Early Operational
Numerical Weather Prediction
outside the USA

An outline to a history

by

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Dedicated to the memory of Professor James R. Holton, who encouraged me to carry on with my historical research on early NWP

"Personally, I am not impressed by appeals to authority (even such a great authority as Holton) so I hope that you will emphasize physical examples to prove your case rather than quoting textbook authors" (Letter from James R. Holton 10 Nov 1993)

1. INTRODUCTION

The story of numerical weather prediction (NWP) is a great story. It deals with Man's first successful attempt to see into the chaotic future, to predict non-period events. It is to a large extent a story of computers, mathematics and numerical schemes. But the development can not be fully understood unless also non-mathematical and non-technological facts are included. Meteorological understanding, political considerations and human emotions are difficult to quantify, but make the picture complete and the narrative more interesting for readers outside the "NWP community".

The impression can easily be fostered that there is a straight line from Richardson's premature idea 1922 to the start of operational primitive equation models some 40-50 years later. But Man is an impatient animal, reluctant to sit passively and wait. The years 1950-70, when the computers were in their infancy saw many attempts to construct NWP systems using the available resources. Some were successful, others gave up, but many kept struggling just for the sake of it.

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The relation between an historian and a journalist is similar to the one between a meteorological scientist and a weather forecaster: while a historian or scientist mostly can choose the time of their presentation, a journalist or forecaster have to meet certain "dead lines". This "Outline" is less of a comprehensive investigation than a hastily compilation, which had to be ready by 7 May to meet the organisers' deadline. The definition of "early NWP" is vague. It refers to the period "before 1965 or no later than 1970", and deals almost exclusively with non-primitive equation models.

Originally the text was intended to be like an Italian concerto with the accounts of early NWP in Sweden and Britain as "allegros", separated by a Japanese "largo", intermixed with by some minor cadences from other countries. But as often happens in life, art and science; the individual who is supposed to be in command is dragged away by unexpected events, emotions and revelations.

When it became known that I was doing research on early NWP it aroused a lot of interest and encouragement from colleagues around the world. Up to the very last minute before the dead line expired new contributions, circumstances and facts arrived by telephone and email. Luckily already at an early stage I had chosen to abandon ambitions to make a well-balanced report and rather present the findings as they have presented themselves.

To borrow a metaphore from NWP: this is just a "preliminary analysis", based on "early cut-off" observations, which have passed an elementary "quality control" and pitched together with a poor "first guess" using a basic "successive correction" method. The final "Multi-Dimensional Variational Analysis" with much more and better information still lies ahead.

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If I sometimes, over the years, felt like a Mowgli in the jungle of dynamic meteorology it was often George Platzman and Norman Phillips who served as my Bagheera and Baloo.

Söderköping 16 May 2004

Anders Persson

Fig.1 From wheat to bread: A proud Bert Bolin (left) shows an example of a completely automated forecast production to Ragnar Fjørtoft from the Norwegian Meteorological Institute (centre) and George Corby from the UKMO (right). The output of the barotropic forecasts made on the Swedish computer BESK was from 1955 displayed on a cathode ray screen or through a facsimile machine. See Döös and Eaton (1957) and Wippermann (1958) for contemporary details. The picture is taken around 1956. (Copyright Pressens Bild)

2. INTERNATIONALISM AND ENGINEERING: NWP IN SWEDEN 1952-69

2.1 Introduction

The very first operational, real-time +72 h NWP was run in Sweden during a military manoeuv-re in autumn 1954 and operational production started shortly after. This was just two years after active work on NWP had started and half a year before the start of operational NWP in the USA. The success placed Sweden at the forefront of NWP, together with the US, Japan and the UK, a position it would maintain until the early 1970's.

Three factors contributed to the NWP development in Sweden: Carl Gustaf Rossby's return in 1947, the development of state-of-art computers and, not least, international scientific and economic support.

2.2 Why did Rossby return to Sweden?

> "It was to the surprise of many people that Rossby with his reputation steadily growing, should in 1947 return to Sweden to take a chair at the University of Stockholm." R. C. Sutcliffe (1957, 1958)

Why did Carl Gustaf Rossby leave the US to move to Sweden? From those who knew him well there are a multitude of explanations: emotional, scientific, economic and political. It was no secret that Rossby did not go on well with the administrators at the University of Chicago. Others have said that he found life in USA "too hard". Rossby was also known to become restless after several years in one place and liked to change his location and agenda from time to time:

> "Rossby was essentially a restless person. I think that if he thought [he had come] as far as he could in a given line of development of his thought, he would seek stimulation by going elsewhere."(G. Cressman, personal communication, 1992)

Rossby often told his friends that he had a principle of not staying more than ten years in the same place. More important were probably the possibilities that Sweden offered him. It was after the war one of the few countries in Europe that could provide a secure political, economic and scientific base for new projects:

> "[Rossby] felt honoured to be called back to Sweden in such a position that he could decide his working conditions himself. Since he was a restless person, it was always urgent to start new projects. Here arose an exquisite opportunity." (C.C. Wallen, personal communication, 1992)

It seems that Rossby had planned to return home already in the 30's with an intention to work with Vilhelm Ekman at the University of Lund. Ekman was due to retire in 1939, so there might have been an idea to have Rossby replace him. Some leading scientists had approached Rossby to bring him back "home"[1]. These approaches were now repeated.

In 1945 the government initiated a new commission to look into meteorological education. The two commissioners, Harald Norinder, professor of atmospheric electricity from Uppsala and Hans W:son Ahlmann, professor of geography at Stockholm University had good contacts with Swedish meteorologists, in particular with their old friend Carl Gustaf Rossby. Their report concluded that it was important to get Rossby back:

> "Awaiting the retirement of the Director Slettenmark in 1949 it is suggested that professor Carl Gustaf Rossby is acquired as a leading expert, who would be professor at Stockholm University and chairman in the scientific advisory committee, which is regarded as necessary."[2]

Rossby visited Sweden in January 1946 on his return from a visit to the USSR.[3] He and three other meteorologists met Tage Erlander, then minister responsible for education.[4] We know from Erlander's published diary that it was on 19 January 1946. The story is that Rossby had brought with him weather maps, which he

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[1] Jack Bjerknes (1964) and Bert Bolin (1997, 1999). At that time Rossby had just become US Citizen in his quest to become the head of the Weather Bureau. When that post went to Reichelderfer Rossby turned his attention to University of Chicago.

[2] Svenska Dagbladet and Dagens Nyheter 31 January 1946

[3] Svenska Dagbladet and Dagens Nyheter 31 January 1946. Palmén to Taba (1981). Reichelderfer visited the USSR about the same time as Rossby.

[4] Erlander would soon be appointed Prime Minister and serve for 23 years, 1946-69.

displayed on Erlander's desk, over ringing telephones. Erlander had a solid education in mathematics and physics, and was even married to a mathematician. So he was, unusual for a politician, well-informed to understand the scientific problems Rossby laid out for him.

Rossby recommended that a new chair for dynamic or theoretical meteorology should be created in Stockholm. The conversation turned to possible candidates for the professorship. Tage Erlander then suddenly asked Rossby, if *he* would return to Sweden and accept the chair. We do not know from Erlander's diary what Rossby answered, but in a newspaper interview in "Dagens Nyheter" 3 February Rossby expressed doubts about a permanent move, but was quite willing to have a "temporary employment" in the "forthcoming years".

2.3 Meteorology in post-war Sweden

> "Increase the hetrogenity! The Swedish homogeneous group of scientists and students should be mixed up with more foreign guest lecturers and students", Rossby in a speech in Stockholm 2 May 1950 (DN 3 May 1950)

When the Big Bird from the Great Lakes landed in the Swedish pond, he created waves that Swedish meteorology would draw energy from for years to come. His arrival was very timely. Swedish meteorology had started to change to meet the post-war challenges.

This was a stimulating transition period when the international exchange of observations started after the war. It was a time when the peace offered new opportunities, but also new demands. In 1944 an official commission found that SMHI lacked sufficient number of educated personnel. Tor Bergeron, then 50 and still working as a chief forecaster, was promoted to a post at the university and responsible for education. An embryo of an aerological section (with Alf Nyberg as the only member on half-time) was created at SMHI.

In July 1944 the small military section at SMHI was transferred to the Air Force. Under its new leader Oscar Herrlin it expanded into the "Military Weather Central" (MVC). The Air Force was under expansion (it would reach it full capacity in 1946) and there was a desperate need for aviation forecasters. To fill the vacancies retired sea captains were recruited. There were also severe shortages of meteorologists on the civilian side. When the air flights over the Atlantic started in 1945 the aviation authorities had to hire British and American forecasters to make the upper-air aviation forecasts. The problem was raised in the Parliament and the newly created aerological section under Alf Nyberg was further expanded. His staff came almost exclusively from abroad. Renate Schäffer was a Jewish refugee from Germany; Andrzej Berson was a Polish refugee who had worked in Britain. Ernest Hovmöller came from Denmark, Lauri Vuorela and Arturii Similä from Finland.

When Rossby arrived in Sweden in autumn 1947 he resided in the SMHI building. There he gave lectures on dynamical meteorology and other aspects of modern meteorology. One of his main actions was to increase the contacts between Sweden and the outside world. In 1946-47 Alf Nyberg and C-C Wallén (with Erik Palmén from Finland) were given the opportunity to work in Chicago and several visitors such as Charney, Namias, Pettersen, Sutcliffe, Riehl, van Mieghem and others spent shorter or longer times at SMHI. This was a policy that Rossby would continue to pursue during the rest of his life.

The presence of Rossby and his foreign visitors at SMHI couldn't but leave an impact on the Swedish meteorologists. The daily weather briefings, with hemispheric surface and height maps, took an extra dimension when they were attended by the world's leading scientists. Modern dynamical concepts were picked up by the operational forecasters, integrated into their synoptic thinking and applied in fruitful ways in their routine work[5]. Two of these concepts, the barotropic model and the notion of energy dispersion ("downstream development") would play important roles in the future development of NWP.

2.4 Rossby's barotropic concept

> "I had believed that barotropic concepts would be well-adapted in the European area because the climatological situation is characterised by a relatively small meridional temperature gradient,

[5] This applied in particular to Alf Nyberg's aerological section, which gradually came to include also progressive Swedish meteorologists like Karl-Einar Karlsson and Gösta Sjölander. The Central Forecast Office under Herbert Henriksson worked more traditionally along the Bergen School lines.

> and thus the upper air patterns would be responses to the influence of strong baroclinic systems over the Atlantic to the west." Jerome Namias (Roads, 1986)

Rossby's wave equation shows the relation between the wavelength L, the mean zonal flow $\overline{U}$ and the phase velocity c

$$c = \overline{U} - \frac{\beta L^2}{4\pi^2}$$

There was, for c=0, a stationary wave length L_0

$$L_0 = 2\pi\sqrt{\frac{\overline{U}}{\beta}}$$

The main idea was that if there were a discrepancy between the actual wave length and the stationary one, there would be an adjustment towards the stationary. It was a priori assumed that semi-permanent "anchor troughs" over the western parts of the oceans should remain in their positions. This would allow the positions of the non-permanent troughs to be calculated. The equation was originally intended to be use on time averaged fields. Cressman (1948) and others found shown that it actually worked best on daily upper-air patterns.

Rossby's principal synoptic expert, Jerome Namias, visited SMHI for half a year in 1949. He had hardly arrived when the Easter Holidays 14-18 April 1949 was coming up and the media requested a forecast from the SMHI. The management was reluctant go beyond the normal two-day outlook, and passed the puck to the newly arrived "American expert". The weather was unsettled with a strong southwesterly airflow bringing in low pressure systems from the North Atlantic. But for Namias the synoptic situation was "a clear cut type" where the zonal flow would change into a blocked flow and a new ridge of high pressure would soon dominate northern Europe. A forecast about a sunny and nice Easter was given to the press and shortly verified.

This successful prediction once and immediately established the barotropic concept among the meteorologists in Sweden. It was shown to be more than just translation of waves (Lönnqvist, 1949, 1952). This would turn out to have important consequences for the future, since there would be an intellectual readiness to accept forecasts based on barotropic models, even if it seemed to contradict the Bergen School notion about thermal contrasts as the driving mechanism.

Indeed, most synoptic waves are baroclinic, even the large-scale planetary waves. Rossby had, however, made the point already in 1942, and would continue to make it, that his theory was purely kinematic and was not supposed to answer the question of "the ultimate cause of the waves" (Rossby, 1942, p. 1 and 13). The long planetary waves were created by all kind of physical processes and, although not barotropic, may have their motion *kinematically* described as such for some limited time.

But the barotropic concept had more realistic properties to be explored, one of them was the ability to describe what was to become known as "downstream development".

2.5 Downstream development and the Hovmöller diagram

> "Forecasters have been trying to predict the displacement of troughs and ridges from the wave formulae, but they found that their predictors are far from agreeing with the actual displacements. The theory of group velocity may put an end to these trials, and meteorologists may now start to look for the rules of propagation of energy in the atmosphere, rather than the rules of wave motion" (Abdul Jabber Abdullah, 1946, pp.1080-09)[6].

Rossby's wave equation indicated that shorter waves moved faster than longer, that they were dispersive. In summer 1944 Rossby, when visiting ocean wave scientists in La Jolla California, had come to realise that the dispersive nature of his equation could be used to calculate a "group velocity" from which the speed of the energy transport could be calculated. The group velocity

$$c_g = \overline{U} + \frac{\beta L^2}{4\pi^2}$$

yields values much in excess of the phase velocity, indicating that the wave energy traveled faster than the wave. This implied that new waves could be formed downstream, generated by the energy arriving from upstream.

[6] The first known PhD thesis on group velocity was written in 1946 by an Iraqi meteorologist, Abdul Jabber Abdullah at M.I.T. under supervision by Bernard Haurwitz. I am indebted to Edward Lorenz for having suggested this source.

This was to prove important for NWP in a few years time, but at present it caught the attention of the synopticians. Jerome Namias and Phil Clapp at Rossby's department in Chicago had observed how the effect of strong cyclogeneses on the Gulf of Alaska spread over North America and the North Atlantic in two days, and reached Europe in four days (Namias, 1944, Namias-Clapp, 1944)

When Rossby came to Sweden he became aware that Scandinavian forecasters also had noted a similar process, for example how strong anticyclones frequently formed over the British Isles downstream from an intense storm development over the North Atlantic. A Norwegian forecaster Evjen (1937) explained this as a result of huge quantities of air, released during the cyclogenesis and then transported downstream by the upper-air flow.

It was with an intention to link Rossby's theory of group velocity with this synoptic process that Ernest Hovmöller constructed his famous "Hovmöller Diagram": a time-longitude matrix of daily 500 hPa geopotential heights averaged between 30 and 55 N. From such a "trough-ridge diagram" he could visualise the group velocity, which he estimated to be approximately 25-30 degrees longitude per day[7]. This agreed well with theoretical calculations.

In 1948-49 the mechanism of this energy dispersion process was thoroughly investigated and discussed. Platzman (1949) suggested that the purely barotropic component of the upper tropospheric large-scale circulations may perform an important function by operating as channels through which local concentrations of atmospheric energy are permitted to travel in advance of the parent disturbance. The barotropic component would activate latent supplies of energy successively in different longitudes and thus serve as a "perpetual catalytic agent". The reliance on the upper-tropospheric flow as a carrier of influences was very much in line with Evjen's work, but with an emphasis on transport of kinetic energy rather than mass alone[8].

Even if the understanding of the physical mechanism behind "downstream development" was vague among Swedish forecasters, they realized that the barotropic model was not trivial. They knew from experience seen that it worked, so anything derived from it would probably work as well.

2.6 The definition of the computational area

> "[Charney told us] that what really inspired him to develop the equations that later became the basis for numerical weather prediction was a determination to prove, to those who had assured him that the task was impossible, that they were wrong." Edward Lorenz, 1990

If there was a prevailing view among meteorologists in the late 1940s that NWP was not possible it did not rest only on L.F. Richardson's unsuccessful attempt in 1922. The German meteorologist Hans Ertel (1941, 1944, 1948) stated that dynamical forecasting along mathematical lines was only possible if it was performed over the whole globe. For the NWP pioneers this was a crucial issue. Ertel reasoned that even short-period forecasts using the non-divergent vorticity equation were impossible since even small errors would instantaneously spread over the whole area. Due to computational limitations and the geostrophic assumption the computational area could not be global or hemispheric, but restricted to a limited area.

The basic problem was about the maximum speed at which the "influence" of a given source point is propagated and dispersed into the environment. This speed obviously determined the size of the area over which initial conditions must be known. Rossby (1945, 1949 a,b) unified what would seemed to have been two contrasting problems: - *What is the effect of a point source on the entire range of longitude? And what is the effect of the entire range of longitude on a single forecast point?*

Charney had shown that by a filtered approach one would circumvent Richardson's problem. It was also Charney who would show that Ertel's doubts could be disregarded. Drawing on Ross-

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[7] Before Hovmöller (1949) George Cressman (1948) had made the same inference from profiles of longitudinal strips of the 500 mb heights. See Frædrich and Lutz (1987) for a way of measuring the group velocity by Hovmöller diagrams.

[8] Norman Phillips, quoting Rossby has made a similar interpretation: "The initial development occurs through a baroclinic process, in which potential energy is converted into kinetic energy. This would act as a "point source" following which by amplification of disturbances downstream will take place in the wake of the zonal trajectory."(Phillips, 1990, p. 66)

by (1945) Charney[9] showed that, although Ertel was right in principle, for *practical purposes* there was an effective limit to the speed of propagation of disturbances given by the maximum "group velocity", typically 40º per day.

As was demonstrated in the first NWP experiment on ENIAC in 1950, a successful 24-hour forecast could be run only considering an area as large as North America plus parts of the east Pacific and western Atlantic.

2.7 1949-51 The start of international cooperation

> Arnt Eliassen: Rossby had invited many people from various countries. I think he had the idea to reconcile people who had been on each side in the war.
>
> John Green: Gosh! What was it, a political motive?
>
> AE: Ja, partly, I think so at first….
>
> JG: I mean, was he a pacifist, or something like that?
>
> AE: No, not really, not really, I think. He would not be against fighting the war when that was necessary. But he was very much for the friendship and communication between various nations, ja, always… (Professor Arnt Eliassen interviewed by Dr John Green for the Royal Meteorological Society 1984)

Among the several important initiatives Rossby undertook after his return to Sweden one was to establish a new international journal "Tellus"[10]. Before the war there had been two leading theoretical journals in Europe, "Quarterly Journal of the Royal Meteorological Society" (QJRMS) and "Zeitschrift für Meteorologie" (Z. Met). The war had left QJRMS dominating the scene[11]. Just a few years earlier Rossby had started "Journal of Meteorology" (J. Met) in the USA.

Rossby had several intentions with Tellus. Apart from stimulating Scandinavian and in particular Swedish geophysical research, it should serve as a counter weight to QJRMS. Tellus would also serve geophysical research in other countries, for example France[12].

Two years later Rossby took a step further and suggested the creation of a "true" international meteorological institute. Its main objective was to bring together scientists who would otherwise wither in isolation. It would also serve as a bridge between nations in east and west[13]. As with Rossby's move to Sweden his international program can be viewed in different ways: as an altruistic idealistic idea, as a way for Sweden to extend its resources and influence and as a bridge for American influence over European meteorology.

At the UGGI meeting in Brussels in August 1951 Rossby presented his plan for "one or several regional institutions with rotating, overlapping staff, to which scientific workers from different countries may be sent or invited for participation over limited periods of time." The proposal failed, partly due to French and British objections. The French had already suggested something to similar to Rossby's idea in 1923. The French Director André Viaut told that there were now plans to erect such an international institute just out-side Paris.

The negative British attitude can not only be explained by their general foreign policy at the time. Rossby had made no secret of his vision:

> "Basic research institutions of this kind must be built around individuals, not set up as paper schemes. We are faced with the necessity of creating, not only facilities, but also leadership for joint research tasks." (1951 UGGI Protocoll, p. 27-28)

Not only the British delegation, but also others in the traditionally conservative meteorological

[9] Charney (1949) was a revision of a memorandum to John von Neumann in autumn 1948 presented at a meeting of the AMS in New York 28 January 1949.

[10] Tor Bergeron designed the logo, which was slightly changed in 1962, and again in 1970 when Tellus became bi-monthly.

[11] Other journals of meteorological interest in the post-war years were Monthly Weather Review (USA), Meteorological Magazine and Weather (UK), La Météorologie (France), Geophysica (Finland), Geofysiske Publikasjoner (Norway).

[12] Rossby remarked that French and British scientists would never publish in each other's journals. He never published anything himself in QJRMS. His 1940 QJRMS paper on long waves was published by the *Canadian* branch of the Royal Meteorological Society. Tellus' subtitle "A Quarterly Journal of Geophysics" was perhaps not only meant to inform the reader that the journal would appear every three months.

[13] Rossby had always been keen to stay in contact over political boundaries. He never broke off his relation with the German Hans Ertel, neither during the Nazi epoch or when Ertel sided with DDR. He also had good relations with Soviet scientist and has directly influenced a generation of meteorologists in Communist China. At the same time he had excellent relations with the United States, especially its Air Force!

establishment might have been reluctant to let Rossby at large in the heart of Europe[14].

2.8 1951: Preparations for NWP in the aftermath of ENIAC 1950

It was in the November 1950 issue of Tellus that the results of the successful ENIAC experiment were published and reached the readers in spring 1951 (March in Europe, May in the US). What initially had been considered an experiment to test numerical techniques had turned into a major meteorological breakthrough[15]. From this two different conclusions were drawn which would have far reaching consequences for the future:

Charney wanted to press ahead further in his hierarchy with the work on baroclinic models from the natural assumption that if a simple barotropic model could perform that well, wouldn't then a baroclinic model perform even better? Rossby on the other hand wanted to remain with the barotropic model and explore its potential further.

Although the development in Sweden and the US took different paths they would interact and mutually benefit from each other.

For Sweden the negative UGGI attitude was a blessing in disguise, since Rossby, not discouraged, started to prepare for the centre to be located in Sweden. European and American scientists were invited. An arrangement was made with US Air Force Geophysics Research Directorate (GRD) in Cambridge, Massachusetts. Several US Air Force officers visited MISU for one or two years[16]. Several organisations in the US established semi-permanent positions, which were changed annually, or every second year. In general non-permanent staff were invited for 3, 6 or 12 months.

> "No sharp line of demarcation is maintained between teacher and graduate student. It is rather expected that each new visitor will profit from, and add to, the intellectual capital at the institute."(Rossby, 1953)

It was decided to make an adequate technique for numerical weather prediction one of the principle fields of research at the expanded and reorganised Institute of Meteorology of the University of Stockholm.

In 1951 Bert Bolin (Taba, 1988b) spent half a year at the Institute for Advanced Study in Princeton. There he finalised his work on the effect of mountains on the atmospheric flow. Together with Charney he calculated manually 12-hour 500 hPa tendencies for 12 hour intervals 3-13 February 1951. The ENIAC runs had used a 736 km grid, but Charney and Bolin used 315 km over 18 x 15 grid points. The hand-calculations took four to five hours for each case. The results were positive, the calculated tendencies correlated 0.74 or 0.77 against observed tendencies depending on routine or carefully analysed charts were used.

The work at MISU in autumn 1951, in Bolin's absence, was characterised by attempts to go through the main part of the work that had recently been done in the field of NWP. There were seminars by members of the group: Eliassen, Hinkelman, Arnason and Clapp. In particular Hinkelmann's lectures were regarded as extremely good and he was considered as one of the most inspiring persons at the institute.

2.9 1951 The choice of NWP model

> "Purely barotropic processes define the the stage upon which the thermal play is performed." (Bert Bolin in a memo to the Military Weather Central, late 1951)

It was when Bolin came back in autumn 1951 that the work on the Swedish NWP really started[17]. At his return Bolin gave about 25 lectures, not only at MISU, but also elsewhere. In an internal memo he provided the arguments to restrict the work to the barotropic model. Instead of starting from the general atmospheric equations and simplify them *mathematically* in order to make them numerically solv-

[14] The "International Meteorological Institute" (IMI) was nevertheless set up in 1955, financed by grants from the Swedish, American, Belgian and other governments.

[15] "There is no doubt that it was Rossby who inspired Jule Charney to use the barotropic vorticity equation prognostically. It was also Rossby who was giving Jule Cahrney the idea to develop the quasi-geostrophic theory. Actually, he mentioned this to me in one of our many discussions. For sure, however, I know he would never make any claim about this. He was much too generous to do such a thing. (Bo Döös, personal communication 1992)

[16] There were also some visiting scientists from the US Navy, such as Dan Rex, William Hubert and Max Eaton.

[17] He was replaced by Ernest Hovmöller, who spent the first half of 1952 at the University of Chicago, the second half at IAS.

able, one should first simplify the problem *physically* into a model of the atmosphere which could then be described by more solvable mathematics[18]. The physical model was a thin 2-dimensional incompressible fluid of constant temperature and density, called the barotropic model. Here it becomes important to make a distinction between two types of barotropic models.

The model Bolin obviously was referring to is what later became known as the divergent-barotropic model: a thin, incompressible fluid with a free upper surface, which would be able to move in the vertical as a reflection of the diverging or converging motion. The motion was in principle described by the conservation of potential vorticity[19].

But the model that was the basis for the 1950 ENIAC forecasts, the 1952 tendency calculations and the subsequent Swedish NWP system was based on conservation of *absolute* vorticity. Physically it meant that a lid had been put to constrain the upper surface.

But this model was actually an integrated part of a *baroclinic* model of a peculiar kind, suggested by Charney. In this baroclinic model the wind was assumed to be constant in direction with height in the vertical – and remain so. The speed, however, varied linearly from the bottom of the troposphere $p=p_0$ to the level of maximum level $p=p_1$. Further up it decreased to zero at $p=0$.

Applying further constraints such as conservation of potential vorticity and potential temperature for individual air parcels, it is possible to infer a vertical motion as a transport mechanism to fulfil the constraints. These vertical motions will peak at some uniform level p^* between p_0 and p_1. No air is sucked in or pushed out, i.e. the divergence is zero. At this level of non-divergence a simplified version of the equation of motion, the *conservation of absolute vorticity*, can be applied in a so-called "equivalent barotropic" model[20].

The decision to stay with a barotropic model came from both theoretical and practical considerations. Limitations of the number and accuracy of observational data, especially over the oceans would make it very unrealistic to try to apply a detailed three-dimensional model of the atmosphere over such an area. To this came the limited capacity of the electronic computers.

The numerical schemes still left much to be explored, problems of orographic and thermal forcing had just started to be worked on and - last but not least - the problem of objective, automatic analysis still remained to be solved.

As Hinkelmann mentioned in his WMO-Bulletin interview (Taba, 1985c):

> "-My impression [in 1952] was that Rossby was coming to the conclusion that he had exhausted all the possibilities of barotropic models, but he was right in insisting that before going on to baroclinic models, we must have thoroughly mastered the use of barotropic equations..."

From a practical point of view it seemed as essential to improve the barotropic model as to introduce new physical factors that, though important, would also increase the computational work considerably (Staff Members, 1952).

[18] Bolin in an undated memo, probably from 1951. This was in line with the "Rossby Schools'" general physical approach to the meteorological science (McIntyre, 1951)

[19] Bolin and Charney (1951) invoked G.I.Taylor's classical experiment from 1921, which demonstrated that the relative motion in a rotating fluid became quasi-similar in planes perpendicular to the rotation axis.

[20] Some, such as Burger, did not find this convincing. The power of the conservation of absolute vorticity had been known for long. Starr had made graphical NWP during the war based on this concept. Charney's derivation was a justification for an already trusted concept.

The equivalent barotropic model – a qualitative explanation

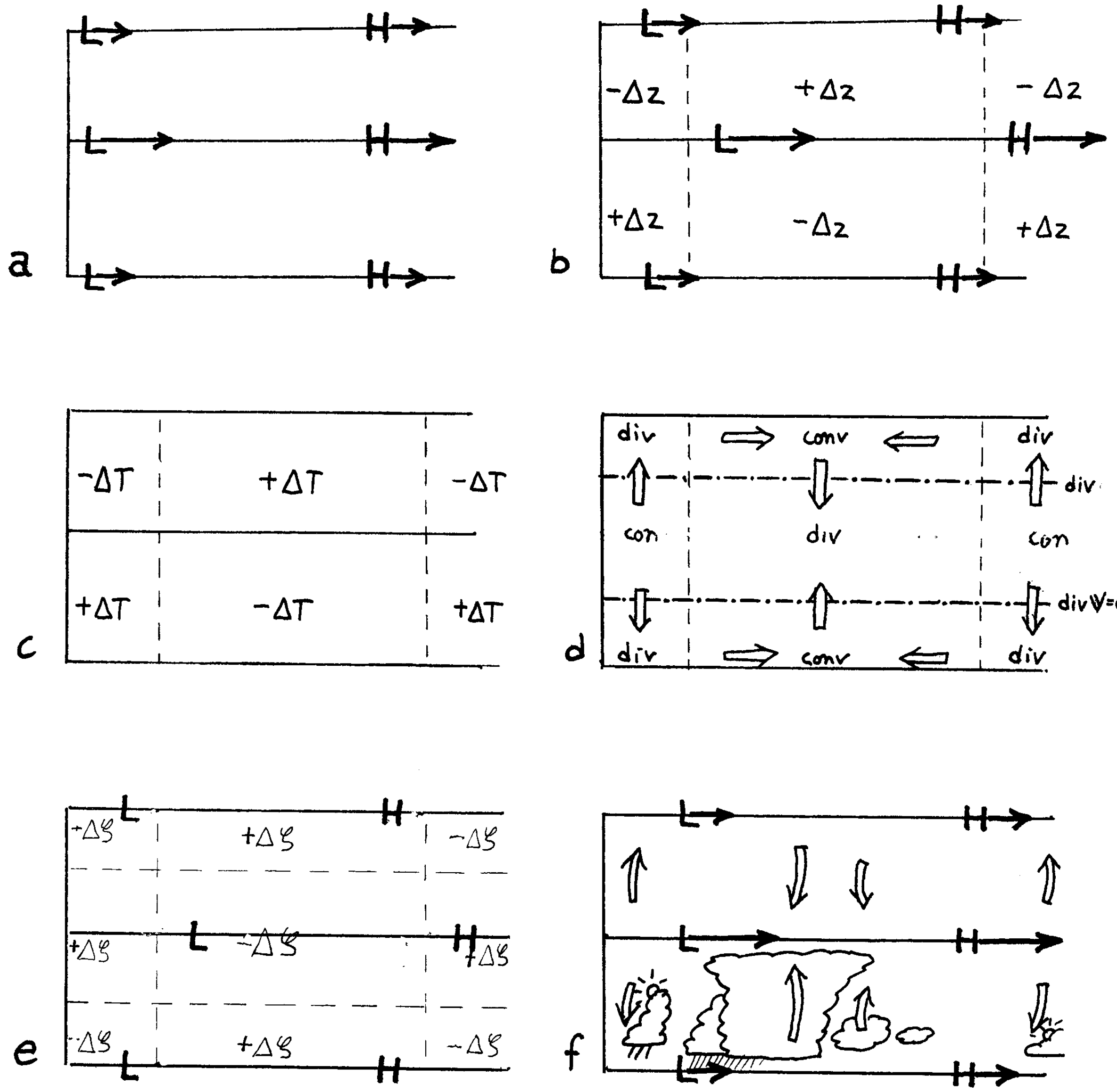

Fig.2: In an equivalent barotropic model the wind direction is constant with height, but varies vertically (a). During a short moment of time the flow patterns is advected downstream, most rapidly in the jet level (200-300 hPa). Assuming geostropohy this leads to changes in the thickness fields (b). Assuming hydrostatic balance this implies thermal change (c). Since thermal advection is not possible (wind direction constant with height) only vertical motion in a stable stratification can accomplish thermal changes (d). Assuming conservation of potential vorticity the vertical motions induce changes in vorticity, which counter the progress of the flow patterns in the jet level and speeds up the slower levels at the bottom and top (e).

