we may calculate the vertical mean of each term integrating the first term by parts. The result is:

$$\frac{T}{T_1} = \frac{1}{1+\kappa}\left(1 + \frac{\sigma_0 p_0^2}{f_0^2} \frac{1}{3-\delta}\right), \tag{14}$$

and we get as the final result:

$$q^2 = \frac{f_0^2}{\sigma_0 p_0^2} \frac{(1+\kappa)(3-\delta)\, RT_1}{(3-\delta)\, RT_1 + \sigma_0 p_0^2}. \tag{15}$$

In a recent study of the vertical distribution of the stability parameter (Christensen and Wiin-Nielsen, 1991), the values of σ_0 and δ were determined from a set of globally averaged temperatures for each month of the years 1982–88, inclusive. The static stabilities were calculated at the levels: 925, 775, 600, 450, 350, 275, 225, 175, 125, and 75 hPa, i.e., at 10 levels. Using a regression line technique based on (7), we have determined the two parameters with the result that $\sigma_0 = 0.73$ and $\delta = 2.24$. With these values, we get:

$$q^2 = 1.58 \times 10^{-12}\ \mathrm{m}^{-2}. \tag{16}$$

The value found here should be the proper value to use for the divergence effect in an equivalent, barotropic model.

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