Eventually all troughs and ridges move with the same speed with a vertical velocity field transporting the "necessary" vorticity vertically. From (d) follows that there are two levels of non-divergence, one between the jet level and the surface (600-500 hPa), the other above the jet level, around 150 hPa, as verified by a young Hessam Taba (1959). In these two levels of non-divergence the simplified equation of motion, the conservation of absolute vorticity can be applied.

2.10 Swedish computers

> "The speed of the [ENIAC] computer is phenomenal." (From "New Electronic Computer", article in BAMS April 1946)

On 27 July 1946 a fire raged in Gothenburg harbour. Among the losses were electronic instruments and other devises, which belonged to technicians from Chalmers Institute of Technology. One of them, Stig Ekelöf, had just returned from a fact-finding mission to the US. He had studied advanced technical laboratories on behalf of the Swedish military[21]. There was a great interest in Sweden after the war, in particular in military circles, to acquire a computer system. During and after the war it became known that fast computing machines were under construction at various institutions in the USA.

Ekelöf's loss was, however, to some degree compensated by the novelties that he had been exposed to on his tour. On 17 September he reported to the military, scientific and commercial authorities on what he called "super calculators" – the Swedish language still lacked a word for "electronic computer". Swedish military authorities considered acquiring a similar machine, both for engineering purposes, code breaking, ballistic calculations and other purposes.

The code breaking application was important to the FRA (Försvarets Radioanstalt or National Defence Radio Establishment) comparable (except in size) to the American National Security Agency (NSA). Head of its decoding department was Åke Rossby, younger brother to Carl Gustaf[22]. Both brothers were present at a meeting with SBCM 4 October 1947 when the possible use of "mathematics machines" was discussed and the application in meteorology was mentioned.

The parliament granted money and in May 1947 five young experts were sent to the US, one to Norway and two, Stig Ekelöf and a colleague to England. In spring 1948 Ekelöf returned to the USA to find out more and investigate the possible purchases. But at the end of October 1948 the US State Department made it known that export licences of "certain relay machines or details" could not be granted. They were of "highly strategic significance". This was part of a general policy, not only directed towards Sweden.

The American refusal to sell was a blessing in disguise for Sweden. In 1948 a project to build a Swedish machine started. "The Swedish Board of Computer Machinery[23]" (SBCM) was founded to direct the work.

The first computer BARK ("Binary Automatic Relay-Calculator") constructed by the SBCM became operational in spring 1950. BARK was constructed after American prototypes, like MARK I from Harvard University. It was able to perform calculations in 40 hours that would have taken 1 800 hours to perform manually. During 1951-1952 BARK was heavily in use, mainly for military purposes, but also for research and industrial applications.

The second and improved Swedish computer, financed by the SBCM was BESK (=Binary Electronic Sequence Calculator) planned to become operational in 1953. The construction was influenced by John von Neumann's Princeton machine. It had at the beginning an internal memory of 25 640 binary digital numbers, but its capacity was expected to increase later with a magnetic drum memory. Compared to what was available elsewhere in the world at that time it was comparatively fast (about 10 000 MIPS). Its memory (William tubes) was very limited, only 512 40-bit words. It would permit only a very small forecast area (20 x 20 grid points with a 300 km grid

[21] The last time he had been to the USA was in 1939. Now he had re-established some of his pre-war contacts at MIT and Harvard.

[22] There have been suggestions that Carl Gustaf Rossby kept his brother informed about American progress in the computer technology. A retired FRA officer who had worked for Åke Rossby told me: -Information about the American progress within computer technology with applications for decoding came through other channels to FRA in autumn 1943 than from Boris Hagelin [a Swedish manufacturer of decoding machines] and C.G.Rossby. Both were then in the US with good contacts, but I do not think they provided any useful information during the war, although they were contacted. They were [contacted] after the end of the war and C.G. Rossby could then give certain information about the use of computers within the meteorological field. Between him and his brother Åke, head of the decoding department at FRA, related matters were discussed with great interest and Gunnar Berggren's [from the FRA] journey took place with some participation by C-G even if official channels were responsible for the final arrangements (Sven Wäsström, personal communication 1992). Other sources claim that in 1942 FRA received "for the first time an exact information that the US had constructed rapid calculators".

[23] Both the Swedish name and the translation give the misleading impression that SBCM was some sort of "committee", when it in fact was a state-owned technological enterprise.

length). For each time step (1 h) it was necessary to feed in the entire machine programme (the Jacobian, the Liebmann and the extrapolation programme).

2.11 1952 The NWP work starts

> "-It takes on average ten years to educate a good forecaster according to the current system, because it depends so much on practical experience and a more or less subconscious ability to judge, which can never outperform a computer. The new system will be simpler because the personal element falls away." Rossby 17 April 1952 in an interview in Dagens Nyheter

A truly international group had now assembled at Rossby's institution: G. Arnason (Iceland), B. Bolin and E. Hovmöller (Sweden), Ph. Clapp, W. Hubert, C. and Harriet Newton (USA), A. Eliassen (Norway) and K. Hinkelmann, E. Kleinschmidt, H. Schweitzer and Christa Steyer (Germany). They thought it would be a good training to repeat Bolin and Charney's tendency computations, but this time over Europe and eastern Atlantic. 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 energy, patience and ingenuity[24].

In March 1952 they had completed fourteen cases of 12-hourly tendency calculations from November-December 1951. The grid length was 310 km at 50º N over an area covering Europe, the North Atlantic to Greenland and Labrador. To explore the effects of the constant boundary conditions the calculations were made in two versions: one with zero tendencies at the boundary, the other with observed. The verification showed an overall correlation between forecast and observed tendencies of 0.69 for both sets[25]. The conclusion was that boundary influence in most cases was negligible at a distance of more than two grid points (620 km) from the boundary.

Verifications were also made against high-quality British radio sonde measurements. The results were much more encouraging with an average of 0.81 (Staff Members, 1952)

In an interview in "Dagens Nyheter", a major Swedish newspaper, 17 April 1952, Rossby could confidently present "a new meteorological method" to make more accurate and "some time in the future" also more extended forecasts. The new method excluded the element of subjective calculations (”guesswork, but guesswork based on long experience"), which the current meteorological prediction of the upper-air flow demands. He also outlined his plan for the coming 2-3 years:

1) During the rest of 1952 the group would be busy with formulating a simplified mathematical model of the atmosphere

2) In the winter 1952-53 a numerical program would be developed for running on BESK

3) In spring 1953 experimental runs would start on BESK

4) Operational forecasting by the "official weather service" would probably start in 1954

As it would turn out, the readers of "Dagens Nyheter" got an accurate 2½-year prediction.

As a preparation for the work it was important to gather experts from around the world. During the coming twelve months three conferences were to be held in Sweden and neighbouring Finland on NWP. Ragnar Fjörtoft (1952), Eric Eady (1952) then at MISU, Hinkelmann and van Mieghem attended a conference in Stockhom in May 1952. Neovius and Kjellberg presented BARK and the plans for BESK (Bolin and Newton, 1952)

A second conference, devoted to NWP, cloud physics and atmospheric chemistry, was held at 13-18 October 1952 in Stockholm (Rossby, 1952, Tellus, 1952). Among the attendees were Charney, Lingelbach, Arnason (1953), Eady, van Mieghem and Sutcliffe. According to a newspaper report it came as a surprise to the audience that Britain had computers available for meteorological forecasts (DN 14 Oct 1952).

On 18-21 May 1953 a third conference was held in Helsinki covering a wide range of

[24] When I studied meteorology in the mid-1960's Bo Döös let us do exactly this: barotropic tendency calculation over 12 hours with a 10 x 10 or 12 x 12 grid with 300 km apart. It took a long time, but is one of the highlights in my meteorological education! The solution showed an eastward motion of a low.

[25] The average of 0.69 seems to be a mistake. A re-calculation of the average scores yields an overall tendency correlations of 0.62 for the constant and 0.66 observed boundary conditions respectively. The RMSE was reported as 58 m for both cases. A re-calculation gives 51 and 52 m.

Fig.3: Scientific opponents – good friends. R. C. Sutcliffe's and C. G. Rossby's acquaintance dated back to 1944 when Sutcliffe made an informal visit to University of Chicago (Vincent Oliver, personal communication). This picture is taken at the October 1952 conference in Stockholm.

Fig. 4: Rossby and part of his international team in 1952. From left to right: William Hubert (USA), Lauri Vuorela (Finland), Christa Steyer (West-Germany), Carl Gustaf Rossby, Eric Eady (Britain) and Jacques van Mieghem (Belgium). The headline reads: "New model for weather forecast"

topics, most of which were not directly related to NWP. A young Erik Eliasen from Denmark made his debut with a presentation on correlation functions for wind observations in the free atmosphere and Arnt Eliassen discussed the demands on the aerological network.

2.12 1953 Pre-operational runs on BESK

> "- The first steam boats were not particularly fast, but the invention of the steam engine implied possibilities which first after very long time would be possible to utilise. Besides, the sailing ships, in competition with the steamboats, made progress, not imagined beforehand. Let us say that the mathematical machine is our steam engine." Rossby in an interview in "Svenska Dagbladet" 30 Dec 1953 and to Pierre Welander (personal communication 1994)

During 1953, waiting for BESK to become operational, the group put the barotropic model into a numerical code. The key figures in this undertaking was Germund Dahlqvist and Norman A. Phillips. Dahlqvist was in his late 20's and got involved in 1952 when he worked at the SBCM. He attended a seminar on numerical weather forecasting by Bolin who in the ensuing discussion informed him about the plans to use BESK.

Norman A. Phillips was an equally young American meteorologist of Swedish heritage[26]. Originally planning to become a chemist he had worked as a forecaster at the end of the war on the Azores. After the war he became engaged by the meteorological department at the University of Chicago. He was not only a good theoretician and mathematician, but also an able synoptician. When he arrived in Sweden he saw that they were making rapid progress and that the work on the computer BESK was almost completed[27]. He was busy advising them to use less complicated and more stable numerical schemes. Dahlqvist:

> "Norman Phillips taught us much. Among other things he got us to disregard mnemotechnical notations for operations and other general programming aids, which we had intended to develop and instead write the program directly in hexadecimal machine code. This was the only right thing to do for a project of this type, where one was forced to apply all sorts of tricks to be able to store the problem in BESK."

The possibility of being overshadowed and outperformed by the European groups undoubtedly provided motivation for different American centres to put their disagreements aside. In July 1953 the Joint NWP Unit (JNWPU) was founded with George Cressman as Director[28].

BESK became operational in December 1953. The opening was dominated by meteorological applications. One newspaper, the tabloid "Expressen", ambitiously showed a map over Europe with the forecast and observed 24-hour changes of the 500 hPa geopotential field (fig.2). Norman Phillips told the journalist that "the Swedish machine is faster than the one we have in Princeton". In another interview BESK's constructor, Erik Stemme agreed that the machine could calculate faster than the American machines, but was slower in providing the output.

The first cases were completed before Christmas 1953. The group sent Rossby a wire to Capri to tell him that this milestone had been passed. On his return he was enthusiastic:

> "This is not even the first step on the way toward a more scientific weather forecast, it is at most a tenth of a step. The skill, which we will achieve initially, is perhaps overall not greater than what a not too experienced forecaster can achieve using the traditional methods."(Sv. Dagbladet 30 Dec 1953)

Phillips left in January 1954 and spent February in Norway. He was replaced by Bo Döös (Taba, 1997 b). Leader of the BESK runs was Major Harold (Art) Bedient, US Air Force, who arrived in Sweden in autumn 1953. He

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[26] Phillips' grand-parents had all come from different parts of Sweden during the second half of the 19th century: Victor Pettersson (Öland) and Anna Hjertstedt (Östergötland) had the sin Alton, Fritz Larsson (Dalsland) and Eva-Lena Salmén (Västergötland) had a daughter Linnea Marie Larsson. Alton Pettersson was a carpenter in Chicago during the 1920's, wanted to expand his market also to non-Scandinavians and thus changed his name to Phillips. Norman took the opportunity during his five months stay to visit relatives. "I could speak Swedish, albeit haltingly, having studied it on Berlitz records early in 1953. Phillips left in January 1954.

[27] Perhaps it was because Phil Thompson visited Sweden before the BESK became operational that his report was so far off the mark. He wrote that the Europeans were six months behind in theory and 1-2 years behind in operational applications. Rossby's group "professed to have no definite plans for operational applications but have the capabilities for putting numerical methods into practice by early 1955."

[28] See Kris Harper's text on the background to JNWPU.

learned about the practicalities of NWP through self-studies and manual calculations. It was Bedient who developed the "zebra plots" of output fields together with Dahlqvist[29].

A second milestone was passed in two runs 23-24 March 1954 when Harold Bedient and Bo Döös ran the first real time NWP which verified 90 minutes before the verification time (03 GMT). Another telegram went to Rossby in the USA: "First operational NWP carried out last night".

2.13 1954 Towards operational runs

The BESK calculations of twenty-four 24-hour barotropic forecasts were summarized in a report (Staff Members, 1954) that was sent to Tellus in May 1954 by a group consisting of G. Arnason and P. Bergthorson (Iceland), H. Bedient and N. Phillips (USA), B. Bolin, G. Dahlqvist and B. Döös (Sweden). The maps had been analysed by W. H. Hubert and Ch. Newton (USA) and L.Vuorela (Finland).

The results were better than previous tests had indicated with an average tendency correlation of 0.77. Errors were mainly due to the assumption of constant boundaries, analysis errors over the N Atlantic, poorly resolved small scale features and baroclinic developments. The group "hoped" that in future runs the effect of the boundaries would not propagate far into the area[30]. In a few cases the forecasts were made on a real-time basis to gain experience for routine forecasting. They found that the whole production would take 6-7 hours distributed according to the following list

Checking and plotting data	1h 30m
Analysis of 500 mb map	1h 30m
Reading values in grid points	1h 20m
Punching of input data	40m
Checking and correction	20m
Machine tine for forecast	40m
Plotting and analysis	30m
Total time	6h 30m

This test of numerical integrations indicated clearly that the results of the 24 hour forecast could be used operationally, if prepared in time. In order to reduce the negative impact of the constant boundary conditions it would be important to run on a larger geographical area. This increased the need to find ways to reduce the preparation time for the forecasts.

Rossby reported in June 1954 to Charney about the positive results and revealed that they were preparing to make operational 48-hour forecasts later in autumn. To the Americans this confirmed Smagorinsky's report in March 1954 that the Swedes and British anticipated making daily operational predictions within six months.

By the time the American JNWPU was formally inaugurated Rossby's group was on the edge of operational implementation. The group hoped that by increasing BESK's memory after incorporating the magnetic drum they would be able to extend the forecast area considerably, to 31 x 55 points, and allow longer forecasts. It would then also be possible to extend the forecasts to +48 and +72 hours and investigate "the breakdown of the barotropic model (or of the computational scheme!)" Páll Bergthórsson wrote to Rossby, 11 August 1954:

> "-The other day I gave a little radio talk about this subject, insisting that the results in Stockholm were quite comparable with conventional methods, in spite of the fact, that many important factors were neglected. There is every reason to think that a gradual inclusion of these factors together with improvement in observations will bring out a real progress in meteorology, I said. I am sure it is of great importance to inform just the man in the street about these projects. He is often aware of the shortcomings of the weather forecast, and if he knows that it is possible to improve the weather service, he will raise his demands, and there are many men in the street!"

BESK was powerful but expensive. To run an hour would almost cost the same as a monthly salary of a civil servant. So who would be willing to pay?

[29] Bedient left Sweden in spring 1954 to join the JNWPU.

[30] Inspection of the maps in the article shows that errors due to constant boundary conditions spread from the western boundary (from Hudson Bay and Central Atlantic) to the east with 30º per day or 25-30 m/s.

2.14 SMHI versus MVC

-Is it you or I who will take the greatest risks? (Rossby to a nervous Germund Dahlqvist)

At this time Rossby had no economical support of SMHI. They had no immediate prospect of finding the money that would be needed (Taba, 1984, Nyberg, 1975). They were not yet quite convinced about the realism of the project[31]. The official line of SMHI was that basic research was the task of the universities. SMHI's rôle was practical implementation. To Dagens Nyheter SMHI Nyberg said that they "for the time being" wanted to await further results and that they had no immediate plans to "engage BESK as meteorologist". The meteorological professors Hilding Köhler and Tor Bergeron were not devoted to NWP, nor was the SMHI Director (since 1949) Anders Ångström and Erik Palmén.

In this situation it was natural for Rossby to approach the other major weather service in Sweden. The Air Force and its Military Weather Central (MVC) had already contributed to the experiments. Its Chief, Oskar Herrlin, was very much a self-made man, with a non-academic and "practical" education in meteorology. The management of SMHI looked down upon him (he had once served there in a low rank position). But Herrlin was no fool and an ambitious man. He had few possibilities to grasp the complexities of dynamic meteorology and numerical mathematics, but he felt that the progress made in the technique of 1-2 days forecasts had been very small. The development appeared to have become almost stagnant and the conventional techniques had been literately squeezed dry:

> "Under those circumstances it is not very encouraging to be the spokesman for meteorology within the armed forces."

Since Sweden was a small country neither personnel nor money could have changed the situation to any considerable extent, as shown by the experience of the larger countries. It was in this pessimistic mood that Herrlin was approached by Rossby in winter 1953-54. Rossby told him that he was convinced that a practical program for numerical forecasts of the 500 mb surface was now available. Herrlin and his staff were eager to develop and test the system and make it into a general routine. Rossby's persuasive talent combined with positive signals from Herrlin's American colleagues convinced him that NWP had a future.[32]

Compared with the overall military budget, the cost of a numerical prediction programme for the Weather Service was not too significant[33]. Herrlin had no problems to convince the newly appointed commander of the Swedish Royal Air Force, Axel Ljungdahl[34], that the project was desired, even necessary.

In March 1954 Bert Bolin made arrangement with the USAF[35]. The aim was to explore the possibilities of making forecasts over regions where there were no observations, in particular over the Atlantic. But it would of course also apply to enemy territory during a war. It would for example be crucial for the Swedes to make forecasts of the air pollution in case of a nuclear attack on Leningrad.

A decisive test would take place early in the autumn. Also the Americans were present. In August 1954 Brig. Gen. Th. Moorman, USAF visited Rossby's institution.

[31] In a conversation in 1992 Alf Nyberg told me that Rossby had a lot of ideas, most of which were unrealistic. This was also Rossby's own view. He used to say that it was enough that one idea out of ten worked out. At this time (1954) he also had a campaign for cloud seeding and weather modification.

[32] The alliance between Rossby and Herrlin probably was an important factor to the early start of the project. Rossby was eager to put his theories to test and Herrlin realized the operational potential of the new methods. "[Their] personal involvement and enthusiasm had a significant impact on the whole project. It convinced everybody who was involved that we were doing a meaningful job. Not only we who were just starting our careers but also our older colleagues were highly motivated". (Söderberg to Bushby, 1986).

[33] Sweden was not a member of NATO but still had one of the world's most powerful air forces

[34] It is not clear when the Air Force was contacted. Ljungdahl is mentioned as the man who gave Herrlin the green light, but Ljungdahl took over after the previous commander Carl-Axel Nordensköld as late as in July 1954. Nordensköld might very well have supported the idea, since he also had a positive attitude to the advancement of meteorology. During the war he had supported Alf Nyberg in his struggle to modernize SMHI (Nyberg, personal communication).

[35] This had been discussed between Herrlin and Rossby in February. An agreement was reached between the Air Force and Rossby's International Meteorological Institute at the University of Stockholm, whereby a number of Rossby's collaborators and students were designated to work for the Air Force for a period of one year.

2.15 1954 The operational start

> "-Perhaps it will start a revolution - I hope so, and the time is ripe!" (Oscar Herrlin 19 November 1954 to the High Command of the Swedish Air Force)

From 21 September to 2 October 1954 a large military manoeuvre, "Dalamanövern", the largest since 1944, was due to take place in central Sweden. 45 000 soldier would participate and protection against nuclear warfare would be tested for the first time. Forecast of the upper air movement would be of crucial importance. During summer 1954 preparations were made to run operational real-time NWP up to +72 hours. Some high placed "old timer" at SMHI publicly expressed the view that trying to make weather forecasts with "data machines" was an irresponsible use of governmental funds, a game a professional service should not get involved in.

The attitude at SMHI was also influenced by the imminent appointment of a new Director General for SMHI, after Anders Ångström, who was due to retire. The two main candidates were Oscar Herrlin and Alf Nyberg, the former supported by Rossby, MISU and the MVC, the latter by the rest of the Swedish meteorological community. The real-time barotropic forecast experiment was drawn into a political struggle between MVC and SMHI [36].

The outcome of the operational trial was a splendid success, operationally, scientifically and politically. Comparisons with independent purely subjective forecasts (which only went up to 48 hours) showed that the NWP was clearly better with tendency correlations of 0.85, 0.82 and 0.70 for the one, two and three day forecasts. On at least one occasion the NWP forecast provided information of crucial importance (Bolin, 1955).

Soon after the manoeuvre the Swedish government reached its decision and appointed Alf Nyberg as the new Director of SMHI. With the political air clear, a successful test paved the way for operational activity in December.

On the 19 November, Oscar Herrlin held a talk for the Air Force High Command. He showed the impressive forecasts made during the manoeuvre and expressed his firm belief that NWP was the first step in a new development of great importance for the forecast technique:

> "-The development within [weather] forecasting has, as I see it, and also many others, more or less got stuck... I have also during the last years come to the conclusion that a further increase of the correctness simply is impossible with conventional methods - in any case not significantly and for reasonable costs… We hope now that the numerical methods, which also have the advantage to start from a good initial state, will be able to lead us out of the cul-de-sac we have ended up in." (Herrlin, 1954)

He then described how the NWP was produced. The finale is somewhat emotional:

> "This project which is watched by so much international interest and which in particular American press written so much about, that they believe the computations are a daily feature, yes, we will be the first in the world, driven by practical needs, and we are proud of that; poor as I said, but proud....

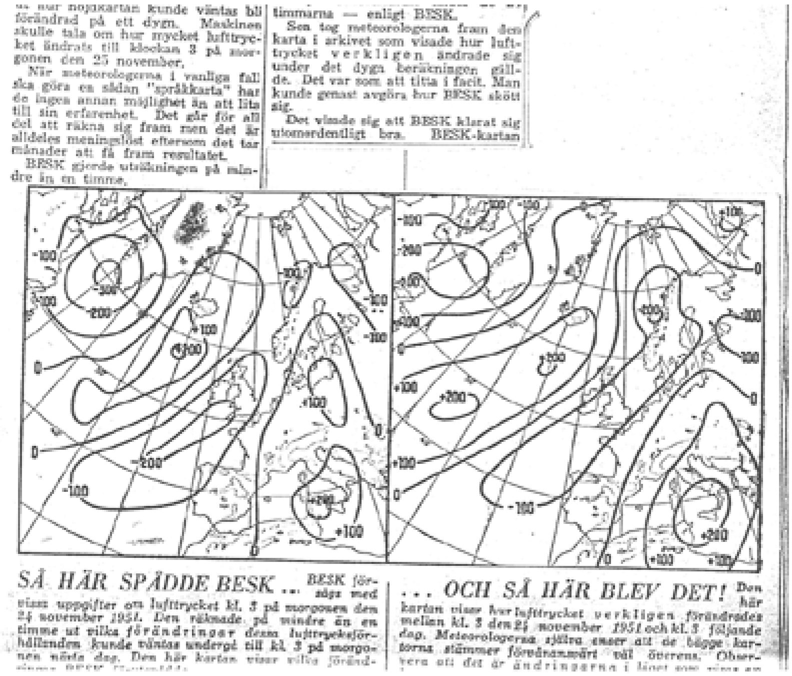
Considering all approximations the horizon of the NWP appeared to Herrlin as rather "hazy" and the road ahead "jumpy and full of slopes". But he was confident that during the winter they would, with a military metaphore, "reach the first hill, and from the next and the next and the next until the horizon becomes more free":

> "100 years ago an important meteorological-historical-practical step was taken by the French Navy[37], the next will be taken by the Swedish Air Force - and that is a great pleasure."

The mood was different at the SMHI. Ernest Hovmöller wrote to Platzman 19 December

[36] Some of the intrigues are described in a satirical song to a popular tune: "-In the corridors and offices to the sound of rustling papers// there was whisper, there were chats, and tittle and tattle //-Will it be him? -That bastard? -Will it be him?// There were roses, there were rods over the favourite //-He is incompetent, -He is the obvious choice, damm" //-And he is willing, if only he gets a chance" (see appendix 4 for the full text).

[37] Herrlin alludes to the investigation by Leverrier in 1855 by order from the French government after the British-French Navy had been destroyed by a vigorous storm in the Black Sea during the Crimean War 1854-55.



...hur höjdkartan kunde väntas bli förändrad på ett dygn. Maskinen skulle tala om hur mycket lufttrycket ändrats till klockan 3 på morgonen den 25 november.

När meteorologerna i vanliga fall ska göra en sådan "spåkarta" har de ingen annan möjlighet än att lita till sin erfarenhet. Det går för all del att räkna sig fram men det är alldeles meningslöst eftersom det tar månader att få fram resultatet.

BESK gjorde uträkningen på mindre än en timme.

...timmarna — enligt BESK.

Sen tog meteorologerna fram den karta i arkivet som visade hur lufttrycket verkligen ändrade sig under det dygn beräkningen gällde. Det var som att titta i facit. Man kunde genast avgöra hur BESK skött sig.

Det visade sig att BESK klarat sig utomordentligt bra. BESK-kartan

SÅ HÄR SPÅDDE BESK ... BESK försågs med vissa uppgifter om lufttrycket kl. 3 på morgonen den 24 november 1951. Den räknade på mindre än en timme ut vilka förändringar dessa lufttrycksförhållanden kunde väntas undergå till kl. 3 på morgonen nästa dag. Den här kartan visar vilka föränd-...

... OCH SÅ HÄR BLEV DET! Den här kartan visar hur lufttrycket verkligen förändrades mellan kl. 3 den 24 november 1951 och kl. 3 följande dag. Meteorologerna själva anser att de bägge kartorna stämmer förvånansvärt väl överens. Observera att det är ändringarna i ...

Fig. 5: The first 12-hour tendency calculations on BESK grabbed the headlines of the popular press. The daily tabloid “Expressen” even published the 24 hour tendency maps well ahead of “Tellus”. The caption reads: “BESK prophesised like this….and it turned out like this.”

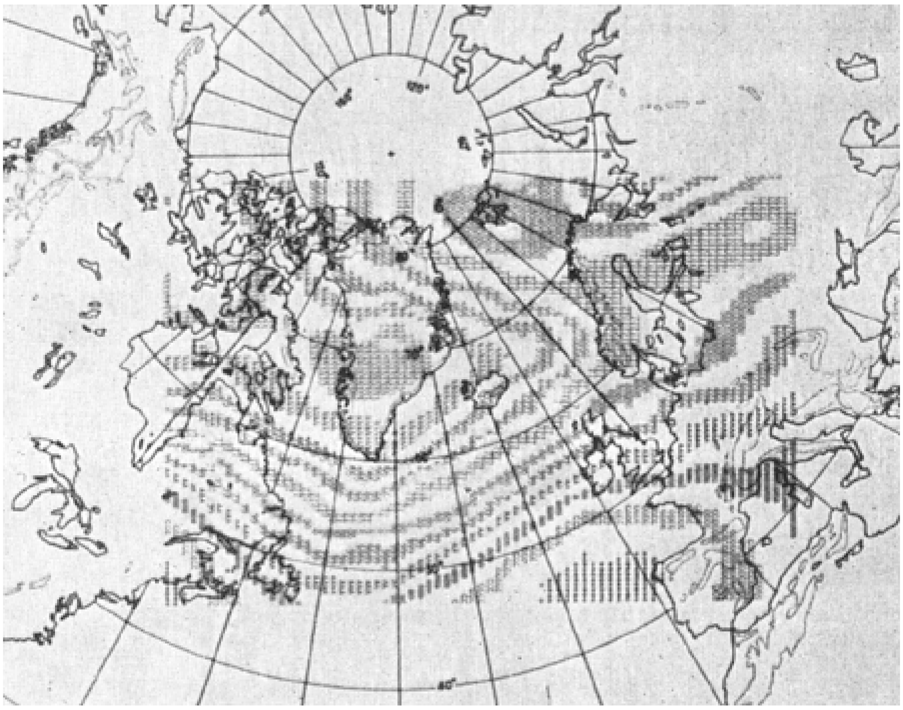


Fig. 6: An authentic “zebra plot” from the autumn 1954 operational BESK forecasts. Photo by Guy Dady in his article in “La Météorologie” some months later (Dady, 1955). In the caption text it said that the picture showed the computational area, whereas it only depicted the verification area.

1954 about the first runs as "the great item among Swedish meteorologists just now":

> "-The result was a remarkable success for the barotropic model, which even seemed to have, somehow, a apocryphal preconception of baroclinic events."

The ongoing experiments were at SMHI watched "with considerable interest - and with about as much scepticism as you might expect, or perhaps slightly more".

But SMHI was changing its mind. Now with the successor to Ångströms sorted out SMHI took a positive approach. In Dagens Nyheter 31.12 1954 Alf Nyberg said that both he and his predecessor Ångström were positively disposed towards the use of BESK and had applied for SEK 30.000 from the government to contribute to the costs.

2.16 Operational runs 1955

A first operational period began in 1 December 1954 and lasted for two weeks. The break was decided to solve some problems, in particular investigate unrealistic generations of high geopotential values.

A second operational period started 17 January and ended 25 February 1955. Now SMHI had joined the experiment. Bert Bolin lectured on BESK for SMHI staff which were invited to have a look at it (W.Persson, 1955).

Bo Döös patiently accepted all visits, his only apparent worry seeming to be the fact that BESK would stop every so often without consideration even to groups of distinguished visitors. He solved the problem, though, by always having a paper tape ready in the console typewriter. At any embarrassing computer failure he manually and unnoticed started the typewriter to print on line from the tape: *COFFEE BREAK*. The group duly impressed by the "complete automation" went to have their coffee, later escorted back to a system "still" in full operation, not aware of the frantic fixing efforts during the "coffee break".

A report was issued in spring 1955. As a complement to the normal correlation coefficient between forecast and observed changes, the charts were also subjectively evaluated by the authors: P. Bergthorson (Iceland), O. Haug (Norway) and B. Döös, S. Fryklund and R. Lindquist (Sweden). They labled the forecasts from 1 (bad) to 5 (good). As before the manual forecasts from SMHI were also evaluated.

Both the subjective and objective scores showed that the numerical forecasts were better. Correlations were 0.77 and 0.66 for the NWP against 0.63 and 0.53 for the manual. The subjective scoring also favoured the NWP (4.2 and 3.7 for NWP against 3.8 and 3.0 for the manual).

A third operational period started 12 April. The weather this April 1955 was cold and spring refused to come. One day, when Germund Dahlquist was standing in front of BESK following how the machine printed a "zebra plot", an Israeli visiting scientist E. Charrash came up to him:

-Mr Dahlquist, when is spring coming?

Dahlquist took a quick look at the barotropic +24 hour 500-mb forecast that was slowly emerging on the printer:

-Well, tomorrow afternoon, at 2 o'clock!

Next day the weather was awful as usual, but just before 2 pm the sun broke through the clouds, and spring had arrived. The previously sceptical Charrash was instantaneously converted to a true believer in NWP. "Such cases of unexpected success contributed surely to make NWP popular", remembers Dahlqvist.

On 25 May the experiment was terminated. By now the report of the two first periods had been translated to English and sent to Tellus, probably by Rossby. It was done in some haste because the authors were not notified.

Controversy struck when the report was published. Alf Nyberg pointed out that some of the subjective forecasts had received unrealistically low scores. So for example the +24 forecasts from 9 and 13 December had scored 0.00 and 0.06, whereas the +48 were much better. This indicated either an editing error or, as Nyberg suspected, that they had been verified against the wrong analysis. He wrote a letter to the Editor of Tellus to point out the mistake but it was never was published.

A fourth operational test period began at the end of October 1955 and ended at the beginning of May 1956. Some changes were made

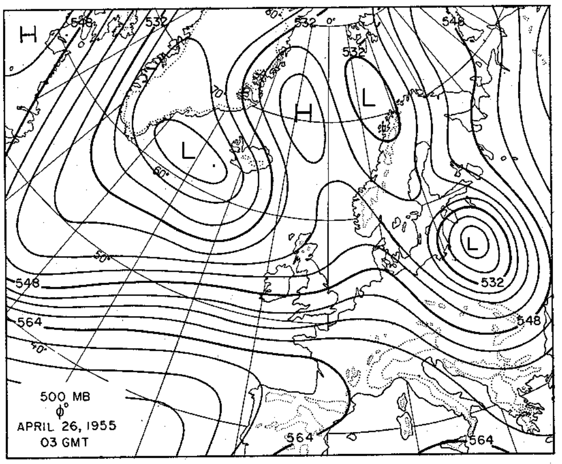


Fig. 3 a. Observed 500 mb contours on April 26, 1955, 0300 GMT. The heights are given in decameter as unit.

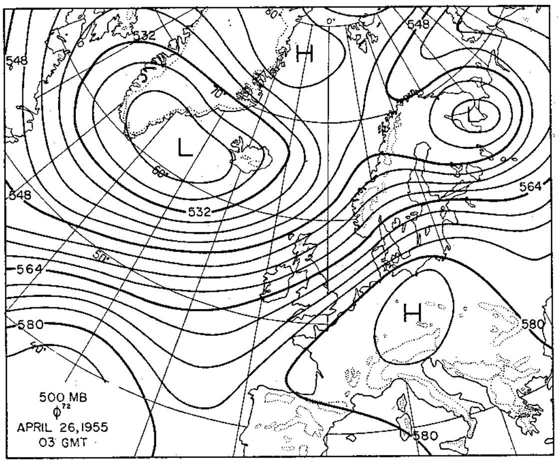


Fig. 3 b. 72-hour forecast of 500 mb from map shown in a.

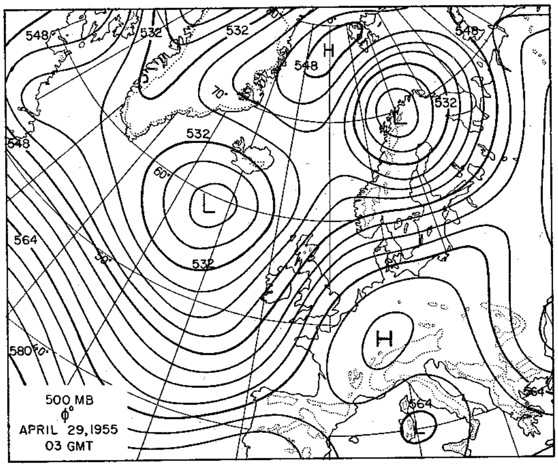


Fig. 3 c. Observed contours on April 29, 1955, 0300 GMT.

Fig. 7: The BESK forecast that gave hope about spring at the end of the chilly month of April in 1955. From a situation with predominantly zonal flow 26 April (upper chart), the simple barotropic model predicted a +72 hour change to high pressure over central Europe and mild winds over Scandinavia (middle chart), which almost perfectly verified three days later 29 April (lower chart). The maps were published in Bergthorsson et al (1955) and Bushby (1986).

in the numerical forecasting procedure, the most important the introduction of an objective (numerical) analysis of the 500-mb chart formulated by Pal Bergthorsson and Bo Döös.

2.17 New controversy – automated analysis

It had now become quite apparent that the manual analysis was too time consuming to enable an effective operational activity. Each day a subjective analysis was made of the circumpolar 500 mb 03 UTC, completed 12-13 UTC. The geopotential height data at the 31 x 42 grid points were read and punched. The computation on BESK began at 14 UTC and generally lasted for 65-70 minutes. The computed height values were plotted and analysed on a chart over a smaller area than the basic chart. The transmission in code of the charts took place 17-18 UTC. A number of 3-4 meteorologists and 3-4 assistants were daily employed with this numerical work.

Bergthorsson and Döös' (1955, see also Best, 1955) new idea was to use a short-range forecast from the previous run as a "first guess". The idea had been aired already during the NWP meeting in Stockholm in May 1952 and Dahlqvist regards the idea of a "first guess" as a synthesis of different approaches.

The introduction of objective analyses made it possible to save both time and labour. The personnel occupied with numerical forecast during these periods were reduced to 1 meteorologist and 2-3 assistants per day.

Another alteration compared with the previous was that the forecasts were based on the 15 UTC analysis. The work was done in the course of the night. All the forecast charts were transmitted in the morning and offered the most current data available at that time.

There was an interval every weekend caused by shortage of forecasters. Subjective analyses needed then to be made each Monday. The period 14 November – 15 December 1955 forms an exception to this rule, as no intervals were made and no subjective analyses were made for 32 days.

The emergence of objective analysis seems to have aroused stronger emotions from the meteorological community than the introduction of objective forecasts. Söderberg (1986):

> "The machine computed analyses were not as readily accepted as the NWP. Quite a few 'old-timers' were disgusted by the thought that the artistic, intuitive and mystical qualities professionals were so proud of could be simulated by a computer." (Bushby, 1986)

Döös remembered that Tor Bergeron's reaction to "Numerical Weather Map" analysis was not positive to say the least. His concern was not so much that the analysis was not accurate enough, but that it was carried out numerically, *by a computer*! For Bergeron weather map analysis was rather a fine art than applied science. The analysis was, for a Bergen School connoisseur, not only a method to determine the "initial state", but also a process whereby the forecasters could familiarise himself with the weather, create an inner picture of the synoptic situation.

There was also suggestions from a Swedish meteorologist Duvedal (1961) employed by the Scandinavian Airline System to rely entirely on manual analyses. According to Duvedal's experience the conventional analysis could start before all observations have arrived and could be adjusted when they arrive.

In autumn 1955 the objective analysis scheme was nevertheless introduced, and several series of 3-day barotropic forecasts were computed on a routine basis. Subsequently the memory of BESK was improved and expanded. The Williams tube memory was replaced by a magnetic core memory and a magnetic drum memory was installed. This made it possible to both increase the forecast area and speed up the computations. It was decided to continue operational NWP later in the autumn

2.18 The “User Guide”

When the Swedes in 1954-55 started to produce operational NWP they asked themselves: -What should we do with the output?

Each day (in the morning) a so-called comment was issued on the forecast charts, as an attempt to interpret or “translate” the forecast charts into terms of weather to be expected for the next 72-96 hours (3-4 days), when front systems could be expected to pass and so on. The purpose of the “commentary” was to enable the military forecasters at the Air Force bases to “translate into weather” the one, two and three day forecasts of the upper air flow, which had been computed by BESK. But using NWP was, *and is*, not trivial.

In case the numerical output agrees with what can be expected from experience and intuition, it will be welcomed by the forecaster because it makes him more confident. In case the NWP has a different view a problem arises. But while it is always possible to discuss with your colleagues, *the NWP output lies on the table or hangs on the wall in a mute silence*.

The Swedes set out to tackle this problem in some sort of “User Guide” to the forecasters who were going to use the BESK output. Like the very few subsequent NWP guides the advices are partly contradictory.

Since it had turned out that the numerical NWP was superior to the manually made forecasts they should, according to the “Guide”, be accepted as they were. No speculations about possible baroclinic developments or other features should be made. The “Guide” warned that most probably the forecasts would be even more erroneous (or just as erroneous) in those cases. When the forecasts indicate that a change of weather type was to be expected, this should be particularly thoroughly treated in the commentary. Changes of the large-scale flow were particularly difficult to forecast by con-ventional methods. It was one of the advan-tages brought about by NWP that improve-ments had been achieved in this respect.

Since the main fronts were intimately coupled to the jet streams it should be quite easy to indicate the frontal zones on the forecast charts. In case a well-developed jet was not present it was realistic to assume that also the main fronts were less pronounced.

Concerning identification of individual frontal systems, it was reasonable to assume that every wave (wavelength 1500-2000 km) was coupled with a separate frontal system á la Bergen school. The forecaster could distinguish between two different stages of development:

> “In case the wave was not seen in the 500 mb flow, probably a baroclinic development was under way. The intensification of the wave could not be completely predicted by the barotropic model and in those cases only the direction of the wave’s movement can be forecast. It must necessarily move in the direction of the main flow. The forecaster should be aware that a wave can develop and thus he can predict within which area a cyclogenesis is *probable* and even predict its direction, i.e. predict those areas, which are within the ‘danger zone’”.

So what to do when the NWP could not be fully trusted? According to the “Guide” corrections should be allowed in two cases:

1. when later information (mainly the 03 GMT analysis 12 hours later) indicated unforecasted developments.
2. when the NWP developed spurious anticyclones or overdeveloped ridges.

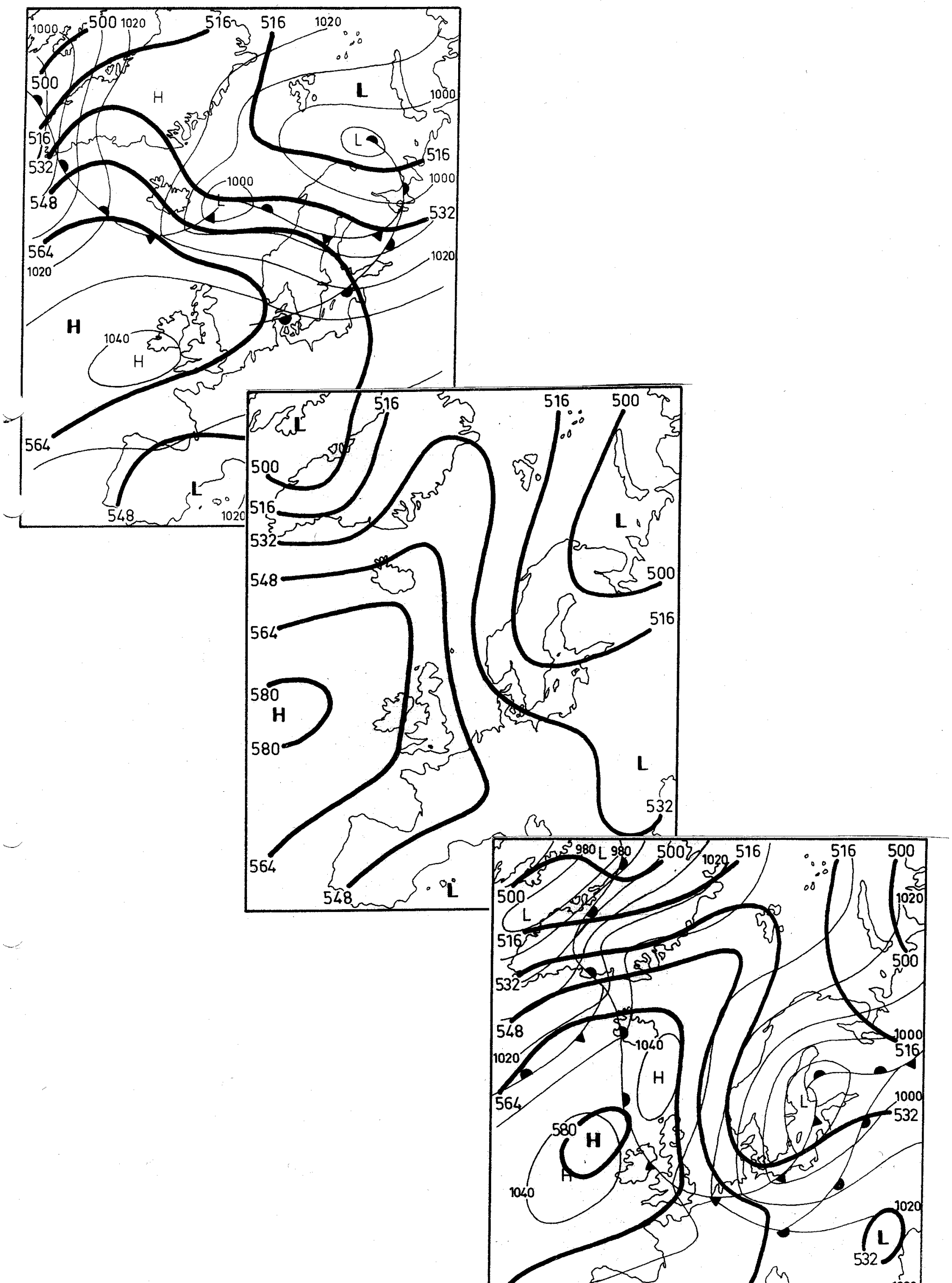


Fig.8: In January 1954 a devastating north-easterly storm brought destruction to eastern Sweden. It had been poorly forecast, but a +24 hour forecast re-run on BESK, initiated by Karl-Einar Karlssson, one of the progressive meteorologists at the aerological section, showed that a NWP system operational at the time, could have provided crucial guidance.

Upper chart depicts the 500 hPa flow 2 January 1954 00 UTC, middle chart the +24 hour barotropic forecast and, bottom chart the 3 January 1954 verifying analysis. The MSLP fields have been added for reference and were of course not part of the forecast calculations.

Already in 1955 the forecasters made the same experience as later generations*: the NWP could change ("jump") drastically from one day to the other*. This meant that one of the forecasts were wrong (or both). It also created a communication problem with the public. The "NWP Guide" therefore advised the forecaster to strive for continuity in the comments. In those cases when they were forced to deviate from that an explanation should be given, for example: "Yesterday's NWP suite bad due to strong baroclinic development over the North Sea".

When the anonymous author of the "Guide" was drawing to a close he seems to have realized that he had not given much guidance. So he ends by a giving them a rather free hand:

> "What is said above is as the heading indicates only supposed to form the main guidelines and still leave some freedom for the meteorologist when formulating the comment".

The recommendations from NWP modellers to the weather forecasters have not advanced much during 50 years.

2.19 The MVC takes the lead

As early as 1952 Rossby had become interested in atmospheric chemistry and for that purpose added a chemist to the staff. By summer 1955 the emphasis at his institution had shifted away from NWP toward cloud physics, artificial rain, atmospheric chemistry and global temperature changes. During the coming years the NWP would go over into the hands of the two weather forecast institutes MVC and SMHI.

At the bi-annual Nordic Directors' meeting in Oslo in September 1955 Alf Nyberg put in a supportive word for the BESK forecasts and suggested some form of Nordic cooperation.

Operational runs were still mainly a responsibility for the MVC, but SMHI's involvement increased. In January-June 1956 SMHI made 28 routine computation twice a week on BESK and a new FACIT-EDB machine.

The arrival of numerical forecasts implied changes in the staffing and need for more resources. A new commission looked into the matter. In contrast to the military, which argued along lines of cheaper and quicker forecast production, SMHI chose to emphasise improvement in *forecast quality*. Waiting for the commission the NWP was run to a bare necessity. According to the annual report for 1957 NWP forecasts were run only in connection with Easter, Christmas and new years holidays. At the Nordic Directors' Meeting in Helsinki in 1957 Alf Nyberg continued to argue in favour of NWP. He mentioned that it was useful during zonal regimes, worse during blockings or on occasions of strong cyclogene-ses over E USA coast. During 1958-60 SMHI continued to run barotropic forecasts on an irregular basis with forecasts up to +48 hours, sometimes +72 hours. The development work was left to MISU/IMI and MVC.

At a NWP symposium in Frankfurt in May Oscar Herrlin (1956) said that he felt that convinced that NWP was the modest beginning of "a new era" within the forecasting technique, comparable with "the era created by the Norwegian school in the 1920's." He also mentioned plans to rationalise the production by engaging assistants instead of graduate meteorologists in the operational work.

By this time the NWP work was strengthened by the arrival in September 1955 of the Danish meteorologist Aksel Wiin-Nielsen from Copenhagen. He had worked part time at the Danish Meteorological Institute, part time at the University for Ragnar Fjörtoft before he returned to Norway in 1955. While Döös worked on problems related to analysis, Wiin-Nielsen worked with Bengt Söderberg at MVC to develop a two-parameter model[38]. In December 1958 Wiin-Nielsen left to join the JNWPU. He would remain there until 1974.

2.20 The first baroclinic model

Aksel Wiin-Nielsen's 2-parameter model consisted of a 600 hPa level, advecting a 400-800 hPa thickness, with no feed-back from the thermal field to the advective level. The grid size was 300 km and the time step 45 minutes. 23 x 24 gridpoints covered an area from Greenland to the Azores, to Greece and then to the Ural Mountains. Apart from Wiin-Nielsen the group consisted of Rolf Lindquist and Bengt Söderberg (MVC), Hlynar Siggtryggsson (Iceland), Hessam Taba (Iran) and Aimo Väisänen (Finland). The model was subjected

[38] There was also an earlier attempt by Duvedal (1954)

to extensive real-time testing from October 1958 to April 1959. The results were "very satisfying" although there was a drift towards lowering the geopotential heights and effects of the constant boundaries after 36 hours. Compared to the conventional forecasts the automatic NWP was "superior". Since the analysis for the computa-tions still had to be done manually, there was a 12-hour operational delay. If this was taken into account, the NWP forecasts were still of even quality to the subjective.

After the experiment period ended, the military called a press conference on 29 April 1959. A dozen or so journalists turned up and were given a BESK demonstration by Bengt Söderberg. It was only five years since MVC, as the first weather service in the world, had run operational NWP. Soon the military would acquire an IBM 7070. They intended to create a fully automatic weather service within a near future.

To please the journalists the military Press Officer invited them to his home for beer, sandwich and "schnaps". The party lasted into the small hours and everybody, journalists and meteorologists alike got quite drunk. Next day only a few daily newspapers carried stories and they were short and rather poorly written. The Press Officer realized that the party had not been such a good idea after all. Then someone showed him the Christian daily, which had a comprehensive and rather correct account. This was most surprising since that particular journalist had been one of the most drunk!

But the plans for a quick automation had to wait. It would take some years before the 2-parameter model was implemented. In 1960 Söderberg ran more tests, now with 500 hPa as the advective level and 850-300 hPa thicknes, still with no feedback from the 500 hPa. The grid was 300 km with a 1-hour time step. The details of the model were only published in some internal MVC memoranda (Söderberg, 1955, 1964 and others). But it would last until October 1962 with the arrival of a new computer, IBM 7090-1401, that the 2-parameter model at last could become operational. The area was extended to 47 x 47 grid points for the analysis, 44 x 33 for the forecasts. The centre of the area was over Greenland, with 500 hPa advecting the 300-700 hPa thickness

This model would run at the MVC with small changes for eleven years until 1973, when SMHI definitely took over the full responsibility for all NWP production in Sweden.

The military meteorologists had realised around 1960 that if they insisted on being in the lead of NWP in Sweden, it would increase the "risk" (as they saw it) that the state authorities rejoined them with SMHI (Söderberg, personal communication).

2.21 SMHI takes the initiative

During the late 1950's the Swedish government had finally realised the potential of NWP and given SMHI increased resources. In December 1960, after having made a journey to the US and attended the NWP symposium in Tokyo, Bo R. Döös was employed by the SMHI together with a Norwegian meteorologist, Thomas Thompson. Routine computations of 500 hPa barotropic forecasts on a 300 km grid were run up to +48 hours, later +60 hours (fig. 8). The machines were BESK and FACIT EDB at the Scandinavian Electricity Plant.

Verifications indicated that the NWP was "on average clearly" better than the conventional. The special forecast run for aviation winds showed an improvement of 50% compared to the conventional methods. Staff at the aerological section could therefore be drastically reduced, mainly by re-educating them to become computer operators.

The construction of a 2-parameter model started in 1962 and was completed in 1963. In contrast to the MVC version it allowed interaction between the advective 500 hPa flow and the 300-850 hPa thickness fields. Bo Döös, Lennart Bengtsson, Thomas Thompson and a young recruit, Lars Moen, attended the international symposium on NWP in Oslo 11-16 March 1963.

But the results of the 2-parameter model were slightly disappointing. It had been noticed that the cyclogenetic process was too weak initially and too strong in later stages. At a seminar at SMHI in November 1963 (Kempe, 1963) Moen noted that although experiments with baroclinic models had gone on for some ten years, much still remained to be done. In spite of the fact that the 2-parameter baroclinic model performed no better than the barotropic the group still had great expectations.

In 1964 Lennart Bengtsson, submitted a paper to Tellus where he discussed the causes of the shortcoming of their model. The model suffered from the "geometrical" constraint that both the vertical wind and the divergence are *symmetrically* distributed around the advective 500 hPa level. Consequently the model was unable to describe in a realistic way the vertical re-distribution of divergence during a cyclogenesis. Other sources of errors were the model's inability to vary the static stability and its inconsistent numerical approximations (Bengtsson, 1964).

The obvious alternative was a three-parameter model, but that had to await more powerful computational resources; it was difficult enough to run the 2-parameter model operationally. This became possible only in 1965 when SMHI had acquired their first own computer, SAAB D21[39]. A special run twice a day provided aviation wind and temperature forecasts for five levels.

In October 1965 SMHI took the bold decision to start to issue *five-day* weather forecasts, twice a week, on Monday and Thursdays, to the press, to radio and TV. This is a practice that continues to these very days.

A three-parameter model was then already under way at SMHI. Work had started already in 1962 and was completed in 1965, although the operational implementation had to wait until 1 April 1966. The three-parameter model was run up to +36 hours, and then barotropic to +120 hours. The grid length was 300 km and the octagonal area covered a large part of the Northern Hemisphere. After 1 September it was run twice a day.

Arne Lindblad, then console operator (later computer operation chief), still remembers when he delivered the first 3-parameter forecasts to the forecast office. The duty forecaster, who belonged to the old school, was working on his manual analysis. Arne started to explain the new material but was abruptly interrupted:

> -I don't want to see that rubbish – put it on the table over there![40]

But not everybody was conservative. In June 1968, at a Nordic meeting in Stockholm, Tor Bergeron, in a talk "*Fifty years with the Polar Front"* suggested that even the forecast of the weather with respect to local and periodic factors, eventually should be automated. Bergeron said that he had been impressed by the surprisingly high skill of a statistical interpretation scheme designed by Lönnqvist (1966), Head of the Meteorological Division at SMHI.

2.22 Changing conditions

In 1969 SMHI acquired a new SAAB D22 computer. This allowed the "telescope technique" to be introduced with 150 km resolution for an inner area, receiving boundary conditions from the hemispheric area run. In 1977 a six-level balanced model was introduced. It lasted only for six years because now there were prospects of running primitive equation limited area models with boundary conditions from a larger model. By then the planning for a European meteorological computer centre was under way, what would become the ECMWF. Sweden decided to divert much of its resources into this co-operation.

It is no coincidence that three senior figures at ECMWF: Lennart Bengtsson, Daniel Söderman and Aksel Wiin-Nielsen, derived their experience working with the numerical weather prediction project that was originally initiated by Rossby 25 years earlier. The fact that only one of them was Swedish bears witness to the truly international character of the project.

[39] SMHI had to share the computer with the Swedish Road Administration (SRA) until 1966. In a "coup" Döös and the head of SRA signed the D21 contract before the money was officially secured.

[40] Many years later, Arne Lindblad recalls, the same forecaster was very upset when he was told that a computer standstill meant that there would be no NWP on that day: -Bloody hell, how do you expect me to be able to make a forecast without the numerical charts! (Lindblad, 1986)

3. IMPRESSIONS OF EARLY OPERATIONAL NWP AT OTHER CENTRES

3.1 General

When computers became available in the 50's meteorologists were among the first to display a keen interest in their use. They also had the highest demands on their capacity. The earliest NWP initiatives sometimes came from the universities, sometimes from the meteorological institutes, in particular when the development of computers allowed operational runs.

The historical accounts on early NWP given below are based on a multitude of sources: the incomplete record of contemporary "NWP Progress Reports" in the SMHI library, some WMO Technical Notes from the 1960's, Hessan Taba's interviews in the WMO Bulletin with leading meteorologists, my own correspondence with some participants and passing remarks in scientific papers. In 1955 Professor George W. Platzman distributed a questionnaire to all meteorological centres. The questionnaires and the summary of the answers are found elsewhere on this CD. (See bibliography for historical review articles on British, German, French, Canadian and Australian NWP).

Before we go to the description of the development in seventeen counties, there is a decision to be made what is meant by "NWP". Between the conventional weather forecasting practices and the computer based forecasts, there were several semi-objective graphical forecast methods, some of which could be, and often were, converted into computer code. The most popular and widespread was Ragnar Fjørtoft's quasi-Lagrangian graphical method of integrating the barotropic vorticity equation[41]. The method was presented on the NWP symposium in Stockholm in 1952 (Fjørtoft, 1952) and was widely used in the 1950's and 1960's in many minor meteorological services, which had not yet access to computers. When a computer became available, experiences from Fjørtoft's method often served as an encouragement to develop a proper NWP mo-del. It also paved the way for Lagrangian or quasi-Lagrangian numerical techniques. However, I have chosen not to include institutes, which, up to 1970, *only* used Fjørtoft's method. The reason is mainly that it deserves its own scientific and historical presentation.

3.2 Japan

The history of Japanese NWP is not only about difficulties in a war torn country, but also about a constant "brain-drain" to foreign institutions, most notably in the USA. The contributions the Japanese have made to NWP are therefore as much part of the NWP development in the USA as in their own country.

The story starts in 1949 and the (late) arrival to the Geophysical Institute in Tokyo of Jule Charney's 1947 paper on baroclinic disturbances in the westerlies. One day in 1949 Professor Syono rushed into the classroom with a copy of Charney's paper in his hand and excitedly proclaimed:

> "-Look! This paper has modernised the science of meteorology!"[42]

Shigekata Syono had been appointed professor at the meteorological department of the Tokyo University in 1945 (a post he would held to 1969). The students, obviously, had to read the Charney paper carefully from beginning to end. Kasahara had the feeling that Syono himself had been thinking of developing a similar theory and now was excited that his long-term wish had been accomplished. Syono had always emphasised to his students that the research atmosphere in dynamical meteorology in Japan was inferior compared to other countries. So, he strongly encouraged the young students to make contact with the international colleagues.

3.2.1 The first NWP group

A NWP group was formed around Syono that included young staff and scientists, and researchers from the Japanese Meteorological Agency (JMA). The group studied other papers from abroad, among them Charney and Eliassen (1949) on one-dimensional barotropic forecasting. In 1950 Syono and some members

[41] Fjørtoft (19529 followed an old suggestion by Vilhelm Bjerknes to solve a non-linear equation by graphical methods.

[42] Akira Kasahara in Lewis (1992), a very interesting and illuminating paper on post-WWII Japanese meteorology.

in his group published in the Journal of the Meteorological Society of Japan, the first in a series of three papers on "numerical weather prediction". The first was a comparison between Syono's ideas and Charney-Eliassen's, which were rather similar.

In the meantime one of Syono's students, Kanzaburo Gambo had entered into a correspondence with Jule Charney. Gambo had graduated from the university in Tokyo in 1945. In 1947 he was employed as a research associate in 1947. The times were hard; they were poorly paid and had to find part-time work as teachers in high-schools to make ends meet. One of his first papers (Gambo, 1951) was a study of energy dispersion, much inspired by Yeh (1949). In 1952 Syono and Gambo published two more papers on NWP in J Met Soc Jap. Now they were much more following in the steps of Charney and Rossby.

The contact with Charney was mutually fruitful. Gambo was invited to spend two years at the Institute for Advanced Study in Princeton with Charney's group. He also had contacts with George Platzman at the Chicago University. During the stay he sent letters home to report on the progress of NWP in the US.

3.2.2. NWP research

When Gambo came back to Japan in 1954 he established a NWP research group in Tokyo. Together with Syono he co-ordinated a monthly set of lectures that included about 25 researchers who were either graduates or JMA employees. They occasionally held retreats in the outskirts of Tokyo. The lectures covered a broad spectrum of subjects including dynamical meteorology and numerical weather prediction. From the mid-1950's there were a number of seminal contributions from the members of the NWP group, for example the first paper on barotropic typhoon track forecasting by Sasaki and Miyakoda in 1954 (see also Staff Members, 1955 b).

The ability of Japanese meteorologists to make important contributions to NWP already at this stage is the more remarkable since they had limited financial support from the federal government (except for the stipend to Gambo). As a matter of fact, their greatest sponsor was one of the largest newspaper companies in Japan, the Asahi Press. In May 1954 it provided a lump sum gift of $ 2 800, which significantly enhanced the group's activities.

Between 1954 and 1960 the Japanese meteorologists performed their numerical calculations using Fjørtoft's graphical methods, desk calculators and the FACOM 100, an electromechanical relay switching computer that was built by the Fuji Tsu Shin Ki Company (what we today know as Fujitsu). They carried out prediction experiments of a wide range of problems, including predictions of precipitation, motion of tropical cyclones as well as extended forecasts.

3.2.3. JMA joins the NWP work

It was not because of any non-interest that the JMA had not been actively involved in the NWP. Already in 1950 its Director General Kiyoo Wadati had visited the US and met Rossby and his group. He had been much impressed by what he saw*: weather radar, automated observations and NWP.* In the mid-1950's the time had come for Wadati to start to put in requests for money to install their own electronic computer.

But it was not until April 1959 that JMA succeeded in obtaining an IBM704 (memory 8KW)[43]. Now it became meaningful to create a NWP section also at JMA, under the formal name of Section of Electronic Computation Centre (ECC). Gambo and others joined from University of Tokyo. The plan was to make NWP operational. Already in summer 1959 they had a hemispheric barotropic model up and running forecast to +48 hours. The model was very similar to the one used by NMC at the same time with a grid length of 381 km. The reception by the forecasters was mixed.

> "-At that time, some forecasters understood our efforts well, while other ones did not accept our efforts."(Gambo, personal communication 1995)

As with the NMC model they experienced serious problems with rapid retrogressive

[43] According to what Wadati told Taba (1985a) NWP was introduced in Japan in March 1959, according to Staff Members (1960) the IBM computer was acquired in April 1959 and regular barotropic forecast started in June (Bushby, 1986) or July 1959 (WMO, 1962). Bushby presented his information, which he had got from Japanese colleagues, during a conference in Tokyo and are published by the Journal of the Meteorological Society of Japan.

motion of the ultra-long waves. It was solved by introducing the so-called "Cressman term[44].

3.2.4 The lateral boundary conditions

In my correspondence with professor Gambo I asked specifically about problems with constant boundary conditions. He assured me emphatically that there had been few problems:

> "In case of NWP over Japan, the westerlies are not maximum over Japan due to the Himalayas. As is well known, the maximum speed of the jet stream is found over the middle of the Pacific Ocean, not over Japan, which is situated on the western side of the Pacific Ocean. In this sense, the problem of western boundary in relation to the energy dispersion was not serious for the short range forecasts (2-3 days)."

Due to sensible heat fluxes from the warm water in the Pacific Ocean there were more problems with non-adiabatic effects. Again, experiences from the European area, on the eastern side of an ocean, is not necessarily applicable to regions on the western side.

3.2.4. The 1960 Tokyo NWP Symposium

At the end of the 1950s, professor Syono, in order to show the world what was being accomplished in Japan and to benefit from work in other countries, worked passionately to organise a large international NWP symposium to be held in Tokyo 7-11 November 1960. It was a great success and became a milestone for the advancement of numerical weather prediction. It was here the development of primitive equation models definitely was put on the agenda, it was also here that Edward Lorenz presented his first experiences of diverging non-linear computations due to small round-off errors…

> "Looking back, it is fair to say that the meeting was one of the epoch-making events in the history of NWP" (A. Kasahara to Lewis, 1992).

The 1960 symposium (Tokyo, 1962) was followed in autumn 1968 by a second symposium on NWP (Tokyo, 1969)

Already in 1961 the ECC team had developed a baroclinic model, first with three, later with four levels. It was, according to Gambo (1995, personal communication) run "informally" and not operationally. In January 1965 the JMA acquired a new computer (HITAC 502F/5020) which became operational in February 1967 after which the baroclinic models could become operational in 1970 (Bushby, 1986).

3.3 Germany

Like Japan, meteorologists in war torn Germany lacked resources to rapidly bring it back into the forefront position it has traditionally held in European meteorology. But, like the Japanese, what they lacked in finanacial and technical resources, they compensated by making use of their enthusiasm and human resources in real heroic efforts (see Reiser, 2001, for a detailed account of early German NWP).

In 1949 the main German meteorological service was in the US-Zone in Bad Kissingen. The research department was lead by Hermann Flohn (Taba, 1984). One of his co-workers was a young, gifted meteorologists, Karl-Heinz Hinkelmann (Taba, 1985c). He had become interested in the problem of "noise" in the equations of motion. At a symposium in Germany in 1951 he met Rossby, who invited him to Stockholm. There he took part in the tendency calculations for the barotropic model.

In 1952 Deutscher Wetterdienst (DWD) was created by a merger of all weather services in West-Germany. When Hinkelmann returned from Stockholm he was not content with just barotropic forecasts, but wanted to proceed to make baroclinic models. The fact that there was no computer available did not deter him. If it were possible to manually integrate the barotropic model in one time step of 12-hour, perhaps the same could be made with a baroclinic model?

3.3.1. The hand calculations

Hinkelmann first defined a 3-level model at 850, 500 and 225 hPa with 34 x 25 (737) grid points 300 km apart. To perform the calculations he set up a group of two meteorological students and two female clerks. They were all

[44] This term is often called "artificial", but, as I understand it, has a solid physical-hydrodynamical basis. By allowing the upper surface to become an *internal* surface it could increase its motion so that some of the energy that previously went into moving the ultra-long waves westward was now deflected upwards (Cressman, 1958).

crammed in a narrow room in an old hotel full of cigarette smoke and dust everywhere.

The forecast office at DWD prepared manually the initial analyses. The group then spent several days evaluating the vorticity and the Jacobians by graphical methods. The grid point values then had to be interpolated, read out and written very small on a huge paper covering the blackboard. The main step of the work was the solution of the three-dimensional elliptic equations for the tendency by relaxation. One girl read out the figures of the difference operator to the other, who sat at a very heavy noisy, slow mechanical calculator. She called the results back to the first girl.

So it went on from grid point to grid point, from row to row. After some hours, the girls changed their places. After a couple of days they had done one iteration for the whole field, and after several weeks they got figures they assumed to be the solution to the elliptic equation. The tendency was converted back to a map and graphically added to the initial field, giving them a 12-hour forecast. The entire procedure was then repeated to give a 24-h prediction. The result of some months of work did not totally look unreasonable.

By now (around 1954) access to computers had become feasible. According to DWD's answer to Platzman's 1955 questionnaire they ran barotropic forecasts up to +72 h on and a limited number of baroclinic forecasts with 850, 700, 500 and 300 hPa on the Swedish computer BESK. Later they seem to have used an IBM704 in Paris.

3.3.2. A new direction for NWP?

The period 1956-60 was according to several accounts, the critical years of NWP worldwide. The quasi-geostrophic models did not perform as well as expected, there were large scale errors due to retrogression of the planetary waves and, finally, there were alternative propositions that numerical weather forecasting could be run more along statistical than dynamical lines[45].

It was now that Hinkelmann pointed out a new direction, at first against many experts, even Rossby (Wippermann, 1988). His idea was to develop hydrostatic primitive equation models. Flohn later remembered a late night discussion with Rossby in 1956 on a railway station, where Rossby finally was won over[46]. Meanwhile the rapid progress in computer technology made the integration of more ambitious prognostic systems technically and economically feasible.

The following years, 1957-60 were frustrating because the DWD refused to buy a computer They regarded Hinkelmann as an unrealistic romantic, something that made him rather bitter and decide to leave DWD as soon as an opportunity offered itself (Flohn, 1973).

3.3.3. Operational NWP

However, in the 1960's the tide slowly turned. In 1965 Hinkelmann becomes Head of Research at DWD. Its president, George Bell, was slowly becoming less hesitant about purchasing a computer. It seems to have been the persuasive powers of Erich Süssenberger and Ernest Lingelbach (meteorologists at the Ministry of Transport) which brought about the change. In November 1965 a CDC3400 was delivered to the DWD.

The following year, in October 1966, operational NWP started with +72 h barotropic forecasts and three-dimensional atmospheric analyses of a resolution of 381 km. In summer 1967 a new CD 3800 arrived and in October baroclinic forecasts started over an octagonal area with 1940 grid points, which were increased to 2080.

The following year, 1968, the opportunity Karl-Heinz Hinkelmann had waited for offered itself and he accepted a professorship at the University of Mainz.

3.3.3 Developments in the DDR

Just before this text had to be sent to the organizers I found in the WMO Bulletin an interview with the East-German professor Dr. Wolfgang Böhme. He told Taba (1998) that when he was in his 30's (in the late 1950's) he

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[45] The American mathematician Norbert Wiener suggested that statistical schemes were able to replicate non-linear time evolutions. It was with the purpose to look into this that Edward Lorenz in 1958-59 was running rather abstract non-linear mathematical models to see if their behaviour could be reproduced by statistics. During such a calculation the famous coffee break occurred after which he discovered ”chaos”.

[46] Rossby, as many others, were sceptical about the primitive equation approach because, as they felt, there was nothing to be taught about the real atmosphere. As Edward Lorenz would later phrase it: *it is through non-perfect models that we learn how the real atmosphere works.*

and a group of younger scientists were not happy about the prevailing subjective methods and wished to introduce new objective procedures on the basis of theoretical meteorology and the use of computers.

At the beginning of 1958, they had no computer, except for some manual or electrically powered table calculators and machines for punch tapes. The first computer, of medium efficiency, was developed and produced by the Carl Zeiss Enterprise in Jena. As of 1961 they could, in principle, use this machine but there were too many demands by other customers who had priority. They could only use it at night or during the weekend. Two years later, they were able to use machines of a similar nature at other sites, such as the astronomical observatory in Babelsberg.

Finally, in 1969, with strong support from the Russian scientist, Academician E.K. Fedorov, the authorities gave them the Soviet machine BESM-6. A new building for the computer was constructed and the training of staff followed. BESM-6 was a relatively fast computer and they started routine operations in January 1971.

3.4 France

One of the first national meteorological institutes to show an active interest in NWP, was Météorologie Nationale (Météo France). But it would last until the early 1970 before it started to make itself known in the field. The history behind the "Big Bang" in French dynamic meteorology is worth telling in some detail.

3.4.1. The pioneers

At the beginning of the 1950's Jean Bessemoulin, head of the Forecasting Division at Météorologie Nationale, discussed NWP with his deputy, Robert Pône. Pône was keen to start such activity and in October 1952 he attended the NWP conference in Stockholm. He remained there until Christmas.

In autumn 1954 "Météorologie National" sent their leading theoretician, Guy Dady, to Stockholm for a few months. This was the time of the first operational NWP and the article Dady was to publish in "La Météorologie" is one of the few "eye witness accounts" with unique illustrations. Later Dady (1991) remembered:

> "At the end of 1954, during one season, I had the privilege almost daily, to have lunch with Rossby, and at those occasions discuss with him[47]. He was improving his French and being the only person of this nationality at the institute, I was quite happy to give him this favour. Rossby was a man of great simplicity, which also manifested itself both in his behaviour and his scientific logic. He thought that the problems [in meteorology] only appeared complicated to us, just because we had not tackled them in the right way. What concerned the problems of prediction, which occupied us, he did not let him be troubled by the apparent complexity of the atmosphere on a human scale."

For Rossby the atmosphere was bi-dimensionel and to represent it the 500 hPa level could be used. With this approximation the forecast problem was simply one of "advection and integration of Poisson's equation". But Dady realized that Rossby saw deeper:

> "But this prediction prototype is poorly understood…it is generally interpreted as a forecast of the 500 hPa, which it is not."

Dady also remembered Rossby's irritation when he learned that the British meteorologist Reginald C. Sutcliffe tried to treat the atmosphere as a two-layer model.

3.4.2 First computer calculations

Before Dady went to Stockholm he had experimented with a kinematic numerical prediction method designed in 1929 by A. Giao based on extrapolation of isobaric and isallobaric fields.

In 1954, there were only three computers in France (Dady, 1991). The following year he was allowed "some hours per week" (during lunchtime!) on one of them, at "Direction des Études et Fabrications d'Armement". According to Dady (1991) the resistance towards NWP at that time did not come from the forecasters, as one might surmise, but from Dady's colleagues at the research department EERM (l'Établissement d'Études et de Recheres Météorologi-ques). Dady therefore joined

[47] At this time the dominating news from the US was that the Senate voted to condemn Joseph McCarthy and his anti-Communist campaign. Dady got the impression from Rossby that by returning to Sweden he had also wanted to escape the US political environment.

forces with Robert Pône on the operational side at SMMA (Service Prévisions de la Météorologie Nationale).

In 1955 Guy Dady, Robert Pône and Jean Andreoletti got access to a French machine CAB2022 (Calculatrice Binaire Arithmétique), built in 1954[48]. It had innumerable bugs and breakdowns, but allowed them to implement a simple barotropic model +24, +48 and +72 hours ahead. The area contained 1024 (32 x 32) grid points with grid length 400 km.

At this time Dady (1957) argued strongly that the Météorologie Nationale should acquire their own computer: "France is about two years behind the USA, UK, Sweden and USSR". In fact, already in 1956 Météorologie Nationale had made a decision to acquire a computer. The necessary money had been granted in October 1957. Still it lasted until October 1960 before the KL901 computer was installed and inaugurated on 18 Nov 1960. The machine was built especially for Météorologie Nationale by Societé Nouvelle d'Electronique Radio Industrie (SNERI) and cost 230 milj old francs (Dady and Pône, 1958; Pône, 1993).

3.4.3. "A brain within a brain"

A year before this happy event the "French Meteorological Society" in November 1959, organised a full day symposium together with the French Society of Hydrotechnique. The theme was "The mechanics of fluids and meteorology". Among the speakers were Guy Dady and Robert Pône. Dady spoke about the "Special character of the mechanics of meteorological fluids" and Pône about "Perspectives of numerical prediction of precipitation". The chairman, Joannès Thomas, acting for the Director André Viaut, who was on a mission, presented Dady as "the meteorologist who is most envied by his colleagues" because of the imminent arrival of a computer:

> "-Indeed, if one is committed to play with the characterisation, I would say that Monsieur Dady is in fact 'a brain within a brain', because we await, this year, at the Meteorological Service, the installation of an electronic machine, that will be used to forecast the weather."

The talks by Dady and Pône seemed to have been well received and were followed by long discussions. One participant asked Dady if the same equations were used abroad, and Pône got questions about the possibilities of forecasting rain.

Since the KL901 was a prototype computer the work in NWP was mainly experimental. The computer was used, apart from running the barotropic model, for automatic plotting, chart drawing, and analysis. The annual report to the French National Committee on Geodesy and Geophysics in 1959 suggests that they were trying to run the barotropic model hemispherically. If this were true, Dady and Pône would have encountered the common problem of retrogression of ultra-long planetary waves.

3.4.4 The NWP development stagnates

For reasons, which are not quite clear, the NWP development seems to grind to a halt. But Dady kept the flame alive. He was teaching dynamic meteorology at the École Nationale Météorologique (ENM) for the coming generation. At a conference organised by the French Meteorological Society in November 1962 Dady made a strong case for the use of dynamic meteorology in daily meteorological forecasting, although it presented "certain weaknesses" in its "logical comprehension and experimental verification".

Dady (1962) outlined his ideas on filtering the equations, models, influence functions and spectral techniques. At this time he worked on an approach where the atmosphere was represented by spectral functions in the horizontal and empirical orthogonal functions (EOF) in the vertical. He had on this topic relation with Van Isacker in Belgium. At a WMO meeting 1964 Viaut said that with respect to NWP in France, "progress was deliberately slow". The reason was problems with decoding of data[49].

One might wonder what Viaut had in mind. It is not unusual for civil servants to progress with "deliberate slowness", but it is rarely admitted. In 1964 France had had ten years of great difficulties: a painful retreat from Indo-

[48] The first French computer CUBA (Calculatrice Universelle Binaire Arithmetique) from 1952 did not function operationally.

[49] At that time Fjørtoft's method seems to have offered operational possibilities. In 1963 Jean Lepas published an article in La Météorologie which showed tendency correlations of 80% for +24 h, 74% for +48 h forecasts (Lepas, 1963). The forecasts were run on the KL901 computer with 1024 grid points and 400 km grid lengths.

china, a change from the 4th republic with its constant governmental crises to the 5th republic under de Gaulle, with right-wing terrorism and insurrection after the traumatic war in Algeria. In 1964 all this was behind and it seems natural that developments could take a more optimistic turn.

But to find the cause we must go even further back in history, to the Second World War. The stagnation in the early 1960's was due to lack of any young recruitment since 1945!

In the aftermath of the liberation in 1944-45 the staff of Météorologie Nationale was completely renewed. Some young scientist entered in the period from 1945 to 1951, but in those days, they had to spend their first duty years in Africa. So they knew more about tropical meteorology than NWP. After 1952 there were no retirements, and consequently *no recruitment at all!*

Only in the mid-1960's civil servants again began to retire and leave place to young scientists. The first one to enter was a man who was to become the leader of the modern generation of French dynamical meteorologists.

3.4.5 The father of modern French NWP

When Daniel Rousseau joined Météorologie Nationale in 1963 he was in fact the first young scientist to join after a very long period. No wonder he was warmly welcomed by G.Dady, J.Lepas, R.Pône and others.

Rousseau says he was very fortunate to have Guy Dady as teacher in "Météorologie Dynamique et Prévision Numérique" at the "Ecole Nationale de la Météorologie" (ENM). It was thanks to Dady's support that Roussseau would spent a year abroad.

At this time Météorologie Nationale had taken a strategic decision of decisive importance. Instead of challenging the Anglo-Saxon dominance in meteorology by (for example) rallying French meteorology around "La Météorologie" as a French-speaking alternative to "Quarterly Journal" or "Journal of Meteorology", it was wisely decided to send out staff in all directions in the world, in particular to the US.

In 1965 Rousseau was sent to spend a year at Massachusetts Institute of Technology (MIT). Although the theoretical teaching in dynamical meteorology was very good in France (works by Rossby, Charney, Kibel, Gandin, Lorenz were very familiar), there was a neglect in numerical analysis. He therefore learned more at MIT in the Mathematics department (Prof. Gilbert Strang) and in the NWP lectures in the Meteorology Department (Roger T. Williams who replaced Norman A. Philips in 1965-66) than in France. He attended Jule Charney's course on "Planetary Fluid Dynamics". Charney was his adviser for a Master thesis, a work on the interpretation of the first days of Mintz' general circulation model[50].

3.4.6 Operational upgradings

In the same year, 1965, Jean Lepas and Jean Labrousse went to the USA to study the Weather Bureau organisation mainly for NWP and to get information on computer manufacturers. On their initiative a special section, CETI (Centre de Traitement de l'Information) was created within was Météorologie Nationale. It was dedicated to development of automatic data processing. CETI was in the operational department but served as a support both for operations and research. Operational forecasts were produced in real time, using a barotropic model with quite good results. But, according to Jean Bessemoulin, the French forecasters were not accustomed to interpreting 500 hPa patterns and did not know what to infer from them (Taba, 1982).

The research at Météorologie Nationale was mainly operational, while the university research was analytical and theoretical (Paul Queney). Paradoxically the initiative for a NWP computer came from the operations department. Later in 1965 the acquisition of a commercial computer came with a strong support of the research institutions (which gave scholarship to send people abroad and helped to finance the acquisition).

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[50] Daniel Rousseau would be in charge of the NWP research from 1969 to 1985.

From Guy Dady's Stockholm article:

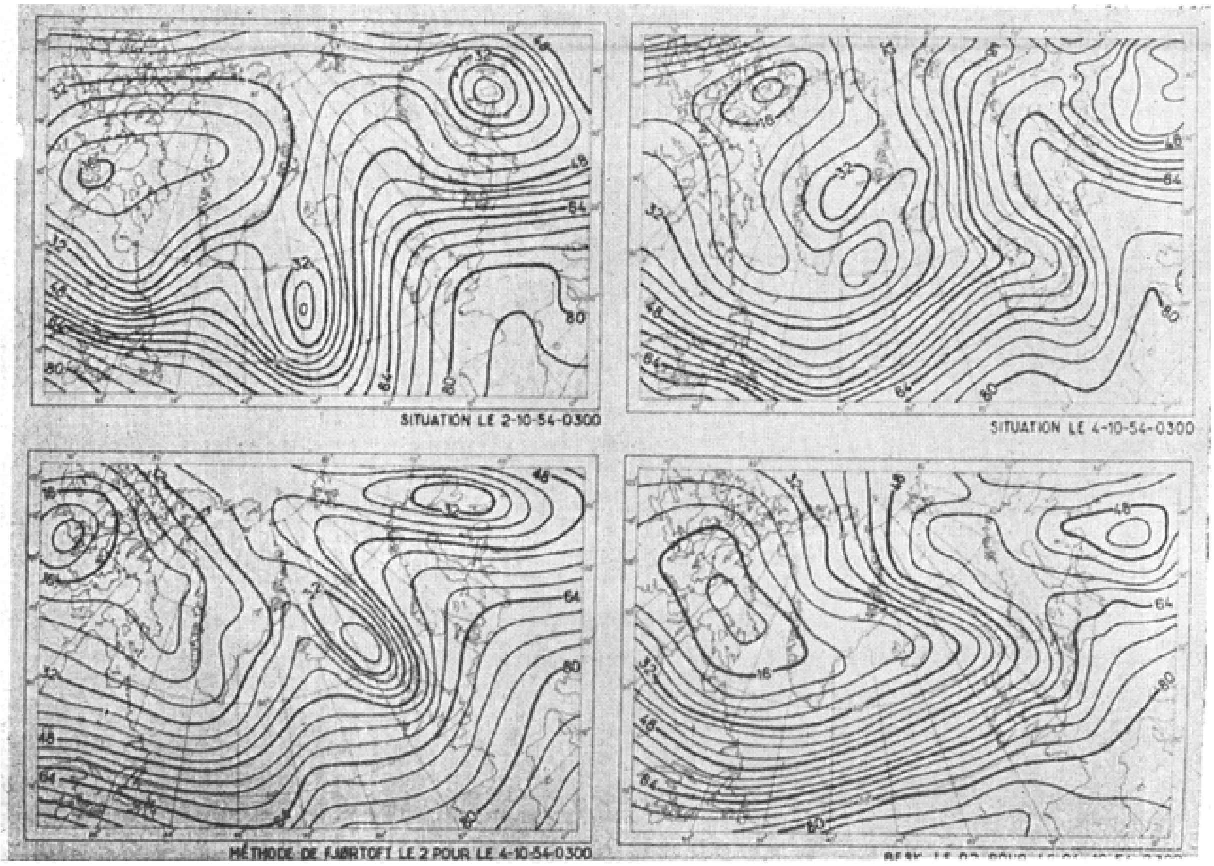

Fig.9: A contemporary comparison between two +24 hour barotropic forecasts, one made according to Fjørtoft's graphical method (lower left), an other according to a BESK integration of the barotropic vorticity equation (lower right). The initial analysis, 2 October 1954 03 UTC is on the upper left, the verifying analysis, 3 October 1954 is on the upper right. Until 1957 the radio soundings were launched at 03 and 15 UTC (Dady, 1955)

On 1 December 1967 the Météorologie Nationale acquired a new computer, a CDC 6400 (Rousseau, 1976). The choice of the computer for operational was taken late 1966 or beginning 1967 (Rousseau et al 1995).

The surge that French dynamic meteorology was to undergo probably owed a lot of its success to Guy Dady. His vision and teaching had intellectually paved the way for the technological advance. The interest French meteorologists displayed for spectral methods can to some degree be credited him. At a conference organised by the French Meteorological Society in February 1968 Dady's entire talk is devoted to spectral methods. He was keen, both in the talk and in an subsequent article (Dady, 1968) to argue on a non-technical level, avoiding "minute description of the techniques used". An echo of this work can be heard in a discussion about objective analysis with L. S. Gandin at the 1968 NWP Symposium (Tokyo, 1969, p. VI-53)[51].

1968 was the year of the "May Events" in France. Related or unrelated to this upheaval, there were profound changes at the Météorologie Nationale. It was the start of operational NWP with a single filtered barotropic model and an analysis system. In 1970 an acclimated version of "Shuman model" was run operationally. This first primitive equation model was not very successful in France and the routines were stopped in 1972. Until 1979 the main operational model was a filtered model.

Parallel to this primitive barotropic model was presented, there were balanced models with several levels and experiments were under way of multi-level primitive equation models[52].

3.4.7 French meteorologists go abroad

As mentioned above in 1965-70 there was a deliberate policy from Météorologie Nationale to send French meteorologists abroad to study. A policy to develop meteorological research had been engaged and the possibility of further training abroad was an element to attract young scientists. More than a dozen scientists benefited from this policy from 1965 to 1975:

Daniel Rousseau 1965 USA (MIT)

François Cayla 1967 USA

Claude Pastre 1969 USA (NOAA)

Frederic Delsol 1969 USA (GFDL)

Jean-Pierre Labarthe 1968 URSS (Akademgorodosk)

Giles Sommeria from CNRS, to NCAR around 1970

Claude Sinolecka 1970 URSS (Moscou)

Michel Rochas 1971 URSS (Akademgorodosk)

Jean-Paul Goutorbe 1970 USA (NCAR)

Jean-Claude André 1971 USA (NCAR)

Jean-François Geleyn 1973 Germany (Mainz)

Marc Gilet 1974 USA (Miami)

Jean-Pierre Chalon 1974 USA (NCAR)

Many of these have done work connected with NWP or modeling (Rousseau, Cayla, Delsol, Labarthe, Sinolecka, Rochas, André, Geleyn).

Other scientists from CNRS who benefited from the same program were Robert Sadourny who spent 1965 at UCLA and Olivier Talagrand who was at GFDL from January 1969 to July 1970. He had been working in a group headed by Pierre Morel since autumn 1966 on a project of the general circulation of the Southern Hemisphere using constant level drifting balloons. Morel arranged his visit through Smagorinsky.

3.4.8 France comes back in the lead of dynamic meteorology

The purchase of the CDC6400 allowed the scientists to develop the first elementary primitive equation models.

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51 Strangely Dady was reluctant to give too much importance to numerical methods. In the introduction to his 1968 textbook in dynamic meteorology, in which he asked Rousseau to write the chapter on numerical methods, he wrote: "The apparent simplicity with which the physical problems of the atmosphere let themselves be treated by multiplying the memories, by speeding up the calculations, deprives meteorology of reflections. It tends only to transform it into bad numerics, which only seeks 'fixes'. In our opinion it is a serious danger."

52 Rousseau developed the primitive equation model by elaborating a regional numerical model. First the coupling was studied between two primitive equation barotropic models, one large-scale model and a second fine mesh model. The experiment was successful (published in 1968) and demonstrated the feasibility of the approach. A two-dimension vertical model was done to study the vertical aspect before constructing the three-dimensional primitive equation model (1969). It was available in three versions: a hemispherical 360-km mesh (A), an European-Atlantic 180 km mesh (B) and a French for dynamical adaptation 36 km mesh (C). The B version was run daily from 1972 to 1979 coupled with the operational hemispherical filtered model except from 1974 to 1976 when the A version was also run.

This leads us into the 1970's where France for the first time since the days of Léon Teisserenc de Bort moved up in the forefront of meteorological development. A landmark was the 1970 summer school in Lannion in Brittany where Norman A. Phillips, Jule Charney, Verner Suomi and other leading meteorologists lectured for a French dominated, but international team of young scientists. It was important because it established new links between meteorologists from the Météorologie Nationale and other scientists in the meteorological field or in the space community.

The research division contained a NWP section (lead by Guy Dady till Rousseau succeeded him in 1969). They developed a primitive equation model for small-scale studies (barotropic, then 10 level model, mesh 36 km). From 1972 to 1979 a version of this research model (mesh 180 km) ran daily and was available to the forecasters in Paris, as a complement to the operational filtered model and the NWP from NMC, UKMO and DWD[53].

3.5 Algeria

In summer 1968 the Algerian Meteorological Service had, with the help of WMO, implemented a small computer. They were now looking for someone able to build an atmospheric model. The Director of IBM in Algiers introduced to the Algieran meteorologists a young French ingineer who was looking for a job in scientific calculation.

Jean Coiffier had a background in electronics and did not at this time know anything about meteorology. Opportunities of professional training and the military service oriented him toward computer sciences and numerical calculation rather than weather forecasting.

But the challenge was very attractive. Coiffier accepted and began to read various books in meteorology. He first practical job was the decoding of the TEMP contained on punched strips. Then he tackled the problem of the objective analysis by using IBM software based on geometrical interpolation. Finally he arrived to the numerical model and spent a lot of time to find an efficient method to solve large linear systems.

As pointed out by Coiffier (1995) it was very important in 1968 for a Met Service of a developing country to recognize the importance of NWP, to be able to mobilize resources in order to implement a small (but efficient) computer and to create a training school where this emerging science could be teached.

Two names stand out: Kamel Mostefa-Kara who was at the ENM at the same time as Daniel Rousseau, and Mahi Tabel-Aoul who was at the ENM in 1967. Coiffier does not exactly remember the exact position of both of them inside the Algerain administrative structure. K. Mostefa-Kara was probably reponsible for the Meteorology at the Algerian Transportation Mininstery while M. Tabet-Aoul, was in charge of meteorology inside the Civil Aviation Security. They had taken the initiative to launch, with the support of WMO, a project for the automatization of the meteorological applications (including of course NWP) and the creation of a training school (for all the former WMO levels of competence, i.e. up to the Class I).

At this time Coiffier met Jean Lepas who had been designated as WMO advisor for the Algerian Met Service. Lepas made Coiffier rapidly understand that the problem was simpler than he had imagined. Thanks to Lepas' good knowledge of NWP and enthusiastic support they were able to implement rapidly a complete numerical suit including TEMP decoding, Cressmann objective analysis and a filtered barotropic model providing charts on the printer. They had the opportunity to make a demonstration for the former WMO Secretary General Sir Arthur Davies when he visited the computer at the NMS Algiers.

Then, following the advices of Jean Lepas, Coiffier decided to join the Research Department of the Météorologie Nationale in Paris where he worked with Daniel Rousseau on the implementation of primitive equation models.

Two years later (probably in 1972) Jean Lepas worked for the WMO as the head of the "Institute Hydrométéorologique de Formation et de Recherches" (IHFR) in Oran. One day he asked Coiffier to come for a month in order to transfer the small NWP operational suit they

[53] From 1975 the efforts were concentrated on the development of the AMETHYSTE forecasting system in cooperation with the forecast division. It entered into operation in 1979 and was used until 1985 to be replaced by Emeraude and Péridot models (when Jean-François Geleyn succeeded Rousseau).

once had implemented. As the memory of the IBM 1130 computer was larger, they also increased the horizontal size of the covered area. Thanks to the help of Guy Der Megreditchian (trained in USSR but arrived at the Météorologie Nationale in 1971) they completed this suit by implementing a statistical interpretation of the model outputs.

Coiffier is keen to stress that the early development of NWP techniques in Algeria is mainly the work of Jean Lepas who had the luck to find a zealous worker! Jean Lepas also encouraged and personally helped many young students of the IHFR to carry out studies related to dynamic meteorology and numerical weather prediction.

3.6 Belgium

According to Platzman's questionnaire Jacques van Miegham with J. van Isacker and Defriese as collaborators worked on a barotropic model in 1955. The area was W Europe with constant boundary conditions. The computer was stationed at "Institute de la Réchèrche Scientifique" in Antwerp. Later they used the IBM704 in Paris. Operational forecasts seem to have started in 1962 when a larger computer became available. The model was barotropic with about 500 km grid hemispheric grid. It took 2 hours to run a 48 h forecast. It was replaced by a 2-parameter model in the mid-1960's with 300-700 hPa as the relative topography.

Van Isacker did not publish much of his work, which was rather theoretical dealing with the problem of reducing the multi-level isobaric data information from radio soundings by empirical orthogonal functions (EOF). In 1964 the Royal Belgian Meteorological Service changed from an IBM7070 to IBM7040.

Belgium would soon be one of the leading countries, which supported the establishment of the ECMWF.

3.7 Italy

In 1955 the Italian Meteorological Service began to study the numerical methods of dynamic meteorology and five years later the first daily forecasts were run. The key figure in this development was Sabino Palmieri. In 1958-1959 he spent about two years at JNWPU in Suitland (Maryland) as a young visiting scientist. At that time the Head of the Center was George Cressman, while senior experts in atmospheric modeling were F. Schuman, L. Vanderman and Aksel Wiin Nielsen. The computer was an IBM 704 with a number of ancillary equipments.

His task was to follow the progress in numerical forecasting and to develop an early model for the Italian Meteorological Service to be run on the extremely limited computing facilities expected to be available shortly in Italy.

On that occasion Palmieri produced and tested a simple barotropic model, partially linearised by using a space mean advecting field constant for 12 hour. After the implementation of the vorticity advection in a sequence of hourly time steps, at the end of the 12 hour run the height field was recovered from the predicted vorticity field by means of a Liebman sequential relaxation process. The procedure could be then iterated to extend the forecast in time. The integration area was half of the northern Hemisphere. Input data manually extracted from maps were the 500 hPa geopotential heights at points 300 km apart. The time required for a 12 hour forecast 12-14 minutes, for a full two-day forecast around an hour.

When Palmieri came back home in 1960 the IBM 1401 was installed at the Defence Ministry as a multipurpose computing facility and the Met Service succeeded in reserving a sufficient computing time (about 1 hour) for a daily run of the model. The model wasa used with minor refinements (to account for the effect of major topographic features) until 1963. At that time the introduction of new energy conserving advecting schemes and of higher resolution suggested a thorough redesign of operational models.

In 1967 a Numerical Forecast Centre was established. By then they had acquired an IBM 360/30 and in 1968 its core memory was enlarged to 64 KB. An automatic data analysis system prepared by Francesco Mosco became operational and a two-level quasi-geostrophic model by Lodovico La Valle replaced the barotropic model. This model used Lagrangian advection, forward time integration and influence functions. In 1970 the Centre was equipped with an IBM 360/40 computer with memory 129 KB. During the same year the number of levels incorporated in the quasi-geostrophic model was increased to four. Up to the early 1970's this work involved scientists Francesco Mosco, Ludovico La Valle and Carlo Finizio.

3.8 Canada

Already in the mid-50's NWP was on the agenda at the University of Toronto with plans to start a barotropic mode for a numerical weather prediction program using its computer. What came out of this has not been possible to establish[54]. Instead the next initiative seems to have come from Quebec, which since then has given Canadian NWP a certain Gallic flavour.

3.8.1 Kwizak and Robert

André J. Robert (1929-94) was a forecaster when he in 1953 became interested in NWP from what he could read in American and European journals. Since the forecast office was in the same building as the local meteorological bureau he used to attend and hold seminars about NWP.

In 1959 Robert was transferred to the newly created "Dynamic Prediction Division", led by Michael Kwizak. Kwizak was as interested in NWP as André and wrote to the HQ of the Canadian Meteorological Service in Toronto to ask for funds to buy time on the computer owned by Canadair. Not only did they get money right away, the got three times more than they had asked for.

> "-It was Kwizak who initiated NWP in Canada. He was at the bottom of everything. He had to fight the Director of the Canadian Meteorological Centre who really did not believe in NWP."

Their task was to construct a model and the group was expanded. Two new members joined the group: Robert Strachan and Amos Eddy. Strachan was asked to write programs to decode data and Eddy to create an objective analysis system using the Cressman scheme. In September 1962 they had got their own computer, Bendix G-60.

3.8.2. Operational NWP starts

After a years work of applied research and development, programming, and reprogramming, weather analysis and prediction with a non-divergent barotropic model was introduced on a routine operational basis on 1 September 1963. It was an evolved version with 28 x 32 grid points with grid lengths of 381 km with topographic effects and stabilising of the retrogressive planetary waves. Two runs were made daily for each of the 00 AND 12 UTC upper-air data periods. The 00 UTC run went up to +36 h available at 06 UTC, the second to +72 h available at 18 UTC. Daily operations revealed a problem at the boundaries, which was suppressed by smoothing.

During the second half of 1963 the research and development program in NWP at the Atmospheric Research Section at Meteorological Headquarters was completely taken up with changing over from the 700-point rectangular grid used in earlier work to the 1709-point octagonal grid used at the Central Analysis Office.

3.8.2 Towards baroclinic models

In late 1960 the Operational Development and Evaluation Unit of the Central Analysis Office at Montreal Airport began a systematic study of baroclinic models. This was done to increase the Unit's knowledge of, and experience with, more comprehensive mathematical prediction models. The choice of a particular baroclinic model was limited by two factors. The model had to possess operational potential and development flexibility. Secondly, its study must be feasible on the IBM 650 installation at McGill University, which was sometimes restrictive for this scale of study. In particular the model should be capable of predicting hourly vertical motions and similar dynamical effects. The group had by 1962 also developed a four-level baroclinic model on the 28 x 32 grid. It was intended to be implemented in the winter 1964, but it would last until 1969 after an upgrading of the computer in 1967 to enable an enlargement of the computational area to a 51 x 55 grid. (Lin and Laprise, 1994; Ritchie and Robert, 1994;

[54] In the membership journal of the Canadian branch of the Royal Meteorological Society there is in 1954 (Vol. 5, No 3) an enthusiastic article by Warren Godson on NWP: "These charts will add a new dimension to the tools available to the weather forecaster". He envisaged the day when raw data from the teletype circuits would be fed directly into an electronic computer which would proceed to grind out prognostics contour height and vertical motion data for various levels and time intervals (Godson, 1954, Taba, 1993).

Staniforth, 1994 plus “Progress Reports” from the 1960’s in the SMHI library).

3.9 Australia

Like their overseas colleagues, Australian meteorologists followed with great interest the development of numerical forecasting during the 1950’s. When W.C. Swinbank (1954) came back from a IUGG meeting in Rome he reported on the progress of NWP in the USA and Europe: “Australia would do well to notice the progress of these techniques.” Those who took the lead were the universities (mainly the one in Melbourne), but in the 1960’s the responsibility gradully went over to the Bureau of Meteorology.

3.9.1. Start at the university

In 1955-56 Ross Maine (1957) worked on Charney-Eliassen’s one-dimensional model. In March 1957 Uwe Radok had a seminar on NWP at the University of Melbourne. In summer 1957 W. J. (“Bill”) Gibbs (Taba, 1985 b), responsible for research, attended a meeting in Moscow where NWP was discussed. Radok had a new seminar in October 1958 and showed a barotropic forecast run on the University’s CSIRAC digital computer (Gibbs, 1954; Leslie and Dietachmayer, 1992; “Progress Reports” in the SMHI library).

Numerical forecasting started as part of the course provided by the Meteorology Department in the university. The first full barotropic forecasting program was written in 1958 by Dick Jenssen, a physicist working for an M.Sc. degree. He obtained it in 1959 with a thesis entitled “On numerical forecasting with the barotropic model”. Jenssen used data and analysed charts provided by the Bureau of Meteorology and the University’s CSIRAC computer, an old machine which at that time had only a small direct-access storage (512 words of 20 binary digits each) with a slow backing store of 1024 words. Owing to these limitations the first 24-hour forecast for a 21 x 17 grid with 300 km grid interval required 4 ½ hours of real time. Many of them came to grief over machine errors, difficult to evade in such protracted calculations.

The first barotropic forecasts with the UTECOM program was made in 1960 by Jenssen and Radok and presented at the Bureau of Meteorology’s Symposium on rain. They found that relatively minor changes in the initial 500 hPa pattern drastically altered the forecast.

This work, nevertheless, was immensely useful, and enabled Jenssen apart from becoming familiar with NWP in other countries, to develop a “quasi-stream function” which avoided the common problem of spurious anti-cyclogensis. In June 1959 Jenssen held a talk on computers in Australia (Jenssen,1959).

On the completion of his first barotropic forecast study Jenssen surveyed the larger computers available in Australia at the time (1959) and selected the DEUCE type computer UTECOM of the University of New Wales for future work on numerical analysis and NWP.

3.9.2 The Bureau of Meteorology takes over

It seems that the Australian Bureau of Meteorology became actively involved from about 1963 (Jenssen, 1966; Maine, 1962; Jenssen, 1967; Maine and Seamann, 1967; Clark, 1967). This year they started to develop an automatic analysis system, in cooperation with the university. The first computer was installed in 1968 and the first operational NWP ran in 1969. The domain was 24 x 36 with grid 254 km and the model was probably barotropic. Australia occupied a 1/3 of the longitudinal area (40º OUT of 120º). This area, where the effect of the south-western quadrant as felt, was kept the same up to 1990. The first hemispheric forecast was run in 1973.

3.10 New Zealand

The first numerical forecasting experiments in the Southern Hemisphere had been carried out in New Zealand (Kerr, 1954) with the Fjørtoft (1952) technique. From 1960 a barotropic model was run on an IBM 650 computer owned by the Treasury. Since 28 May 1963 analyses and 24 hour forecasts of the 500 hPa height were computed on a routine basis of 6 to 8 times per month. The forecasts were barotropic and quasi-geostrophic using a stream function. Forecasts based on the 00 UTC data were available in the forecast room at about 05 UTC. A longitude-latitude grid was used which varied between 322-547 km in a 12 x 18 grid point area.

3.11 Israel

Routine forecasting started in November 1962. The model was barotropic geostrophic and run once a day based on 00 UTC data, producing forecasts up to +72 hours of the 500 hPa field. The machine was a Philco 2000. The time to run a forecast was 1½-2 minutes. The 24 hours forecasts were comparable to conventional. The +48 hours often gave good indications but the +72 hours were never used. The forecast area was 60ºN-10ºW, 60ºN-40ºE, 30ºN-40ºE, 30ºN-10ºW. The resolution was defined in a peculiar variable latitude-longitude grid with gridpoints along every 5th meridian and a resolution which changed with latitude to make the distances between grid points equal to the distances between the above meridians.

3.11 USSR

Kibel and Blinova had already conducted numerical calculations of the atmospheric flow pattern in the early 1940's. See Phillips et al (1960) for a detailed and comprehensive description of Soviet NWP up to 1960. Work on operational system seems to have started in January-October 1954 when graphical methods were used to integrate a quasi-geostrophic model on 48 cases, each with a range of +24 hour. The forecasts were, however, inferior to subjective ones.

The first operational NWP was in 1959, according to a report by Bugaev to WMO in 1964. The model was barotropic.

In 1962 the "Hydrometeorological Service of the USSR" reported that they continued to use their three-level (850-500-300 mb) quasi-geostrophic model. The analysis was based on optimum interpolation and covered an area 26 x 22 points with a grid distance of 300 km. Forecasts were ran up to +36 and +48 hours. The forecasts were available 5-6 hours after the observation time. Twice a day a 24-hour forecast was run which contained the levels 850, 700, 500 and 300 hPa. This was similar to a model used in Leningrad (see below).

In 1965 the department for "Short-Range Weather Forecasting and Meso-meteorology" in Moscow ran operationally a three-level quasi-geostrophic model 26 x 22 with 300 km resolution and 37 x 37 grid, 850, 500 and 300 hPa levels. At that time they were also running, experimentally, a primitive equation model which had started 1964, first twice weekly, later five days a week. It consisted of 26 x 22 grid points with 300 km resolution.

In Novosibirsk a 5-level quasi-geostrophic model was run with 26 x 22 grid points and five levels at 1000, 850, 700, 500 and 300 hPa. In Leningrad a two-level quasi-geostrophic model was run for 850 and 500 hPa (Belousov, 1965)

3.12 Czechoslovakia

In 1952 a small group of scientific workers and students of the Meteorological Institute of Charles University in Prague, on the initiative of S. Brandejs, began to deal with methods of numerical weather prediction and related problems of dynamical meteorology. During the following ten years about 80 papers were published in Czechoslovakia on NWP. In 1963 three departments were dealing with NWP:

-the Meteorological Institute of Charles University (headed by S. Brandejs),

-the research department of the Central Hydrometeorological Institute (head of department J. Jílek)

-the newly set-up department of atmospheric circulation at the Meteorological Laboratory of the Czechoslovak Academy of Sciences (head of Department V.Vítek).

Although the small volume and limited means of Czechoslovak meteorology did not permit very complex research, research into numerical forecasting methods had a positive influence on the development of Czechoslovak meteorology since it oriented it along physical-dynamical lines against the then prevailing statistical approach.

3.12.1 Preparation for NWP

After introductory studies of Kibel's method and Sutcliffe's development equation the research moved into developing a barotropic model. Czechoslovak meteorology at that time had no high-speed computer at its disposal. A successful but isolated attempt at numerical integration of the barotropic vorticity equation was made by means of a Fourier transformation on a calculating punch machine made by Aritma, a national enterprise. A few years later the barotropic vorticity equation was solved on a Soviet high-speed computer Ural 1 at the Computer Laboratory of Transport.

The computer Ural 1 (and later Ural 2) had a very atypical input via perforate film bands and thus telex machines could not be used: the input data were then brought in by car (15km distance) and perforate to the film band on the spot, then verified once more. Results were brought back again by car and they were manually analysed. Although the 24h forecast of 500-hPa level took 15 minutes on the machine, mostly manual data pre-processing took more than 4 hours and manual post processing needed another 30 minutes. The experiment on the Ural 1 computer was encouraging for further experiments since its showed it was possible for a barotropic model in a large region even on a relatively small standard computer. The time handicap was a main reason why the everyday compu-tations had to wait till 1966.

3.12.2 Operational forecasts in 1966

The routine operations started 1966 on a British-made computer LEO 360 also at the Computer Laboratory of Transport. The model was barotropic at the 500 hPa level. Already now the Czechs had developed a system for objective analysis (based on the optimum interpolation method but with climate values as first guess). Calculations were made in a grid of 24 x 20 points for a grid length of 315 km using uncentred differences in the time integration. A barotropic forecast for 24 hours took about 4 hours. This promising activity was brought to a halt in August 1968 with the invasion by the Warsaw Pact military forces.

3.12.3 Some personalities

But the Czech meteorologists did not relinquish. Today's young Czech meteorologists, when they look back in history want to highlight some individuals. Besides heads of research at the University, Academy of Sciences and Hydrometeorological Institute, they want to mention Miroslav Škoda. It was he who really pushed for getting things working in routine. Given the circumstances it was always at the end more or less a lost battle in competition with NWP products coming from western services. However, he never gave up and tried to build a new team, to find a better computer, and so on, practically until the beginning of the nineties.

Although at those times of socialist Czechoslovakia his dream was far away, it is today regarded that the most important outcome from Miroslav's effort was a training of a new generation of meteorologists and maintaining some minimum level of NWP know-how. For training and education in NWP he was not the only one. There was also Michal Baťka, a mathematician specialised in numerical methods, who wrote the program of the first model together with Jaroslav Vocetka (they both worked at the Computational Laboratory of Transport where they were discovered by Škoda).

In 1966 Michal Baťka went to the Faculty of Mathematics and Physics where he worked with professor Brandejs on a model using the omega equation. Then he taught students numerical methods in meteorology. In the second half of the eighties Martin Janoušek and Radmila Brožkova were his students and they worked out a HPE SI-SL model, operationally running on a technologically obsolete computer EC1057 made in ex RDA, but at least located in CHMI.

The pioneering NWP trials, fighting hard for a survival at the time of cold war and Soviet occupation, contributed to that the Czechs do not think that they have such bad position today. They regard themselves as a small but consolidated NWP department in CHMI where they operate the ALADIN model on a small NEC-SX6 machine. But it is another story...

3.13 People's Republic of China

The main source is the article in Bull AMS by Blumen and Washington (1973). According to this article work on NWP seems to have started in 1954. Up to 1958 operational activity was confined to graphical integrations of a two-layer model. Barotropic forecasts up to +48 hours were run, probably on a computer, started in 1960[55]. The computational area seems to have been large, because they experienced problems with the retrogression of the longest planetary waves. In the first half of the 1960's, before the political turmoil during the Cultural Revolution, baroclinic models

[55] I have been told that a Chinese meteorologist, Dr Koo at a seminar at SMHI in the late 50's or early 60's showed a hand calculated hemispheric +24 hour 500 hPa forecast.

were run on a research basis and there were experiments with primitive equation models. During the same time the operational +48 hour forecasts improved by the introduction of a divergence term which mede them superior to manual 500 hPa forecasts.

3.14 Finland

Numerical weather prediction (NWP) in Finland has its roots at the Department of Meteorology of the University of Helsinki. Dr. L.A. Vuorela, later Professor of Meteorology at the University, got in contact with the NWP in the early 1950´s as a visiting scientist in Sweden and the USA. He gave the first lectures on the NWP in Finland.

In summer 1961 Daniel Söderman worked as an assistant to Erik Palmén, who was investigating moisture convergence over Sahara, in some general circulation polemic with Victor Starr. Palmén was employed by the Finnish Academy of Science and had access to the computer ELLIOT803 at the Technical University. Palmén became quite impressed about what the computer could do, so in autumn 1962 he sent Söderman over to Stockholm to join Bo Döös and Lennart Bengtsson's aerological division at SMHI. Söderman worked part-time work at SMHI from December 1962 until 1967. There he participated in the building up of the operational forecasting system on the SAAB D21 computer.

3.14.1 First NWP experiments at the university

The first modelling experiment was performed in April 1964 at the University of Helsinki Department of Meteorology with the financial support of The Finnish Society of Sciences and Letters. The young enthusiasts in the area were Daniel Söderman and Juhani Rinne with a barotropic model on an IBM 1620, with one time-step requiring eight minutes of computations for an area with 360 (18 x 20) grid points. The forecast run was terminated by printing out the text *"Olen tehnyt historiaa"* (I have made history), which proved to be an unexpectedly factual statement as the forecast had progressed backwards in time due to a sign error in the code.

This initial test run was followed by sporadic experimentation on the Technical University Elliot 803/503 system, and by an annual training course on numerical weather prediction at the University of Helsinki.

Barotropic forecasts were then carried out once a month, on the 15th primarily for educational and training purposes. Due to the relatively low internal speed of the available electronic computer, the computing time for a 48 hour forecasts on a area of 360 grid points was about 8 hours.

3.14.2 Operational runs in 1970

At Finnish Meteorological Institute (FMI) there was for long no possibilities to acquire a computer. In the 1950's its Director Matti Franssila hoped at most to be able to influence the purchase of a computer at the University.

When Söderman and Rinne started their experiments they were given gave administrative support and encouragement by Franssila and Dr. Eero Holopainen, the acting Chief of the Weather Forecasting Section of FMI.

In September 1967 a computer group was formed at the FMI with Eero Holopainen as leader. At a Nordic Directors Meeting in Helsinki in September 1968 it was reported that since March barotropic forecasts were run up to +96 hours on a copy of the Swedish model.

At the end of 1968 the first experiments with 1-2 day forecasts started with the Elliott 503 at Helsinki Technical University.

The year 1969 saw revolutionary changes at FMI. In April 1969 a computer division was set up at FMI with Daniel Söderman as leader. A decision was made to purchase of a Datasaab D21 for FMI with the full SMHI software package included in the deal.

Operational NWP started in February 1970 when Holopainen had just come home from a fact-finding mission to Sweden and the USA.

The forecasts were based on the three-level filtered model developed by Lennart Bengtsson and Lars Moen and run daily at 00 and 12 GMT in a 300 km grid to +36 hours and thereafter barotropically at 500 hPa until +96 hours. In addition 150 km forecasts were run to +36 hours at 12 and 18 GMT during summer.

Later the Swedish model was run four times a day at 300-km resolution, with boundaries transferred to an inner 150-grid version.

FMI had two years earlier moved to new locations in Helsinki. The Head of the Weather

Department Sulo Nestori Venho, who was in charge of the new building, told Söderman that the house had been planned on the assumption that it would never house any computers. When Söderman left ten years later (for ECMWF) there were quite a few (30?), and a staff of 42 in the division.

3.15 Denmark

Denmark's involvement in NWP can be divided into three periods: one early, rather pre-mature, in the mid-1950's, a second period of half-measures in the late 50's, 60's and 70's and a third period from the mid-1980's which brought Denmark up to the forefront of NWP in Scandinavia.

3.15.1 The premature years

During his time as professor at University of Copenhagen 1951-55, Ragnar Fjørtoft gathered around him three enthusiastic students: Aksel Wiin-Nielsen, Hans S. Buch and Harry van Loon. The project was not to explore NWP but Fjørtoft's own graphical method. Wiin-Nielsen (1990, 1997, 2001) has given vivid descriptions of this work. The group dispersed when Fjørtoft went back to Oslo in 1955. Wiin-Nielsen moved to Rossby's institution in Stockholm, Buch went into other fields in meteorology and van Loon emigrated to the US.

3.15.2 The period of unfulfilled attempts

In 1956-57 another Danish meteorologist, now from the Danish Meteorological Institute, Ole Lang Rasussen visited Rossby's institution to learn about NWP. On his return he created a barotropic model which was run irregularly, producing forecasts to +48 h forecasts over an area of 32 x 40 gridpoints with 300-km intervals. The calculations were made on DASK, a Danish copy of BESK, at the Danish Computer Central. When Rasmussen left DMI in 1961, the work was taken over by J. Walter Larsen (later head of the Computer Division). Barotropic forecasts were run irregularly. In 1969 they started to develop an objective analysis scheme. Only in 1971 did the Danish Meteorological Institute acquired a computer, RC400 and in 1973 a barotropic model started with 25 x 23 points 254 km apart.

The first WMO "NWP Progress Report" for 1974 contains a brief description of a three-parameter baroclinic model managed by Bjarne Byrnak and Gorm Raabo-Larsen. In the following three years there are no entries from Denmark at all. One reason might be that Raabo-Larsen then worked in England to help set up ECMWF. In 1978 there is again a short report from Gorm-Larssen and his co-workers Anne Mette Jørgenssen and Niels Woertmann-Nielsen. There is a mention of plans to develop and implement a short-range, fine mesh numerical NWP model for Danish forecast areas, including Greenland and the Faeroe Island, improve the data assimilation system and study the ECMWF medium range NWP system.

What happened to these plans we do not know, because there is no entry from Denmark for several years (1981 and 1982 are missing from my SMHI library).

However, in the WMO Progress Report for 1986 we are told that fundamntal changes were underway. In 1985 the Nordic countries fulfilled a long-time ambition, first aired in 1955, *to co-ordinate their NWP work*. A project had been initiated in 1984, with Bennert Machenhauer as leader to create a High Resolution Limited Area (HIRLAM) model. The staff, located in Copenhagen, consisted of two members from each participating country. A special agreement had been made with the Dutch meteorological service, which contributed two scientists. Ireland and Spain joined the HIRLAM group later.

The role HIRLAM played for the development of NWP in these smaller countries deserves its own history. It might be surmised that the profound changes in Danish meteorology were related to Aksel Wiin-Nielsen's appointment as Director of DMI in 1983. But the practical initiative from the Danish side seems to have been taken by the Head of the Meteorological Division, Lars P. Prahm, from 1987 the Director of the DMI and recently appointed Director General for EUMETSAT.

3.16 Netherlands

Operational NWP seems to have started in the late 1960's at the Royal Meteorological Institute of the Netherlands (KNMI). It had been precluded by early research with the computer of the University of Utrecht.

There was some resistance to NWP as such. There is anecdotical evidence from the early

1960's that the Director (Warners) was heavily against computers and tried to keep them out as long as possible.

Numerical Modelling at KNMI started late, with J. van Galen in the mid-sixties. Other people involved were D.Bouman, W.J.A. Kuipers[56] and H. Timmerman, under the Head of Research F. H. Schmidt. They used a Phillips computer EL-X8 with 16K core memory. The language was ALGOL 60.

There was an operational limited area model BK3 (Baroclinic 3 levels) as early as 1968-70 after Lodewijk Heijboer had joined KNMI. There was, as a one of my sources said, a "decent activity", although it was always a bit subdued relative to what they assumed the British and Americans were doing. The activity is reflected in a (Dutch speaking) KNMI Scientific Report WR 69-3 "Speciale Projectgroep Numerieke Voorspel-methoden" from 1969. It describes, extensively, the code to be implementend on the computer.

Chapters are

1 Introduction

2 Automated Data extraction

3 Data Correction

4 Objective (Cressman) Analysis

5 Barotropic model

6 Three parameter baroclinic model

7 Numerical calculations of the water levels along the Dutch coasts

The introduction mentions that the machine is there and soon will be working with a model with a resolution of 375 km.

No authors are mentioned but the various chapters in the report were written by Kuipers, Galen, Timmerman and edited by Bouman

3.17 Norway

The development of NWP in Norway is, at least for an outside historian, shrouded in some mystery. The facts are the following: In 1961 DNMI, (the Norwegian Meteorological Institute) as one of the first meteorological services, acquired a computer, a FACIT EDB. From 1962 to 1971 two types of operational NWP models were run, one barotropic for the 500 hPa surface, another baroclinic 2-parameter model using the 300-700 hPa thickness[57]. Forecasts were run up to +48 hours in a 600 km grid (probably reduced to 300 km at some stage). There was no explicite forecast for 1000 hPa, the tendencies for that level were calculated from the tendencies at 700, 500 and 300 hPa (Ökland, 1963a). In 1971, when DNMI got a new computer, the 2-parameter model from 1962 was replaced with a four-level baroclinic, balanced model borrowed from a 1965 Japanese version.

This is not bad in comparison with other small European nations at that time. *But meteorologically Norway was not a "small" country!*

Indeed, Norway belonged to the meteorological "super powers". It had a long history of outstanding geophysical research with an impressive list of scientists: Henrik Mohn, Gustav Adolf Guldberg, Vilhelm and Jack Bjerknes, Carl Ludvig Godske, Jörgen Holmboe, Sverre Petterssen, Einar Höiland, Ragnar Fjørtoft, Arnt Eliassen, Enoch Palm – just to mention those of world fame.

But there are more things that are difficult to explain. At about this time, in the early 1970's, when other small European nations came together and planned what would become the ECMWF, *Norway decided to stay outside*. Norway signed the convention, but did not take active part until 1988. Since then only a few Norwegians meteorologists have worked there, and only for short periods.

When one tries to understand what lies behind this one encounters some frustration in the development within Norwegian meteorological community. On one hand there is a bitterness of lost opportunities and a feeling that Norway took the wrong track. There is also a certain reluctance to discuss the matter, since it would unavoidably focus on three meteorologists of great fame and stature: Einar Höiland, Ragnar Fjørtoft and Arnt Eliassen.

Einar Höiland (1907-74) was a student of Vilhelm Bjerknes and the leading meteorological theoretician in post-war Norway. He was

[56] One of the fathers to the Hanssen-Kuiper score.

[57] I do not paint a too dark picture. Until recently, before I alerted my Norwegian colleagues to the error, the DNMI website said that the barotropic model was the *only* operational model 1962-72.

regarded as over-theoretical and his work was characterised by a high degree of formalism. He was plainly interested in hydrodynamics and wrote hydrodynamical papers, very mathematical which had nothing to do with weather and climate. He had command of Further Education. Höiland had a strong will and nobody dared to argue against him.

Ragnar Fjørtoft, is well-known for his participation in the first ENIAC run 1950 (Taba, 1988 a) But during his period as professor in Copenhagen 1952-55 he concentrated on refining his graphical method of integration of the barotropic model (Wiin-Nielsen, 2001). When he returned to Oslo and became Director for the Meteorological Service his aim was still to develop and promote his graphical method rather than domestic NWP.

He tried to develop his method by an optical analogue method where the calculations could be evaluated by passing light through two film sheets whose transmittance represented two space functions. However, in spite of heroic efforts involving the calibrations of thousands of small pieces of film with various degrees of blackening, the results were discouraging and the idea was abandoned.

It is true that the Fjørtoft (1952) method was genial, that it helped many poorly equipped meteorological services, to get a taste of NWP. It is also true that this quasi-Lagrangian method helped to inspire the use of semi-Lagrangian numerical techniques (Haug, 1956; Ökland, 1962, 1963b)[58]. But it should have been clear to Fjørtoft that his graphical technique would have been extremely useful only if computers (and Rossby's barotropic concept) had been born a few decades earlier. In the long run his method would not be able to compete with the computer based NWP.

Finally there is Arnt Eliassen (Taba, 1997 c), or rather there are two. There is the "International Eliassen", who works with Charney, Rossby and others on NWP from 1948-65. This Eliassen is a towering figure in the history of NWP. And then there is the "Norwegian Eliassen" who produced fundamental research on important aspects of dynamic meteorology, but seemed to have stayed outside active involvement in domestic NWP. Concepts such as "Eliassen-Palm flux" and "Sawyer-Eliassen theory" on cross-frontal circulation bear witness to this work. It is true that Eliassen was based at the university and that NWP was mainly a duty of the meteorological institute. But as we have seen in other countries, it was quite common for NWP to be developed as a cooperative project. It would have been particularly favourable in Norway because it is just a few minutes walking distance between the two institutions!

Eliassen and Fjørtoft are prominent in the history of NWP – except in their own country!

To understand why Norway stayed outside ECMWF another meteorologist comes into the history. The Norwegian Meteorological Institute is governed by the Church and Educational Department. A key civil servant there was Olaf Devik (1886-1987), one of Vilhelm Bjerknes' first students. Devik seems to have maintained his influence also after his retirement in 1951. The policy of the Church and Educational Department was to follow the advice of Ragnar Fjørtoft and Arnt Eliassen – also after *their* retirement. Fjørtoft and Eliassen recommended the ministry that Norway should not join the ECMWF: *-The USA was already preparing medium range forecasts globally, so why set up another centre?*

It is true that there was also scepticism in Norway that the ECMWF was politically rather than scientifically motivated. At that time (1971-74) UK was not yet admitted to the EEC. The Convention establishing the ECMWF entered in force on 1 November 1975, having been ratified by a dozen Member States – half a year after the British in a referendum decided to join the EEC.

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After surveying NWP in seventeen countries the reader might think that "it's all over".

-Well, it isn't!

The greatest story is still to be told.

The reason why British NWP is given a special chapter is not only because of the story itself, but it, like an old Shakespearean drama, brings up issues, which are still relevant today.

[58] Platzman (1963) noted in his report from the successful international symposium on NWP in Oslo 11-16 March 1963 that Lagrangian or quasi-Lagrangian methods were "gaining ascendancy in some circles, in particular in Norway". He was also impressed by Kaare Pedersen's precipitation forecasts from a quasi-geostrophic model and by Ragnar Fjörtoft's "remarkable discover" of balanced equations of a "mixed type".

4. PATRIOTISM AND MATHEMATICS - BRITISH NWP 1948-1965

4.1 Introduction

> "I do not think your views on [the UKMO] in Dunstable will ruffle any feathers of those who worked there", John Sawyer to the author, 1993 (see also Appendix 5)

The Meteorological Office of the United Kingdom (UKMO) began its operational NWP in November 1965. This marked the start of a very successful activity that would gradually bring it to the forefront of NWP development, where it stands today.

However, except for a computer orientated article by Hinds (1981) very little has been written about the UKMO's road to the operational start of NWP. In an historical exposure about early NWP in general one of the leading actors, Fred Bushby (1986), doesn't tell very much about NWP in his own country, nor do Sir John Mason and John S. Sawyer in their respective interviews for the WMO Bulletin (Taba, 1995, 1997). This scarce publicity is in sharp contrast to the rich literature that is available on L. F. Richardson and his pioneering 1922 work (Ashford, 1985).

The lack of historical accounts of the UKMO work on NWP might be explained by lack of information; the British civil service has a reputation of secrecy. Adding to this, the UKMO belongs to the Minstry of Defence (previously the War Office). So we should not be surprised if most of the NWP development in Britain were kept under a lock. But the opposite is the case. *The published material on early British NWP is overwhelming.*

From the start more than fifty years ago anybody in the world have been able to follow their work in great detail year by year, almost month by month. The UKMO's monthly "Meteorological Magazine", the Royal Meteorological Society's "Quarterly Journal" and their popular monthly magazine "Weather", gave extensive coverage of the NWP work during the 1950's and 60's. Apart from publishing reviewed papers they also reported from meetings and, uniquely, the discussions that followed. The British National Meteorological Library has for long times provided access to declassified internal memoranda.

And there was much to report on. The UKMO actually started to work on NWP about six years before many other national meteorological services. The work was carried out on state-of-art computers by qualified mathematicians and programmers, and was guided by scientists who were among the most prominent in the field of dynamical meteorology. The first numerical integration was made at Christmas time in 1953, almost at the same time as Charney's integration of the Thanksgiving Day storm and the first successful integration in Sweden. But whereas the Swedish model was barotropic, the UKMO model was baroclinic (like Charney's) and also depicted the surface pressure development.

Still, in spite of this early start, it would be more than ten years before the UKMO went operational with NWP; and when it did, it was with great reluctance from its own staff and had to be almost forced though by their newly appointed Director John Mason. The extensive media coverage of this truly historical event contrasted sharply to the low-key presentation given at the back pages of "Meteorological Magazine" under the laconic headline "Press Conference".

These strange circumstances normally indicate that there is a story to be told – and indeed there is. The British road to operational NWP 1948-65 was marred with problems and emotions worthy a BBC drama or a Hollywood blockbuster. Here we find meteorologists with a mixture of admiration for the computer and fears about the future of synoptic forecasting. There are frustrated mathematicians who see their forecast getting worse when they ought to be improving. We can, almost verbatim, listen to eminent scientists who put the right questions, but get the wrong answers. And finally the whole drama is imbued with feelings of national pride and independence.

One good reasons to treat this well-documented historical development in more detail, is because the problems were not unique to British meteorology. The same or similar problems probably affected other centres; although there is little or no documentation. Indeed some of the issues are still debated today.

4.2 The background to British NWP

> "[If] charts could be constructed showing the atmospheric conditions prevailing both at sea level and at some suitable fixed altitude above it, and if the nature of atmospheric circulation were at the same time thoroughly understood, the present [worthless] empirical method of long-period weather forecasting, would soon become resolved into one involving the scientific process of inference and deduction" (Bonacina, 1905).

When the time was "ripe" for NWP after the Second World War few countries or meteorological institute were so well equipped and prepared for NWP as the the United Kindom and their Meteorological Office (UKMO). Three factors contributed to this:

-The growth of the organization during the war: From 750 employees in 1939 the UKMO had expanded almost tenfold during the war to 6800 in 1945. The increase brought some consternation to the peacetime organisation and 1946-47 saw a general re-organisation of the whole activity, where greater emphasis should be put on research. In 1941 the Director Nelson K. Johnson had set up the "Meteorolo-gical Research Committee" (MRC). Its purpo-se was not only to give advice on the general lines of meteorological research, but to assist in such research, discuss reports and make recommendations for further action. Its mem-bers, who were paid a small fee, included lea-ding meteorologists such S. Chapman (chair-man), D. Brunt, G. M. B. Dobson, C. Normand, G. I. Taylor, O. G. Sutton and P.A. Sheppard, together with the Director of the UKMO and representatives from the Navy, Air Force and civil aviation (Scrase, 1962). In 1945 the MRC was increased with five more meteorologists. In 1942 another group, the "Gassiot Committee", was set up as a link to the broader scientific community in the Royal Society. Last, but not least, there was the Royal Meteorological Society (RMS) with its prestigious "Quarterly Journal" and, from 1946, its popular journal "Weather".

-The development of British made computers:

Most of the impetus to British post-war computer technology came from the war time Colossus machine. It had been constructed to aid the mathematicians in the Bletchley Park team to decode enemy telecommunication traffic. In the years after the war there were three influential computer centres in Britain:

-Manchester University constructed its first computer in 1947 with the help of some of the Bletchley Park team. Its first program ran in June 1948. The government provided funding for a scaled up version, the "Ferranti Mark 1". Alan Turing joined in 1948 to become assistant director of the work on the MADAM (Manchester Automatic Digital Machine).

-Cambridge University started to operate their own Electronic Delay Storage Automatic Calculator (EDSAC) in May 1949. It was constructed by Maurice Wilkes and was the first machine to use delay lines to store information. When the catering firm J.Lyons decided to investigate the use of electronically office computing they chose EDSAC as the prototype. The first Lyons Electronic Office (LEO) machines ran in September 1951.

-The National Physical Laboratory in Teddington established a mathematical division under Alan Turing in 1945 to continue and co-ordinate various computing projects initiated by the war. In 1946 James H. Wilkinson joined the group. The plans were for an ambitious Automatic Compu-ting Engine (ACE). However, delays led to that the first calculations were made in 1950.

It is not clear how much of this that was known to the outside world. As mentioned in 2.11, in 1952 it came as a surprise to the audience at the NWP conference in Stockholm in that the UKMO had access to computers.

-The development of advanced dynamical concepts:

To put L. F. Richardson's famous 1922 work into perspective one must first acknowledge V. Bjerknes' declarations in 1904 and 1913 about the need to calculate the weather mathematically. These sentiments are also present in a series of articles by Leo C. W. Bonacina (1882-1975) who strongly champio-ned Bjerknes' programme (Bonacina, 1904, 1905, 1913)[59]. The Director of the UK Met

[59] Bonacina seems to have had a feeling for the "butterfly effect" since he wrote that two atmospheric states, except for "minute differences" would finally establish "opposite types of weather" (Bonacina, 1904)identical

Office, Napier Shaw, toyed with ideas of objective forecasts and even wrote an outline of a meteorological version of Newton's "Principia" (Shaw, 1913, 1914). Shaw had in 1905 instigated the "Monday Evening Discussion" to give members of UKMO staff, their friends and others interested in meteorology an opportunity to discuss current scientific papers of general interest.

4.3 1948 Early planning

> "A meteorological electronic brain could be constructed to deal in orderly manner with the immense amount of information available daily and provide much more accurate [pressure forecast charts]. C. S. Durst (quoted by Gold, 1947, p.185)

On 25 May 1948 a meeting was held between the UKMO and Imperial College to discuss "The Possibilities of Using Electronic Computing Machines in Meteorology". Present at the meeting were Drs G.C. McVittie, R.C. Sutcliffe, C.S.Durst and E.T.Eady.

George C. McVittie (1904-88) was a mathematician with insight into meteorology from Queen Mary College, London, who soon left the UKMO.

Charles S. Durst (1888-1961) joined the UKMO in 1919 and became in the 1920's instrumental in introducing the Hollerith punch card system for the storage and analysis of observations from the British Empire. He gradually acquired a deep knowledge both of the world's synoptics and of the state-of-art data handling[60]. In 1947, learning that the EDSAC computer was to be built in Cambridge he re-read Richardson's book[61].

Reginald C Sutcliffe (1904-91), originally a PhD in statistics, was employed by the UKMO in 1926 to map the world climate for the expanding "Imperial Airways" civil traffic (Taba, 1981). Soon he was attracted to synoptic meteorology and was, together with Durst, one of the promoters of the Bergen school concepts. Later they introduced the concept of "quasi-geostrophy". In 1948 Sutcliffe became Assistant Director and Head of Forecasting Research.

Eric T. Eady (1915-66) joined the UKMO in 1938 and served as an aviation weather forecaster during the war, specialising in upper-air analysis and forecasting for bomber groups. He then resigned to become a graduate student in meteorology at Imperial College. When the department did not accept him he registered as a student in the Department of Mathematics, where he wrote a doctoral thesis on "The Theory of Develop-ment in Dynamical Meteorology". The novelty and importance of his work were appreciated and he, in 1949, was welcomed into the meteorological department (Charnock, 1993).

At this first meeting to discuss NWP a difference in opinions emerged that would be a recurring theme for many years: *should numerical techniques be used mainly for research or operational activity?* Whereas Eady hoped to pose to the EDSAC machine simple problems on the effect of perturbations on a uniform baroclinic flow of air, Sutcliffe argued that it was important that actual meteorological situations should be put to the machine to discover if it were capable of solving these situations (Appendix 1).

There were further discussions on the difficulties with boundary conditions and on the limitation of accuracy of wind measurements. They soon realised that further progress could not be made without the presence of some expert familiar with "mechanical methods" in computation.

James Wilkinson from NPL attended the second meeting on 11 June. Sutcliffe and Eady outlined the meteorological problems. Wilkinson made the point that the gain with electronic computers was *speed*. Any problem had to be put to the machine in the same form as if it were to be solved by a very large number of office calculating machines working for a very long time. He did not think that the NPL machine, which would be ready in about two years time, with its limited capacity,

[60] Although Durst advanced to Assistant Director he remained rather unknown outside the UKMO, because his responsibilities were with the MO(9) Special Investigations division, deAnonymous, 1962; aling with secret military work (Meteorological Magazine, 1957; Best, 1962). During the war he must obviously had first hand know-ledge about the rapid advance in the computing technology.

[61] John Saywer, personal communication 1993. Early in 1948 Durst and Sawyer went the Mathematics Branch of the NPL where they met James Wilkinson (or possibly Leslie Fox). "Durst wanted to know if [the computer] could do what L.F.Richardson had proposed in the 1920's." They came back realising that they needed to learn more about numerical mathematics.

would be able to deal with the general problem of weather forecasting, possibly minor idealised problems, as suggested by Eady.

After further discussion it was agreed that the UKMO should recruit or establish contact with someone who was familiar with the methods of computational mathematics and synoptic meteorology, with emphasis on the first aspect.

Eric Eady had been asked to keep the top scientists of the MRC informed. This seems to have happened at its meeting on 24 June, because then the discussion was about the possibility of computing in connection with short- and medium range weather forecasting[62]. Another result of the meeting was that electric desk-calculators were obtained. The staff in the Forecast Research Division then spent "many a boring hour", using these calculators, which even lacked the facility of automatic multiplication (Hinds, 1981).

4.4 The Sutcliffe development equation 1939-50

> "...Rudimentary though [my theories] were, they were ahead of anything published at that time in that they were dynamically based, quantitative in principle, dealt with the atmosphere as a baroclinic continuum and were not obsessed with the cyclone problem." (Sutcliffe to Pedlosky 1982, in Phillips, 1989)

The problem now was to define what kind of mathematical model to present to the computer. This was a crucial issue. There were two schools: the Chicago school favoured a barotropic model, whereas the UKMO were inclined to a baroclinic approach. Forecasts of precipitation was seen as the quintessence of weather forecasting and that demanded forecasts of vertical motion, something which only baroclinic model were realistically capable of. On the other hand they demanded a lot of the limitated computer capacity.

The questions that had to be answered were not only about the mathematics of the two different approaches, but how they related to each other and, above all, to the real atmosphere.

[62] Two months later the MRC president, Sidney Chapman, made a strong case for international meteorological research at the UGGI meeting in Oslo. Among problems, which could be more satisfactorily handed over to an international body than left to the efforts of the national organisations, he cited the computation of forecasts by "electronic methods" (Durst, 1948).

The quasi-geostrophic concept, and indeed the word itself, goes back to papers by Durst and Sutcliffe (1938, a,b) and Sutcliffe (1938). At that time "development" was seen as identical to pressure changes, which only could be calculated by considering the total mass divergence in a vertical column, which Margules had shown was practically impossible. The breakthrough came when Sutcliffe (1939,b), inspired by W. H. Dines, redefined the problem: "development" was now the difference between the divergences at the uppermost and lowermost levels of the troposphere. This could be approximated by the ageostrophic wind. Sutcliffe introduced the thermal wind, $V_T = V - V_0$, or "shear wind", as he preferred to call it, as a link between these two levels. After some manipulations he arrived at an equation in Cartesian coordinates

$$\frac{dV}{dt} \approx V_T \cdot \nabla V_0 + \frac{dV_T}{dt}$$

which allowed him to infer "development" from the surface and upper-air patterns (the first term) and the time evolution of the thermal pattern (the second term)[63].

After the war Sutcliffe "dusted off" his 1939 paper and presented it with pressure coordinates[64] and, probably influenced by Rossby's Chicago school, with vorticity instead of wind components (Sutcliffe, 1947)[65]. He also included the latitudinal variation of the Coriolis parameter (the "β-effect"):

$$Development = f(divV - divV_o) =$$
$$-V_T \frac{\partial \zeta_T}{\partial s} - 2V_T \frac{\partial \zeta_0}{\partial s} - V_T \frac{\partial f}{\partial s}$$

where the first term was the vorticity advection or "thermal vorticity effect", the second the thermal steering and the third the "β-effect". Disregarding this term, which he had "so far"

[63] The interpretation of the first terms led on to the works on cross-frontal circulations by Sawyer (1956) and later Eliassen (1962). The seond term led, via a wartime memorandum (Sutcliffe, 1941), to the "confluence theory" by Namias and Clapp (1949) where the Sutcliffe reference is mentioned inside the text at p. 331. I also have a reference to a paper by Chester Newton (1948) which I cannot find now.

[64] Together with a Belgian mathematician Odon Godart, Sutcliffe introduced pressure as a vertical coordinate (Sutcliffe and Godart, 1943; Godart, personal communications 1993-96).

[65] According to Vincent Oliver (personal communication 1993) Sutcliffe made a visit to Rossby's institution in Chicago in 1944.

not found to be "particularly noticeable", Sutcliffe established practical rules to distinguish areas of cyclonic and anticyclonic development from the upper-air charts.

Eric Eady welcomed Sutcliffe's equation enthusiastically and professor David Brunt saw it as a sign that British meteorology had quite definitely got out of the doldrums. The Director of the UKMO, Nelson K. Johnson called the work "a most promising development for forecasting", all the more welcome as indicating that "in this country, though not overlooking the great importance of work elsewhere, we [are] also working on our own ideas". (Sutcliffe, 1948 a; Met Mag 1949, pp. 125-31; Weather, 1949, p. 127).

Sutcliffe's later claim that his method was ahead of anything at that time may sound pompous, but was nevertheless true. But his equation was still was too laborious for operational use. He had already dropped the "β-term", soon he abandoned the "thermal steering term" and was left only with the "development term"

$$f(divV - divV_0) = -V_T \frac{\delta\zeta_T}{\delta s}$$

the advection of the thermal vorticity by the thermal wind itself (Sutcliffe and Forsdyke, 1950). It was this formula that became the main guideline for operational forecasting at the UKMO, and a number of other weather services around the world.

4.5 Rossby's barotropic concept

> "I have often wondered what a Rossby wave was", Lord Harold Jeffreys to Michael McIntyre 6 February 1987, Roy. Met. Soc. interview.

As with the Coriolis effect, about 90% of the intuitive, qualitative textbook explanations of the Rossby wave are either wrong or misleading. As with the Coriolis effect they are in particular wrong in relation to the mathematics they are supposed to clarify. Part of the blame of the confusion falls on Rossby himself and his classical paper from 1939. To explain his wave equation in a qualitative, intuitive way he borrowed an isobaric channel idea that Jack Bjerknes had made use of at a meeting in Germany 1937. To explain the kinematics of cyclones Bjerknes had made use of the gradient wind approximation. But whereas Bjerknes had just taken the curvature into account, Rossby also considered the latitudinal variation of the Coriolis parameter[66].

The paper had just been published (Rossby, 1939) when he realized that he had made an error. The curvature, which is relevant for the gradient wind approximation, is the curvature of the *trajectories*, not the streamlines (isobars) as he (and Bjerknes) had implicitly assumed. Consequently, his explanation was only valid for stationary pressure patterns.

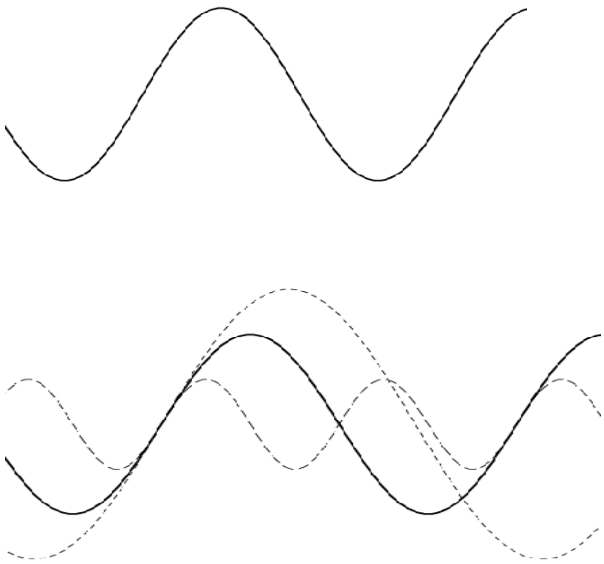

Fig. 10: A CAV-trajectory (above) and (below) the corresponding *progressive*, short wave length streamlines (long dashed lines) and the *retrogressive,* long wavelength streamlines (short dashed lines) it satisfies.

At a conference in Toronto on the eve of the Second World War he worked out a second version of his paper (Rossby, 1940). Here he made use of fact that the conservation of absolute vorticity for individual air parcels make them follow a Constant Absolute Vorticity trajectory. Such a CAV-trajectory would, for eastward motion in the sub- or extratropics, follow a quasi-sinusoidal path. From the kinematic relation between trajectories and streamlines Rossby could now show that a

[66] Using gradient wind balance Rossby (1939) argued that for short waves the curvature effect will dominate and there will be convergence of winds west of the troughs, divergence east of the troughs: consequently the wave would move eastward. On the other hand, for long waves the latitude variation in the Coriolis parameter (the β-effect) would dominate and there would be convergence of winds east of the troughs and divergence west of the troughs: the wave would move westward, *against the flow*.

specific CAV-trajectory was satisfied by either long waves moving westward, or small waves moving eastward. For a certain zonal flow there would be a stationary wavelength L_0. In a later paper Rossby (1942) elaborated further on this kinematic approach.

Judging from scientific papers, textbooks and popular articles from around 1950, which set out to explain Rossby's intentions, one gets the impression that Rossby had not been fully understood. Most readers only seem to have conceptually understood the sinusoidal trajectory of the CAV-trajectory. But it does not by itself define the streamlines of the wave[67]. Secondly, this image does not convey any reason why the flow pattern should move *westward*. This half-understanding turned out to have serious negative consequences since it gave the false impression that a Rossby wave was either a stationary feature or a simple wave moving downstream.

Unrelated to this another misinterpretation developed according to which Rossby's planetary waves were purely barotropic and were *created* by the beta-effect. Already during the war Rossby had to stress that his theory was "purely kinematic" and gave no information concerning "the ultimate cause" of the long waves (Rossby, 1942, p. 1 and 13). The long planetary waves were created by all kind of physical processes and, although not barotropic, may have the motion kinematically described as such for some limited time.

4.6 The 1950 Royal Meteorological Society Centenary debates

> "The session brought out the differences of the points of view of several eminent meteorologists and was stormy". John Sawyer reporting 1950 from the Centenary celebrations in "Weather"[68].

All three misunderstandings of the Rossby wave came to light during the Royal Meteorological Society's Centenary celebration 28 March - 3 April 1950. The afternoon on Wednesday 29 March was devoted to "The structure of weather systems" with Sutcliffe as chairman. The Head of the Dutch Weather Service, Woutan Bleeker, challenged Rossby's theories, in particular his neglect of considering differential heating. Rossby answered that although the ultimate source of the energy of storms was heat, we might get some way to understanding the dynamics of depressions even if we neglected the heat supply. In short-term changes, mechanical processes are more important than thermal, although the ultimate cause was thermal.

The whole next day, Thursday 30 March, was dedicated to "The general circulation"[69]. Rossby had to repeat that, although the ultimate driving force of the atmospheric circulation derived from the temperature difference between pole and equator, nevertheless the pattern of the circulation was determined primarily by mechanical forces in conjunction with the rotation of the earth.

> "Although thermal insolation is the fuel upon which the atmosphere engine feeds, for short-time forecasts we may look to the dynamical rather than the thermodynamically aspects of the problems."

The attempt to solve the dynamical problem consisted, according to Rossby, in the construction of simple models to account for the main observed phenomena, and the gradual adjustment of these models towards complexity to accord for further new information.

At this stage Rossby got somewhat unexpected support from one of the leading UKMO dynamists, John S. Sawyer (Taba, 1997a). From an example of a cyclogenesis over the British Isles on 22-23 May 1948 he concluded that theoretically and practically there was nothing incompatible between Rossby-Charney's and Sutcliffe's concepts. In the early stages of cyclogenesis the surface flow is often weak and the 500-1000 hPa thickness lines and geopotential contours at 500 hPa have similar configurations. The fact that Sutcliffe kept the thermal wind direction constant with height, and Charney-Rossby kept the wind direction constant with heights was therefore of no crucial importance (Sawyer, 1950). From a *practical* point there was therefore little difference between conducting thermal vorticity advection in Sutcliffe's

[67] Synoptic waves and ocean waves are always defined from their streamlines, and not the trajectories of individual particles.

[68] John S. Sawyer, personal communication. Sawyer had even made a note in his diary about the heated discussion.

[69] Rossby had one year earlier, 24 January 1949, lectured on the general circulation for the Royal Geographical Society (Met. Mag. p. 80-82, Weather, p.71-73).

system and or relative vorticity advection in Rossby's system. *The simplifications of Sutcliffe's original three-term equation made it converge to Rossby's barotropic concept.*

Perhaps it was Sawyer's unassuming manner that made the contents of what he said not receive the attention it deserved. So, rather strangely, two forecasting cultures developed where forecasters around the world looked at essentially similar looking patterns, but interpreted them differently depending on whether they were brought up under Rossby's or Sutcliffe's spell. Both schools applied simple vorticity advection, Rossby's students on black lines (isohypses), Sutcliffe's on red lines (the 500-1000 hPa thickness)[70].

Whereas Rossby's forecasters knew they were working with a simple barotropic model, which only represented the average tropospheric flow, Sutcliffe's forecaster felt they were dealing with the troposophere in its full three-dimensional baroclinic complexity. *In fact the two groups were pursuing more similar paths than either appreciated at the time.*

The Rossby and Sutcliffe schools might in the end have found common ground. But a wider gulf opened up between them with respect to another feature - *group velocity*.

4.7 Group velocity

> "Cases could be cites where physicists have been led astry through inattention to mathematical rigour; but these are rare compared with the mathematicians' adventures through lack of physical insight." Sir Arthur Eddington, quoted by E.A.Bernard (Taba, 1989)

At the time of the Royal Meteorological Society Centenary 1950 a series of crucial misinterpretations of group velocity in large-scale atmospheric motion had developed which would have serious repercussions in some countries where NWP was attempted. There were, at least, three ways of misunderstanding:

-The first was the assumption that group velocity, like the rest of Rossby's long-wave theories, was only applicable to pure barotropic waves. This ignored the fact that observed cases of "downstream developments" involved *baroclinic* systems.

-Secondly, from the fact that $c_g > \overline{U} > c$, statements were frequently made to the effect that *the group velocities were in excess of the advective velocities in the free atmosphere.* This is just not true.

The typical c_g of 30-40º per day (which at mid-latitudes 45º amounts to 30-40 m/s) are clearly greater than the *average* tropospheric flow $\overline{U}$, represented by the 500-mb wind which is typically 15-20 m/s or 15-20º per day. But c_g has more or less of the same value as the zonal average wind of the *upper-tropospheric flow*, where the main energy transport takes place.

-The third misinterpretation derived from the formalism of group velocity. "Group velocity" has meaning only when one can assume that the entire motion can be described as consisting of waves with a continually (or nearly so) varying frequency and wavelength, and that the frequency is a function of the wavelength. This is physically true for electromagnetic waves but not for atmospheric planetary or synoptic waves. Rossby (1945) had warned against this fallacy:

> "The group velocity itself is normally derived from an analysis of the propagation of patterns resulting from interference between two simple harmonic superimposed wave trains of very nearly the same wave length. There is a need for a simpler derivation of the group velocity and of its significance, without recourse to such artificially introduced interference patterns"

In spite of Rossby's warning we meteorologists have for the last 50-60 years struggled to conceptually understand "energy dispersion" and "downstream development" using this "recourse to artificially introduced interference patterns". The large-scale atmospheric waves are, after all, no optical phenomena.

[70] Compare the use of potential vorticity (PV) and angular momentum conservation (AMC) in today's meteorology. Although the PV fields are very similar to and hardly more conserved than absolute vorticity, the use of a PV chart gives more elegance to the discussions than AV charts. Although angular momentum is a three-dimensional vector, in meteorology is has always been treated as a scalar. What in dynamic meteorology is called "angular momentum conservation" is nothing more than the special case we know as Kepler's Second Law and was denoted as *Law of areas* in dynamical meteorology before 1920 (*Loi des Aires* in French, F*lächensatz* in German) .

4.8 The Scorer debate

> "I knew that the barotropic model was "dear to the heart of Rossby at that time", Scorer to the author in 1994

At an early stage British meteorologists considered the problem of computational area in NWP. J.L.Galloway (1948) had made a translation of the relevant papers by Ertel (1941, 1944, 1948). Galloway had also raised the problem at a RMS meeting on 18 May 1949. Shortly after the Charney and Eliassen (1949) paper appeared. They introduced an "influence region" in agreement with "signal" or "group velocity" discussions from a previous paper by Charney (1949) on the feasibility of numerical weather prediction.

Dr Richard S. Scorer at Imperial College was one of the leading British experts on waves, albeit acoustic waves, different type of gravity waves and lee waves. He was a good mathematician, but still could not make sense of Charney's paper. In July 1950 he sat down and wrote a letter to "Journal of Meteorology" to ask what kind of disturbance the "signal velocity" was propagating or how it arose. Any energy that may be derived from the mean motion was, according to Scorer, retained locally and did not propagated as stable wave motion:

> "No meteorological information traverses the ground faster than the wind at some level. No stable waves are ever observed that do not either travel with about the speed of sound or a stationary relative to their cause."

Scorer's letter was written in a somewhat provocative style. He did not hesitate to criticise the barotropic model and claim that purely kinematical extrapolations would be as good. If his intention had been to irritate Charney and Rossby, he succeeded. Charney called Scorer "a fool" in a letter to Rossby, who saw Scorer "strongly influenced by Sutcliffe's philosophy" and "apparently being used [probably by] Brunt as a hatchet man" (letter to Charney Sep 28 1950).

In his answer, dated 6 October 1950, Charney wrote that he was not concerned with gravity waves. The essential argument was that the large-scale motions, whether stable or not, are approximated by the barotropic motion at a certain mean level, the equivalent barotropic level. Their phase velocities and dispersive properties, if not their amplification rates, may therefore be studied barotropically. The dispersed character of the large-scale motions, the deepening of a trough and the subsequent intensification of a downstream ridge, during a period too short to be accounted for by advection of energy, had been established theoretically and synoptically[71]. For the large-scale motions there are no steering currents: the currents are part of the motions themselves:

> "If the barotropic model proves inadequate as a forecasting tool, the remedy is not, to return to the antediluvian techniques of isobaric extrapolation, but to extend theory and computational method until the relevant non-barotropic dynamical factors can be taken into account."

For an impartial reader it is obvious that Scorer was trying to find physical processes, which transported the energy as rapidly as Charney's group velocity showed. But Charney could not answer him the way he wanted, with physical arguments[72]. He said essentially: -We have good reasons to trust the barotropic model, and that is why we also trust the group velocity.

It is easy to see where Charney and Scorer could have arrived at a common view. When Scorer was looking for advective causes Charney repeatedly denied that "influences are propagated advectively". As we have seen (Ch. 2.5) there was by 1950 a widespread opinion that the energy indeed was transported by the upper-tropospheric flow. This was in no contradiction to Charney's equivalent barotropic model of the atmosphere.

The advective zonal wind, represented by the 500 hPa wind, is the *average* of weaker winds below and *stronger winds above*. It was the

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[71] When Charney had formulated his answer, in agreement with Rossby, he could take strength from the recent successful ENIAC computations, something Scorer, and rest of the world, was still unaware of.

[72] In summer 1951 Scorer submitted a paper (Scorer, 1952), where he elaborated his view that all large scale meteorological effects are advected with the wind, and none is propagated from one part of the earth to another through the air as a medium:

"Charney, however, suggests that a developing unstable disturbance might change into a propagated stable one. Such an idea is not new though it has never been shown by experiment or theory how such a change could occur. Charney instances certain synoptic studies which are claimed to contain evidence of a propagated influence, but the reader is free in all these cases to disagree with the given interpretation of the observations."

The mechanism behind “downstream development”:

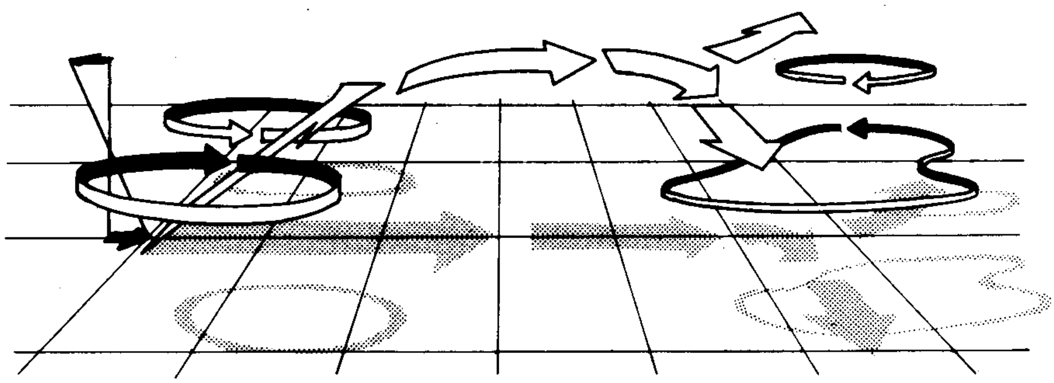

Fig. 11a: A schematic 3-D image of upper-tropospheric energy transport from Hoskins, James and White (1983). The picture illustrates how kinetic energy (in this case represented in terms of E-vectors and wave activity) released in one upstream baroclinic system feeds through the upper-tropospheric flow into the next, downstream baroclinic system which is under development.

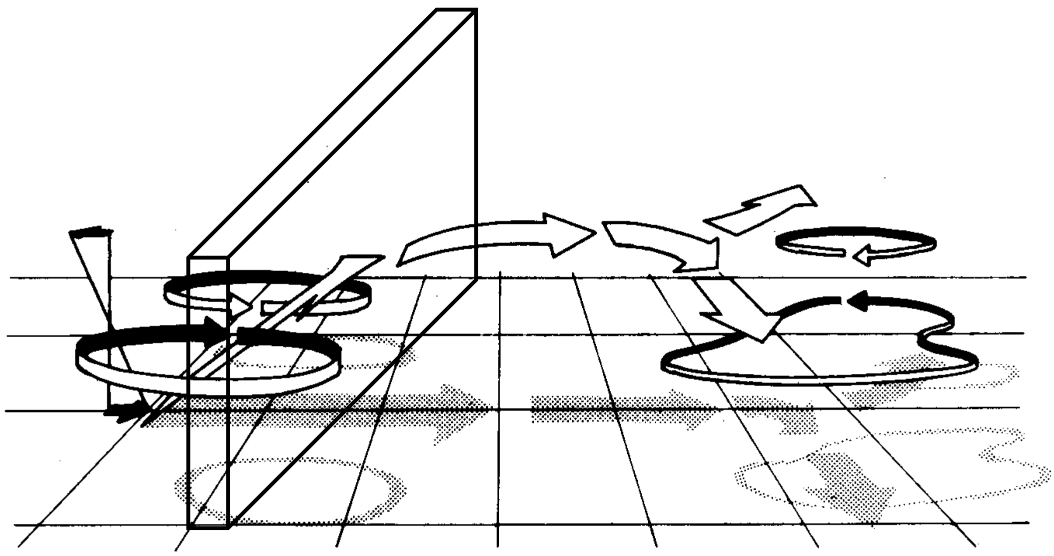

Fig. 11 b: By inserting any type of artificial, more or less transparent “curtain”, would block or diffuse the energy flux from the upstream system to the downstream. This becomes most serious when the “curtain” in reality acts like impenetrable “wall”, which is the case for constant boundary conditions. But even for nested models, any loss of energy at the boundaries, due to smoothing or diffusion, will be detrimental for the simulations of dynamic developments inside the computational area.

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latter which *physically* propagated the energy, whereas the mathematical group velocity formalism appointed the 500 hPa winds to this role. As in the case with Rossby and Sutcliffe's disagreements, the differences in Charney'äs and Scorer's mathematical formalisms took centre stage and disguised the similarity in the underlying physical processes.

Anyhow, whether Scorer was right or wrong in his criticism, most of British meteorologists took the same view as him.

4.9 Fred Bushby

During the discussions in summer 1948 it had been suggested that the UKMO should try to recruit one or more mathematicians who were specially qualified in computational methods. After gaining the necessary knowledge of synoptic and dynamical meteorology such recruits would be available to undertake research into the formulation of meteorological problems in the manner suited to calculation. The first man to be selected for this task was a very able young mathematician, Fred Bushby.

Fred H Bushby (1924-2004) was just 27 and had at only 20 graduated in mathematics from Imperial College London with two distinguished prizes in mathematics as the best student of his year. After serving in RAF Met Branch he became scientific officer in the Meteorological Office in 1948 (Mason, 1984). In 1950 Bushby was posted to the Forecasting Research Branch in Dunstable and attended a course at Imperial College in numerical methods, including relaxation techniques, given by Bernard Southwell (1940, 1946).

During his first year at the UKMO he made himself acquainted with Southwell's relaxation methods applied to meteorological problems (Bushby, 1951a). He was also busy familiarising himself with the literature, in particular Charney and Eliassen's papers on one dimensional method of barotropic forecasting. He found that their method was physically equivalent to separate the actual flow pattern into a series of latitudial wave components each of which was moved on with its own velocity.

A test of the method gave results "better than could be expected if the method was fundamentally unsound." His report (Bushby, 1950 b) was received "with interest" by the MRC. They found the method, though very crude, still was an attempt at numerical forecasting, and discussed possible improvements and alternative methods of attack. Their positive judgement justified further work.

However, it appears that Bushby's results were not quite appreciated at the UKMO. He had to repeat the investigation, now with the aim to establish if Charney-Eliassen's method was superior to the *manual* forecasts at the Central Forecast Office (CFO). As it turned out, it was not, and this would seal its fate. This was to be the topic at a forthcoming Royal Meteorological Society meeting.

4.10 The 17 January 1951 Royal Meteorological Society meeting

> -I have suggested to Dr Charney that higher order extrapolation must necessarily lead to better results than the barotropic model, but he described such a method as 'antediluvian'. Whether or not it was the method employed by our Neanderthal ancestors – and here the word 'ancestors' is not to be taken as indicating any opinion whether *Homo sapiens* superseded or evolved out of Neanderthal man – this method is still better than anything that can come out from the barotropic model. (Dr Richard S. Scorer in the discussion 17 jan 1951)

On Jan 17 1951 an important meeting on "Dynamical methods in synoptic meteorology" took place at the Royal Meteorological Society (Weather p.61-62, QJRMS p.457-73 ff, Met. Mag 112-14). Sutcliffe opened by saying that there were only two approaches to the forecasting problems: Rossby's and his own. He hoped his method would ultimately lead to improved methods of forecasting and that the approach to numerical forecasting "might be along these lines".

Next speaker was Eric Sumner, a young scientist who also had a reputation of being a "shrewd synoptician". He had investigated Rossby's wave formula and found correlations of 70-80 % between calculated and observed 24 hour trough displacements. That was good, but not better than the forecasts made by the CFO (Sumner, 1951, see also Sumner 1950).

Fred Bushby described his second test of Charney-Eliassen's one-dimensional method. The results were good, as they have been in the first test, but now Bushby could add that they were not as good as those obtained by the CFO. Bushby also showed how vertical velocities could be calculated from Sutcliffe's theory (Bushby, 1952).

First out in the general discussion was Richard Scorer who continued to criticise the barotropic model along all the lines it could be misunderstood. It was "defective" since it had no energy source and it did not permit any developments. Since the Rossby formula could only be applied when the phase velocity was zero, the waves were necessarily stationary. At best the formula could move disturbances at a constant velocity, no better than linear extrapolation.

The head of the long-range forecast section, Forsdyke, testified that the Rossby wave formula did not apply to waves in the usual sense of energy-propagating disturbances. Sumner objected that Rossby waves could be both moving and stationary, and that Cressman (1948, 1949) had arrived at slightly better results. Sutcliffe ended the debate by expressing "considerable doubts" about the barotropic theory and "broadly agreeing" with Scorer's view about the extrapolation.

> "When a satisfactory solution to the 3-D problem emerges it will derive little or nothing from the barotropic model – which is literally sterile[73]."

The barotropic model had been brought up, scrutinised and killed off. But the corpse blinked and it would soon rise from the dead when the November 1950 Tellus issue arrived in March with the results from the ENIAC runs.

4.11 1951-53 Preparations for NWP

> "There was a lively discussion on the merits of applying the first calculations to the behaviour of a textbook model cyclone rather than to the irregular disturbances of a real synoptic chart. Nevertheless all were agreed that numerical methods had a more immediate application to dynamical research than to forecasting" (report from the Cambridge conference in September 1951 by Sawyer (1952))

Active work on numerical weather prediction at the UKMO seems to have started in the aftermath of the 17 February meeting. A research plan for "Application of computing Machinery to Forecasting Problems" was decided in March or April. Work was said to be in progress also at the Imperial College.

On their 15th meeting 23 May the Synoptic and Dynamical Sub-Committee of the MRC discussed Rossby's, Charney's and Sutcliffe's approximations and Bushby's second report on the Charney-Eliassen method. (Bushby, 1951c, Met. Mag. p.191). A long unrecorded discussion took place of the validity and value of the various approximations made in the Rossby, Charney and Sutcliffe approaches.

In August Sawyer and Bushby (1951) wrote a note to Tellus to report their own non-convincing tests of Charney-Eliassen's one-dimensional method. They would now, they wrote, in their NWP work, apply Sutcliffe's model where the thermal wind had the same direction in all layers, but not necessarily parallel to the wind direction as in the equivalent barotropic model. They feared though that the numerical integration would be very heavy and "might tax the capacity of even modern electronic methods".

At the same time the IUGG meeting in Brussels gathered many prominent meteorologists, among them Charney and Rossby (Met. Mag. p.326-330). Charney had visited Sweden during the summer, and passed through the UK on his way home. On 6 September he took part in a colloquium in Cambridge on "Numerical methods in meteorology". (Met.Mag 1951, p. 345, Sawyer, 1952, p. 76). Charney concentrated on the dynamics of synoptic systems and said that his model was only the first step in the development of more realistic schemes. Bushby described useful numerical methods and Eady presented preliminary calculations at Imperial College.

In October Bushby attended a course in numerical methods in Cambridge in connection with their EDSAC machine becoming operational. On his return to the UKMO he chaired a colloquium on the possibilities of high-speed computing in meteorology. In the discussion Scorer suggested other applications than forecasting (growth of raindrops, dynamics of standing waves etc). There were "lively discussions" of applying the calculations to idealised or real synoptic systems, to dynamical research or to forecasting (Sawyer, 1952).

[73] This is what Sutcliffe said according to unanimous reports in Weather and Meteorological Magazine. According to QJRMS he was much more conciliatory, probably because he had written these comments at a later stage when the results from the first ENIAC runs had reached Britain in March 1951. See later chapter (4.21) for an analysis of Sutcliffe's attitudes to NWP.

In September 1951 Bushby had completed two investigations, one on computing Charney's two-dimensional height tendencies, the other on computing the mean vertical velocity in the 1000-500 hPa layer of the atmosphere and its effect on the thickness of the layer (Bushby, 1951, d,e). They were presented 13 December to the 18th Synoptic and Dynamic Sub-Committee, (Met. Mag. p.85). Based on this work Bushby published a critical article about "Forecasting methods based on barotropic wave theory" in Meteorological Magazine (Bushby, 1952). Two-dimensional barotropic forecast tendencies showed, according to Bushby, "rather poor agreement" with observed changes. The "equivalent barotropic model" was therefore an inadequate basis for NWP because, as "experienced forecasters" knew, the conditions of constant wind direction with height and linear increase of wind in the vertical were rarely accurately satisfied.

Bushby did not deny that the advection of absolute vorticity at 500 mb was relevant to changes at that level and noted that the results were "much more accurate" if the β-effect was included. In another article Sawyer (1952) credited Charney to have shown that it was possible to use high-speed electronic computing machines to obtain solutions of partial differential equations "which were relevant to the problem of forecasting."

4.12 1952 The Sawyer-Bushby model

The impact of the debates in the British meteorological community is noticeable in a review article "Dynamics of flow patterns in extra-tropical regions" by Eady and Sawyer (1951). The article is very well argued and still makes good reading. Nevertheless, with respect to the question about barotropic versus baroclinic models the paper is rather ambivalent.

Rossby was on one hand said to have made "arbitrary simplifications" and "sweeping approximations" for his long wave theory. The Rossby wave concept was explained in the common erroneous way as a trajectory of an air parcel conserving its absolute vorticity. "Over-enthusiasts" of the barotropic model were then (three times) reminded "that we cannot hope to explain all the principal features of the large-scale motion by such means."

On the other hand, Eady and Sawyer found that the "qualitative success" of the ENIAC runs had encouraged further investigations of the barotropic model. Although the underlaying concepts were theoretically "naive" or "unsatisfactory" and the model "inaccurate" the results were seen as "interesting".

> "The aim is not a completely satisfactory theory but rather to find a way of making a start towards a realistic theory."

Sawyer and Eady presented the concept of group velocity with the conventional geometric arguments of superimposed sine waves.

> "Some investigators have inferred that there is evidence of such behaviour in the atmosphere, but as in the case of the individual waves, there are difficulties in drawing very definite conclusions."

Shortly after the article had been published two British meteorologists, Smith and Forsdyke, would undertake a major synoptic investigation to find out if there existed anything such as "group velocity" and if it any synoptic significance.

At this time, late autumn 1951 and early 1952, Sawyer and Bushby developed a simple baroclinic model, inspired by Sutcliffe's theory (Sawyer and Bushby, 1952). The thermal wind direction was constant in any vertical column and its speed is proportional to the vertical pressure difference through the layer. Three simple differential equations computed the height of change of a contour surface, thickness 1000 hPa to that surface and average vertical velocity. Similar models would soon emerge from the US and Europe[74].

The theoretical work was finished in February 1952 and discussed by the MRC on 26 March. But it lasted until 29 August before the paper was submitted to Journal of Meteorology[75].

In the meantime Bushby made some calculations of the vertical velocity and thickness tendency using Sutcliffe's theory. The results

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74 It is an open question to what degree these other 2-level models were inspired by Sutcliffe's equation. In 1939, he had already shown that a two-level model was quite powerful. On the other hand, there are few alternatives as to how these 2-layer models can actually be constructed.

75 Why this delay? There are few, if any changes between the original and final version (Sawyer and Bushby, 1953).

showed good agreement with reality for six synoptic situations (Bushby, 1952). One year later he would make a further evaluation of Sutcliffe's development formula on twelve cases (Bushby, 1953). Out of 90 development calculations, 41 gave useful guidance, 36 not misleading and 13 misleading.

In October Sutcliffe attended the NWP conference in Stockholm and presented "Some preliminary experiments in numerical computation at the Meteorological Office". On his return he reported in a Monday Discussion that although the research in numerical methods was going well both at home and abroad, there were no reasons to expect "revolutionary improvements" in forecasting by these methods.

On 16 February 1953, on a Monday Evening Discussion Rossby's barotropic concept, where "the critical wave length" played a central role, came under scrutiny. Tests at the UKMO had shown a 90% correlation between computed and observed 24 h displacement of downwind troughs, but again no better than the CFO forecasts. Problems were identified in cases of retrogressive motions, which made Bushby conclude that the Rossby method only worked when the flow was sinusoidal.

The discussion took a new turn when C.V. Smith brought up the mechanism of *downstream effects*[76]. As he understood it, the formation of a major trough in the upper westerlies should give rise to dependent wave trains downstream. Examples of such downstream trough formations, Smith argued, indicated that they were initiated by baroclinic developments. This provoked J. K. Bannon to ask why a barotropic model could anticipate *baroclinic* processes? This highly relevant question seems to have been left unanswered.

4.13 1952-53 Bushby and Hinds' tendency calculations

At the end of 1951, the LEO 1, a copy of the Cambridge EDSAC machine, built by Messrs J. Lyons, the caterers, was installed at their Cadby Hall headquarters. The UKMO managed to have access to the machine. Since LEO 1 was aimed at rationalising office work, it was politically controversial. In order to prevent a labour unrest by the regular office staff, the computer was kept in a secret location different from normal Lyons office space[77]. Most of the UKMO computing was done during evening sessions with assistance from the staff both in operating the computer and the provision of supper in the managers' mess.

The machine's storage medium was mercury delay lines, which were housed in large coffin-like wooden boxes covering most of the floor of the computer room. These were very reliable but had very slow access times and were the only form of storage, as there was no backing store. In the early days the only input and output was by paper tape, but later a card reader/punch and a line printer were installed. Paper tapes were punched on a teleprinter-type hand-perforator with the keys relabelled to the LEO 1 coding and necessary amendments could be made only with the kind assistance of those with access to a reperforator.

All values were stored in the machine in fixed-point binary and careful scaling was necessary if accuracy was not to be lost whilst ensuring that 'overflow' did not lead to wrong answers. There was no counting-register and movement through the grid of values was done by amending all the relevant instructions after each grid point, and then testing them against the appropriate instruction for the last point in the grid line, or the final point in the grid. The storage was so small that it was essential to overwrite data and intermediate results during the computation. Programming was in mnemonic assembler-type code." (Hinds, 1981)

Starting in the winter 1952-53 Fred Bushby and Mavis Hinds worked on a baroclinic two-layer model. They prepared the ground by first conducting tendency calculations: one for the change of the 500 hPa contour height, another for the change of the 1000-500 hPa thickness. The 1000-mb height tendencies were obtained by subtracting the thickness tendencies from the 500 hPa. To their surprise, the 1000 hPa height tendencies showed good agreement with

[76] Cliff V. Smith joined the UKMO in 1948 after degrees in physics and mathematics. Having a "flair for forecasting synoptic developments" he started in 1951 at the long-range forecast team under Forsdyke (Ryder, 1985). He would publish papers on the use of Rossby theory (Smith, 1959). Together with Sumner he seems to have belonged to a small minority at the UKMO who were interested in barotropic theory.

[77] Norman Phillips (personal communication), who visited the machine in summer 1953, on his way to Sweden, found it a "thrilling experience".

reality. Test with correct boundary conditions significantly increased the agreement between computed and actual tendencies.

> "The effect of boundary conditions seems important if a small area is considered. Before time integration is undertaken it would seem necessary either to increase the area under consideration, so that the effect of boundary conditions would not affect the central area, or to make a preliminary estimate of expected changes around the boundary and feed this into the machine."

To dampen the adverse effect of the wrong boundary conditions the westernmost six columns of points were excluded from the verification (Bushby and Hinds, 1953, a-c,e,f)[78].

By the time their tendency calculation paper was accepted for publication in QJRMS in December 1953, Bushby and Hinds had passed a new mile-stone: two 24 hour full-scale integrations of the baroclinic Sawyer-Bushby model (Bushby and Hinds, 1953, d).

But before we come to a presentation of this truly historical calculation, we must review Smith and Forsdyke's paper on "downstream development".

4.14 1952-54 The Smith-Forsdyke paper

> "Indeed, 'dispersion' seems to be a useful concept only in so far as barotropic theory is invoked." Smith and Forsdyke (1952)[79].

We have previously (Ch 2.5) reviewed papers on downstream effects published 1945-50. By 1953 the stock of theoretical and synoptical investigations of downstream development had increased (Reichelderfer, 1952; Riehl et al, 1951, 1952). Theoretically it was recognised that, in the general atmospheric wave motions, kinetic energy released in baroclinic disturbances, was propagated much faster than the phase speed. The transport was mainly carried out by the upper tropospheric flow, most importantly by the jet-streams. The transfer of energy was governed by the large-scale adjustment between wind and pressure gradient in "direct" and "indirect" circulations (McIntyre, 1951)[80].

Synoptically it was found, using Hovmöller's trough-ridge diagrams, that cold outbreaks over E USA could be related to a chain of developments upstream starting three days earlier east of Japan (Parry and Roe, 1952). Another synoptic investigation by Carlin (1952,1953) traced a case of dispersion of energy downstream in the mid-tropospheric long-wave pattern over more than half the hemisphere. Reed and Sanders (1953), Austin (1954) and Austin et al (1953) made similar investigations.

All this accumulated evidence did not seem to have left any impact in the references in the memorandum Smith and Forsdyke (1952) now finalised. The authors only made references to three theoretical papers, one by Eady (1950) and two by Rossby (1945, 1949).

Smith and Forsdyke (1952) acknowledged that interaction between synoptic systems in different longitudes had been recognised for a long time. Laterly *downstream effects* had been derived directly from Rossby's work on long-wave flow patterns as a part of barotropic or equivalent-barotropic theory.

> "Barotropy implies to a close approximation conservation of absolute vorticity. 'Constant vorticity trajectories' were introduced into forecasting (in America) as indicating where new troughs or ridges might form. It was then noticed that new oscillations sometimes emerged so distant from the 'primary trough' as not to be explained by any advection of vorticity: a wave-like process of propagation was inferred."(Smith and Forsdyke, 1952)

Theory had shown that the long-wave phase velocity was less than the mean zonal current and if the influence was to travel faster than by advection it must according to Smith and Forsdyke, be by some wave velocity greater than the current velocity.

> "Rossby (1945) suggested that the process was one of energy dispersion and showed, with various assumptions, that the group

[78] This band, 260 x 6 = 1560 km wide would have been appropriate for a 24 hour forecast if the speed of progression never exceeded 18 m/s.

[79] In the 90's when I lectured at the ECMWF some British students, taught about group velocity in the common "interference of sine waves" way, thought 'energy dispersion' only applied for spectral models, like the ECMWF one, and not to grid point models as the UKMO one. I think Rossby laughed in his heaven.

[80] It had already in 1947-48 been observed that many cyclones increased their potential energy while they developed (Carson, 1948)

velocity should in fact be greater than the current velocity. Apart from affording a plausible explanation of developments not otherwise readily interpreted 'barotropically', it is not clear what the value is claimed for the idea of 'dispersion' in synoptic meteorology and the present statistics do not afford much evidence.

> In the first place it may be that the question is begged by speaking of a 'primary' trough and implying that subsequent formations downstream arise consequently. There is rarely evidence to prove that a downstream oscillation would not have arisen just as well without the presence of the primary."

Then Smith and Forsdyke referred to Eady (1950) and others who implied that long-wave oscillation may arise as a result of 'instability'.

> "There is no reason to look to 'dispersion' for an explanation: indeed, 'dispersion seems to be a useful concept only in so far as barotropic theory is invoked."

In the movement of a wave packet with a group velocity the Smith and Forsdyke would expect to see simultaneously a number of oscillations with the maximum amplitude being transferred forward from one trough-ridge to the next downstream. Their data did not suggest such a process, rather the reverse.

> "The impression is not the one of energy transfer with a 'group velocity' but of a local energy source such as only baroclinic theory can provide for. […..] This does not mean, however, that downstream effects do not occur. Certainly on occasions the development of one oscillation is clearly linked with an earlier disturbance elsewhere, but the mechanism of the linkage probably involves the processes of baroclinic development."

The Smith and Forsdyke (1952) report had been finalised 22 September 1952, but it lasted until 22 January 1953 until it was discussed by the MRC. It was submitted to Quarterly Journal on 24 April where it would appear one year later very much reduced in size (Smith and Forsdyke, 1953). It was also presented at a meeting of the Royal Meteorological Society, almost one year later, on 17 February 1954. This meeting is highly interesting because the discussion of Smith and Forsdyke's paper was eclipsed by Bushby and Hinds' presentation of the first computer based baroclinic forecast in Europe. These were the foci of two important discussions, one on 15 February 1954 at the UKMO, the other on 16 June at the Royal Meteorological Society. The meeting gained extra significance because of the presence of two distinguished scientists from Carl Gustaf Rossby from Sweden and Joe Smagorinsky from the USA[81].

4.15 The 15 February 1954 UKMO Monday Discussion

> "Soon I have to go to England…it is not easy for me…I feel uneasy among British meteorologists". C. G. Rossby to Andrzej Berson (1991).

The first European baroclinic integrations were presented at a UKMO Monday meeting 15 Feb 1954 UKMO. It drew together many great names in meteorology: Rossby and Joe Smagorinsky from abroad, Stagg, Sawyer, Scorer, Douglas, Ludlam and others from Britain.

Fred Bushby made the presentation. The analyse was made in a 18 x 14 grid of 260 km, the forecasts were run in a 14 x 10 grid. The time-step was 1 hour and a 24 h forecast took 4 hours to compute. The machines were the LEO by J. Lyons & Co and Ferranti machine at Manchester University. The baroclinic model was tested in two cases, 14 March 1949 and 27 January 1952.

The first case was from 14 March 1949. They started the computations with a two hour time step. Indeed, such a long time step had been used by Charney et al (1950) at the ENIAC runs. But then the grid length had been 600

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81 In the midst of the regular, and almost ritual, criticism of Rossby's barotropic ideas he was suddenly given the most prestigious award the British meteorological community can bestow a fellow scientist, the Symonds Memorial Medal. It was given to Rossby "in recognition of his outstanding contributions to meteorology, from small-scale turbulence of the lower atmosphere to the dynamics of the large-scale currents and their relation to the general circulation of the atmosphere." He was awarded for his "…vigorous lead to scientific meteorological research…inspiration and encouragement of meteorologists in all parts of the world". Rossby sent a message of thanks:

> "Our efforts to develop techniques for the use of electronic computers in meteorological calculations, and particularly our first steps to develop a rational research programme in cloud physics, are enormously strengthened by the outstanding contributions made by British scientists who from time to time have worked in our group".

For unknown reasons he did not come to receive the prize. It was forwarded through the Swedish ambassador Gunnar Hägglöf.

km, not 260 km as with the UKMO model. Obviously the British were not aware of the Courant-Friedrich-Levy (CFL) condition, although it is discussed by Charney et al (1950).

As could be expected Bushby and Hinds ran into numerical instabilities, although it appeared only 18 hours into the forecast. They nevertheless let the integrations continue for the remaining six hours. The instabilities amplified, but did not affect the region of the British Isles.

In their second case, from 27 January 1952, the instabilities occurred already after 10 hours and the computations had to be stopped after the next time step. A restart was made eight hours into the forecast with the time step changed to one hour.

In his summary Bushby pointed to a tendency of the model to exaggerate anticyclonic developments, which was due to the geostrophic approximation and lack of friction. Errors of lesser extent were due to arbitrary choice of boundary conditions and effects of topography. In the subsequent forecasts a time step of one hour would be used. Problems related to numerical instability should be subject of a special study[82].

Rossby congratulated the UKMO on being the first official weather service to experiment with numerical forecasting. But, as he wrote to Charney some days later, he was "a bit concerned about the direction of their work":

> "It is of course difficult to judge from a lecture and a fleeting look at slides, but I had the impressions:
>
> a) They must use some very peculiar boundary conditions because several of the 500 mb charts showed a most remarkable evidence of some sort of instability in the border regions (wobbly contours etc.)
>
> b) The central portions of the 500 mb charts seemed to give quite good results
>
> c) The predicted sea level charts appeared to be far inferior to the 500 mb charts, but again, I am not sure of this. "[83]

It reflects the interest and excitement Bushby and Hinds' results created they were to dominate the discussion two days later at a meeting of the Royal Meteorological Society, which was announced to be about something else.

4.16 The 17 February 1954 Royal Meteorological Society meeting

Two days later Rossby and Smagorinsky attended a Royal Meteorological Society meeting. Smagorinsky presented his work on the dynamical influence of large-scale heat sources and sinks on the quasi-stationary mean motions of the atmosphere (Smagorinsky, 1953) and Smith and Forsdyke their paper on downstream effects (Smith and Forsdyke, 1953). In the short discussion Smith made the comment that "purely barotropic evolutions" were uncommon over three to four days. To produce extended forecasts by NWP it was therefore necessary to have mathematical models, which included baroclinic developments and non-adiabatic processes. However, as the corres-pondent from "Weather", John S. Sawyer, noted, the lively discussion that followed the presentations had very little to do with the papers, but were on the possibility of numerical forecasting.

It seems to have been Rossby who triggered the debate by turning long-held opinions on their head by stating that NWP would probably be used for forecasting the movement of large-scale features over 2-3 days before it was used for detailed 24 h forecasting. Sawyer wrote:

> "It enabled Fellows to hear Dr Rossby suggest that numerical methods should concentrate on forecasting for two or three days ahead, whereas Dr Eady and others thought that greater promise lay in the attacks on the 24-h problem."

Rossby also suggested, according to his letter to Charney, that the UKMO should, if the capacity of the machine allowed, also run daily

82 When Rossby came back to Stockholm he told his staff: "The problem with the English is that they have to reinvent any major achievement made somewhere else in the world." (Bo Döös, personal communication, 1992). The study in numerical instabilities emerged some years later (Knighting, Jones and Hinds, 1958).

83 The full letter can be found in Appendix 2.

Is this what Rossby and Smagorinsky saw at the UKMO Monday Discussion?

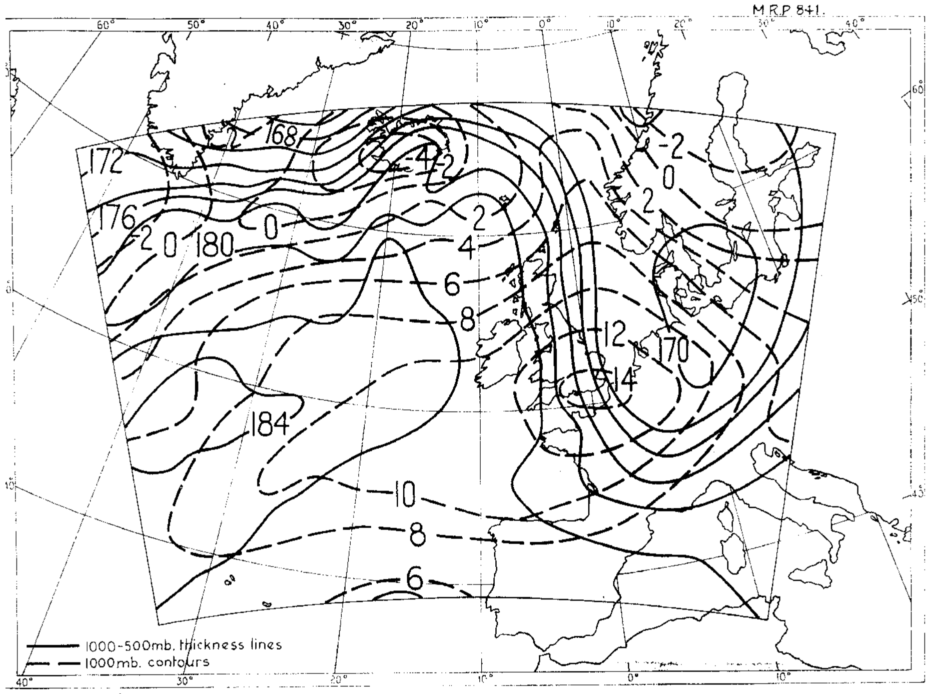

Fig. 10(d) Computed 1000-500mb. thickness and 1000mb. contour charts for 1500 G.M.T. 15.3.49

Fig.11 a: The first forecast from 14 March 1949 15 UTC +24 hours, was run with a time step of 2 hours and numerical instabilities showed up after 18 hours.

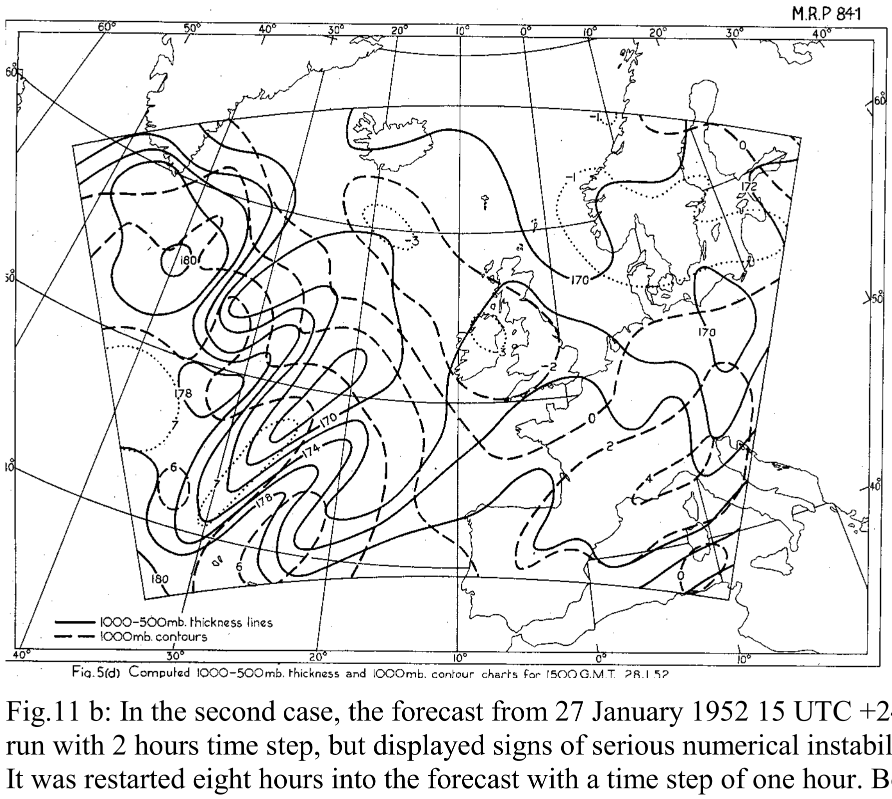

Fig.5(d) Computed 1000-500mb. thickness and 1000mb. contour charts for 1500 G.M.T. 28.1.52

Fig.11 b: In the second case, the forecast from 27 January 1952 15 UTC +24 hour 1000 hPa, was also run with 2 hours time step, but displayed signs of serious numerical instability already after 10 hours. It was restarted eight hours into the forecast with a time step of one hour. Both pictures are from an internal UKMO Meteorological Research Memorandum (Bushby and Hinds, 1953 d).

Barotropic 24 or 48 hour forecasts. The prediction of the sea level charts would be considered as a research project not yet ready for routine tests. In Stockholm, Rossby told the British audience, that in Stockholm they were certainly going to concentrate on this approach and as soon as the new drum memory was ready for use. He hoped that they could go on the 48- or even 60-hour forecasts, using a larger set of observations (with a one- or two-parameter model). Rossby argued that there were both practical and tactical reasons for this.

The detailed plotting and analysis of several upper air maps, twice daily, plus the reading off of a large number of initial values and the punching of these data on tapes would take too much time that a 24-hour forecast hardly can be completed in time. He feared that the mean sea level pressure (MSLP) forecasts, which were sensitive to the details of many assumptions in the models, were going to expose them to "gleeful criticism" from the "old-time school of forecasters". The forecasters needed to become accustomed to 500 hPa charts:

> "You must not skip stages in the mental development of the forecasters. The routine forecasters at Dunstable do not, I fear, possess much of an understanding of the 500 mb (or any other height) chart, and it would do them a lot of good to watch these charts for a long period."

He was inclined to believe that for the time being the principal value of the higher parameter models was that they permitted predictions of the averaged (i.e. the 500 mb motion) more accurately than did the barotropic.

In a letter to Dahlquist 21 March 1954 (Appendix 3) Rossby came back to the issue of boundary values, a problem he most surely also discussed with the UKMO staff. Would it not be possible to set up a computational scheme with the observed, instead of the arbitrarily assumed boundary conditions along the periphery of the forecast area? Since the aerological observations were generally given only every 12th hour they would probably have to develop an interpolation method for time intervals between the aerological times.

For the first tests the extrapolations should be carried out subjectively by "experienced synopticians". Later the machine could do it. If it turned out that the model could be run for 2-4 days without the forecasts for the inner area becoming "too stupid", they would be some way down the road towards forecasts for unknown areas, because sooner or later influence of the initial values in those regions disappear.

Rossby wrote that he would be extremely grateful if Dahlqvist and his group could give some consideration to a suitable working area, how to introduce efficiently the correct boundary values and to preliminary select synoptic situations to interpolate the boundary conditions between the observing times. Some of the arguments from the meeting, either originating from Rossby or the British (or both) can be recognized in the following work at the UKMO.

4.17 More baroclinic calculations and an quasi-adjoint integration

> "We merely changed the Addition instructions to Subtractions!" Marvis Hinds, 2004

At a meeting of the Royal Meteorological Society on 16 June two of Bushby and Hinds papers were presented and discussed. They were first the tendency calculations from summer 1953 (Bushby and Hinds, 1954) followed by their baroclinic integrations in autumn 1953 (Bushby and Hinds, 1954). During the discussion a young meteorologist, a Mr John B. Mason from Imperial College put out an intriguing question[84]:

> "Is it possible for rapid developments on the boundaries of the grid to upset the forecast seriously for the centre of the region within 12 or 24 hours? Had they tried the effect of putting the *actual* boundary conditions into the equations after the event, and, if so, with what result?

Bushby answered that the size of the forecast region was originally chosen so that there would be "little likelihood" of the centre of the region being affected by rapid developments on the boundary during the forecast period."

[84] The coming Director of the UKMO was at this time employed by the meteorological department at Imperial College. He was not yet a PhD and had the formal title "Lecturer in meteorology". When once asked by Tor Bergeron about his education in meteorology, he said that he had none. -But you are lecturing in meteorology? –No, I am not, said Mason. Bergeron laughed: -So here is a lecturer in meteorology who is neither meteorologist nor does any lecturing! (Sir John Mason, personal communication 2003).

However, it now seemed likely that there were isolated occasions when boundary affects may had caused errors in the centre of the region. It was their intention to carry out some computation using the correct boundary conditions.

During spring ten more baroclinic forecasts were made (Bushby and Hinds, 1954b)[85]. They could identify shortcomings due to lack of heating from below of cold air masses over sea and the assumption of zero change along the boundaries. They would also have preferred to have 600 hPa data instead of 500 hPa. Nevertheless they recorded a skill "only slightly less " than a conventional forecaster.

Constant boundaries were applied during the solution of the differential equations but were assumed to change by half the value of the nearest gridpoint inside. But the small areas was still the cause of major problems and made any assessment of the validity of the theoretical model impossible.

Bushby and Hinds (1955) published a condensed version of their 1954 memorandum with thirteen cases. It was never "read" or discussed at a Society meeting. Perhaps it was regarded as too operational. This was a pity because during the work Bushby and Hinds had made what must be considered as the first quasi-adjoint calculation.

Among the thirteen forecasts the one from 8 January 1951 scored particularly badly. On Mavis Hinds' suggestion they decided to start from the *verifying* chart 24 hours and feed the grid point values into the model, *which then was run backwards!* This was easily done because there were no irreversible physical processes (Hinds, 1981 and personal communication).

This "Hindcast", as it came to be called, suggested that the small depression might have been a more intense feature at 15 UTC on the 8th than originally thought. In fact, late surface ship reports, which would not have been available when the upper-air charts (from which the computer data were extracted) indicated that this was probably so. This served as a reminder to the group of the importance of a good analysis for producing a reliable forecast. They also had re-run a few cases with a simple empirical parameterization of the heating of cold air over warm water.

Bushby and Hinds had run with both constant and true boundary conditions and noted clear improvements with the latter.

From now on, until the pre-operational tests around 1960, all NWP experiments would be with true boundary conditions. Of course this just changed the problem from being one of bad influences being propagated into the area, to good influences doing the same! Indeed the use of true boundary values would force the forecast into the right synoptic development.

At a UKMO discussion 21 Feb February 1955 Bushby compared forecasts from 3 Nov 1954 03 z made by new and conventional methods. The fact that the numerical methods were better, but that observed boundary changes had been used led to "some discussion". Pothecary and Bushby (1956) later published a series of computed forecast charts of the movement of a depression, 18-21 August 19-21 1954, where the observed changes around the edge of the area were used as boundary conditions during the calculations.

4.18 Mavis Hinds and the "Ladies of early British NWP"

> "-I was very happy working with computers, but only because what came out at the end was meteorological. I would not have been interested in Income Tax computers! Mavis Hinds, personal communication 2004

At the opening of the 16 June meeting, Fred Busby's co-worker, Mavis Hinds was admitted a "Fellow" of the Royal Meteorological Society[86] as an acknowledgement for her contributions. Mavis Hinds is one of the first female meteorologists of any prominence, and the first woman to play a leading role in the development of NWP[87]. But she was not an

85 Interestingly, one of them is the Dutch storm 31 January 1953. It was very skilfully forecast and the authors expressed the view that "there is no doubt that if these charts had been correct the floods would have been even more calamitous than they actually were!"

86 As it is still today: anybody can become a "Member", but it takes personal recommendations to become a "Fellow". The annual fee, however, is the same.

87 In 1950-53 the wifes of Joe Smagorinsky, John von Neumann and Arnt Eliassen all contributed, anonymously with programming and hand calculations

isolated case. In contrast to what one could expect from the legendary stuffy British Civil Service, the UKMO could proud itself of not only allowing *four* women to make important contributions, but also acknowledge this by co-authorship!

Mavis Hinds was born in 1929 and in 1947 passed her High School Certificate in pure mathematics (eg. calculus, algebra, geometry etc), applied mathematics (which included dynamics, mechanics etc), physics (which in Britain included the physics of heat, light, sound, electricity, magnetism) and geography (which also included climatology!). This was of course an ideal combination for meteorology, which Mavis already found very interesting. However, her father, who was a school-teacher, did not want her to go to university! To understand this one must go back to the very different world of post-war Britain.

In 1950 most children left school at 14 with no qualifications. About 1/5 left at 16 with School Certificate (GCSE) and very few stayed on to take Higher School Certificate (A-levels) at 18 and possibly go to university. But then the student had to be rich or get a scholarship to help with the costs. Even so, there were very few jobs for graduates, in particular female ones, except teaching or medicine.

But Mavis was fortunate, thanks to her abilities, to be granted both a scholarship and a place at University College, London to study mathematics. This was very unusual in 1947, as all the men who had been at war were filling the college places. So her father had to let her go! She graduated in 1950 and worked in the aircraft industry for a short while, before she joined the UKMO in January 1951, as an Experimental Officer.

In the UKMO there were three "classes" of staff. Assistants, with a School Certificate, who did observations and plotted charts. Experimental Officers, with Higher School Certificate or possibly degrees, did the largest part of the routine forecasting, and were based on airfields far and wide across the globe. Then there were a much smaller number of Scientific Officers, with good degrees, who were responsible for research, but were also expected to be able to do forecasting. They filled the most senior posts. There were many females among the Assistants, some among the Experimental staff and practically none among the Scientific staff.

Although Mavis Hinds did the Initial Forecasting Course in spring 1951, she was thought to be too young to go into forecasting. Because of her mathematics degree she was sent to the Forecast Research Division to work with Fred Bushby. Initially they had to work on electrical desk calculators, but when Bushby came back from the Cambridge course in September 1951 he taught them about programming the EDSAC9.

Vera May Huckle (1931-58) was a mathematician, who joined the Meteorological Office in 1952. She had a degree, but like Hinds, entered as an Experimental Officer. In her late 20's she developed a form of leukaemia that was untreatable. Shortly before her death in November 1957 she was elected Fellow of the Royal Meteorological Society.

Joe (Claire) Whitlam, who would produce papers with Bushby, Knighting and others, came in as an Assistant, but advanced through evening class A-level studies to become an Experimental Officer and a programmer. She left the UKMO to have a family. Many years later she came back as an Assistant, her first priority being her family.

Margret Timpson became internationally known for her involvement in the 10-level, primitive equation Bushby-Timpson model of the late 1960's. In her own country, however, her name is more associated with her father's nation wide chain of shoe shops. As an Experimental Officer she worked with Fred Bushby on the Atlas computer, first at Manchester, then at the Rutherford Laboratory, Harwell. She left the UKMO when she got married.

4.19 Impressions from abroad and objective analysis

> "The testing and development of objective analysis was more difficult than the actual forecasting problems", UKMO Discussion 17 October 1960.

In early 1954 the UKMO had changed to Ferranti Mark 1 at Manchester University. Since they needed the computer for several hours at a stretch, most of their usage was at night. For some years they used the machine for two nights each alternate week. Mavis Hinds (1981) remembered

> "We stayed at a nearby commercial hotel, made up of several elderly terraced houses, now happily demolished. The shouting of the cleaners and the insistence of the electricitymeter made sleeping during the day difficult emptier, and if we returned during the night the chorus of snores through the thin walls was unbelievable. Occasionally our time off enabled us to sample the delights of Edale or the Peak, or watch a second-grade film at the local cinema.
>
> It was also sometimes necessary to have one member of the party with sufficient athletic prowess to scale the wrought-iron University gate (whilst the others 'kept cave') in order to gain access to the computer building. Several of those who performed this feat have since reached higher directorate level."

In spring 1954 the new Director O. G. Sutton attended a computer conference in California which made great impressions on him (Sutton, 1954a, 1955b). Jule Charney had shown forecasts of the Thanksgiving Day Storm. He also learned about the newly formed Joint Numerical Weather Prediction Unit (JNWPU). This was, in Sutton's words, "a historic turning point of our science". When he returned home he sent Fred Bushby to Stock-holm for some weeks and Ernest Knighting for a year to the US, first to MIT, then to JNWPU[88].

On 15 Nov 1955 Bushby and Knighting reported to the 37th Synoptic and Dynamic Sub-Committee of the MRC on their visits to Sweden and the USA. Bushby had been for three weeks at Rossby's institution in Stockholm and gave an account of their electronic computations of +24, +48 and +72-h forecasts. Knighting had witnessed during his nine-month visit to the JNWPU+12, +24 and +36-h forecasts of pressure contour heights for 900, 700 and 400 hPa obtained from a three-level atmospheric model[89].

At an UKMO Monday Discussion 19 March 1956 Knighting (1956a) reported how the JNWPU ran 500 hPa barotropic forecasts over nearly the whole of the Northern Hemisphere to avoid boundary errors. These forecasts were intended to specify boundary conditions for baroclinic forecasts for smaller areas. Bushby presented early results of objective analysis. Parallel to their work on objective analysis[90], Bushby and Huckle (1956) worked on replacing the geopotential field by a stream function to avoid the spurious formation of anticyclones. They were successful and the tests showed positive results[91].

In May Norman Phillips, at a Royal Meteorological Society meeting, received his Symons Memorial Medal for his ground-breaking simulation of the general circulation of the atmosphere (Phillips, 1956). In the same month Knighting took part in the largest NWP symposium so far, arranged by the German Weather Service in Frankfurt, where he reported on the UKMO work (Knighting, 1957a,b)

In 1957 the team encountered a new problem. The forecasts were not only sensitive to analysis changes, but also to changes in the size and orientation of the grid!. This was seen by Knighting, Jones and Hinds (1958) as "a serious" problem and seemed to have and put the prospects of accurate NWP in doubt[92].

4.20 Computing and the general public

> "The practising forecaster, caught up in the daily whirligig of wind and weather…does not ask for exact solutions - not the complete canvas but merely for some background pattern on which to practise his art." Editorial in Weather, 1951, p. 322

The advent of electronic computing caused controversy, also into meteorology. Parallel to the mathematical, computational and scientific advances, there was in Britain an ongoing

[88] Ernest Knighting started as a teacher, but joined the Meteorological Office in 1940, first in the CFO, later with Sverre Petterssen's upper-air branch in Dunstable. He left forecasting in 1949 to undertake research.

[89] At the JNWPU he published, in February 1955, a Technical Memorandum No. 3, "The reduction of truncation errors in symmetrical operators" (Knighting, 1955).

[90] It has been difficult to form a picture of the UKMO work on objective analysis. For unknown reasons there were two approaches, one by D.H.Johnson (1956, 1957), Bushby (1956, a,b), Huckle (1956) and Bushby and Huckle (1957).

[91] In Bushby and Huckle (1956) two cases were reported neutral and one positive, whereas in Bushby and Huckle (1956) two forecasts out of three were said to have improved.

[92] This was actually two years before Lorenz' famous coffee break when the "butterfly effect" was officially discovered. But the Stockholm group was also aware of the problem and Roy Berggren made his doctoral thesis on the problem of errors in tbe initial 500 hPa analysis in 1956.

debate about weather forecasting with machines. The debates followed several lines.

In the January 1952 issue of *Weather* John Sawyer explained how the problem must be expressed as a set of mathematical equations, often partial differential equations. Bushby (1951a) had previously explained relaxation methods and their application to meteorological problems. Sutton (1954 a,b, 1955,a,b) did the same, but in a less technical way.

Knighting (1951) pointed out that the fundamental differential equations could not in principle be adequately solved, only approximately formulated. Sutton (1951) made clear that the initial conditions were insufficient to determine the final state. The apparently inescapable element of randomness in the atmospheric system would defeat all attempts to extend mathematical prediction beyond a certain interval of time. When Sutton as an example mentioned that "we may never be able to say with complete confidence that it will not rain on the vicarage garden party", the Editor answered in the next issue that he couldn't see any problem with that, rather the opposite[93]:

> "Wasn't there something to be said for the unexpected depression that washed out the vicarage garden party lest year?"

Sawyer (1952) wrote that whether success or failure was the end of the attempts at NWP, the experiment should improve our understanding of meteorological dynamics:

> "If we only learn that the theoretical basis of computations fails faults, the effort will not have been in vain".

Sutton (1954) saw "encouraging" results, about as good as by subjective methods:

> "In meteorology, the gap between the real situation and the ideal problem is much greater than in laboratory physics, so much so that it may appear sometimes that the mathematical meteorologist hardly lives in the same world as his 'practical' colleague. But the difference is one of degree only, and, not in principle."

Many, like Sutton, Eady (1955) and others expressed the view that there was a "disappointing slow advance" in synoptic meteorology, and the "one ray of light in the gloomy picture" was given by NWP. Sutton was, however, also careful not to distance himself from the forecasters:

> "This is not to say that ultimately the mathematician rather than the physicist will be the operational forecaster".

When there were comparisons between 'conventional' and 'numerical' forecasts, he replied that "no forecaster could be more conventional than the computing machine."

The most positive reactions came during a conference of Commonwealth meteorologists in May 1952. John Sawyer's presentation evoked a great deal of interest, and the ensuing discussion was mostly in the form of questions to the speaker: -How long did the complete process take? Was any smoothing of the data required? -Were more than two parameters necessary? -Could one improve on the geostrophic relation? -What hope was there for tropical regions where the geostrophic relation did not hold? (Met. Mag., 1952).

In December 1954 the 33rd Synoptic and Dynamic Sub-Committee of the MRC concluded that "the time was ripe for an intense effort" in numerical weather forecasting using an electronic computer. One reason mentioned was the need to extend the computational area. In November 1955 it became known that the UKMO had been granted funds to purchase their own computer, already baptized METEOR.

The new computer was expected to arrive in 1957, but it would be more or less three years until it had arrived. The years of waiting were to be rather frustrating for the meteorological staff.

4.21 The pessimistic years 1956-59

> "I am happy to claim membership in the forecasters group, once a forecaster always a forecaster." (Sutcliffe, 1956)

Shortly after the 17 January 1951 Royal Meteorological Society, where there had been heavy critisism of Rossby's barotropic

[93] It was not uncommon among meteorologists, even prominent ones, to be unenthusiastic about objective forecasting. In his WMO interview 1984 Alf Nyberg (Taba, 1984, p. 90) said that he might be "old-fashioned", but personally he did not find the prospect of entirely objective forecasting very attractive: "-What interest would there be in meteorology if infallible machines carried out the whole prediction process?"

concept, the November 1950 edition of Tellus with the stunning results of the ENIAC calculations arrived in Britain. Sutcliffe has later told John Burton (1982) how amazed he was:

> "-It was remarkable. I could not believe really that the barotropic assumption, which means a two-dimensional atmosphere treating the whole atmosphere as one uniform mass… I was very surprised, I think it was surprising…If you treat it as an incompressible fluid and extrapolate it that you get anything that appears to have anything to do with the atmosphere. It has got no vertical motion, no depressions, no anticyclones and no fronts. And yet – it was exciting from a mathematical point of view that this could be done and could be done by a computer in real time[94]."

Sutcliffe saw himself as a forecaster, scientist and as a teacher to forecasters. "The function of research was to feed professional forecasters with ideas":

> "-Wherein lies the special fascination of forecasting? Not in the satisfaction of success, although this is great, but in the very certainty that the prediction will fail, to a greater or less degree. We are taught to look for the day when machines will calculate the future weather with a monotonous degree of success, but if that day comes one satisfying profession will be lost to man and we must look elsewhere. There will be little more joy in the trade than there is in the repetition of the multiplication table."(Sutcliffe, 1956)

As he told John Burton later in life: "The computer had come to early." The full potential of the human forecaster, armed with Sutcliffe's rules and experience, had not yet been exhausted. But it soon turned out that the early ENIASC success was not easily repeated.

Sutcliffe's feelings toward NWP can be sensed in the evaluations in the annual reports from the UKMO reflecting the work 1950-54[95]. At first he was optimistic although the results were not as good as those provided by the forecaster. The pessimism started to set in 1955 when he wrote:

> "The work is still in the research stage, and success cannot be guaranteed, although there is good reasons to hope that the new methods will be a real improvement on the current procedures which leave so much to the experience of the forecaster".

In 1956 it seemed that development work over several years would be necessary to reveal "the full potential" of NWP. And the following year he wrote that the research "in the hands of a group of able mathematical physicists", was becoming "steadily more recondite and technical." In a talk in Sutcliffe (1957a) could not yet find positive grounds for expecting radical advance. It was "pipe-dreaming" to have confidence "in some genius of the future who will solve the problems which today seem so intractable…There might be some marginal improvement, but a far-reaching change is not at present in view". The nadir came in 1959 at a public meeting:

> "I am rather pleased than otherwise to be able to say that [NWP] is not yet a great success story so that there is at present no danger of the art of forecasting being entirely superseded and of my many friends who practise the art being made, as we say, redundant. This may come, I think to a large degree it will come, but not too quickly to be welcome."(Sutcliffe, 1960)

In the UKMO annual report reflecting the work in 1959 Sutcliffe wrote:

> "A few years ago the news of the day was the promising application to weather forecasting of calculations based on the theoretical equations of atmospheric fluid dynamics. The promise led to investment by the Office in advanced electronic computer…to the development of a team of half-a-dozen able mathematicians, and to a programme of research which is now yielding important results for the future of weather forecasting. But the fuller story of this project, for which future mathematical physicists still need to be recruited, may be left to a later year when it is hoped the results will be more definite."

The following year, the UKMO annual report contained an extensive article about the

[94] This surprise might explain why the contribution to the February discussion from Sutcliffe, as reported in QJRMS, which the speaker provided at some later stage, were more conciliatory toward Rossby's barotropic concept than what apprears from the immediate coverage in Met Mag and Weather.

[95] The annual reports covered the start of the year and 12 months back: the 1956 report covered March 1955-March 1956.

progress of the work – probably not written by Sutcliffe, because now the UKMO had something positive to report.

4.22 The Met Office gets its first computer

> "-The baroclinic models tested appear to be no better, if as good, as the barotropic model, and this is disappointing. But it seems much more disappointing that, despite all the efforts, so little had been learned about the physics of large-scale atmospheric behaviour beyond what we knew before." The American professor Robert G. Fleagle at a meeting of the Roy Met Soc 17 June 1959.

Finally in the summer 1958 the new computer, METEOR, arrived. As soon as it was installed in January 1959, the staff of the Dynamical Research Branch put the model to a prolonged test (Knighting et al, 1961).

The first experiment was run on almost every weekday from 12 Jan to May 1959 when 24 - and 30-hour forecasts were made from midnight data. The analysis was based on a +24 hour preliminary field from the previous forecast. The area had been extended to 24 x 20 grid points covering an area from Nova Scotia to Russia and Spitsbergen to North Africa. The grid lengths was variable with 244 km at 30ºN and 320 at 70ºN. Hopes for satisfactory forecasts were limited to an inner "verification" area – a rectangular network of 15 x 10 grid points covering about the same area as the CFO routine verification. There was simple allowance for heating of cold air over warm oceans. The usual problems were identified: non-adiabatic processes, lack of topography, analysis errors, boundary conditions and numerical shortcomings.

In this experiment a lot of manual work was needed to check and correct observations. In the second experiment from 27 July to 13 November 1959 this was made automatic (Knighting, Corby, Bushby and Wallington, 1961).

A third experiment was run 29 February to 2 June 1960 on a three-level model designed by Bushby and Claire Whitelam. It had three levels, 1000, 500 and 200 hPa and was mathematically constructed so that the 600 hPa level would represent a barotropic level (Bushby-Whitelam, 1961).

The results showed it to be superior to the CFO forecasts, apart from spurious anticyclogeneses and effects from the boundaries (Wallington, 1961). It was now decided to mount a fullscale real-time experiment in operational weather prediction from 21 November 1960 to 9 June 1961. Forecasts for 06 UTC were produced at 0930 UTC the day before on 1/3 of the days. About 1/3 of the time the computer failed.

The narrow boundaries caused problems also in the data assimilation. The six hour first guess became occasionally corrupted, in particular at the western boundary, which led to rejections of good data and acceptance of bad. One particularly revealing case from 1 February 1961 is discussed in Knighting, Corby and Rowntree (1962).

At this time Fred Bushby had left Bracknell to stay in Aden as Chief Meteorological Officer for 2 ½ years. This surprising move was the source of jokes and rumours for long times. However, John Sawyer and Mavis Hinds explained to me that since the UKMO was a part of the Ministry of Defence all staff (except females) had to take their turn (or turns) to man stations abroad. Fred knew that despite serving in Burma during the war, his name was near the top of the "Overseas List" in 1960. Since the UKMO was moving from Dunstable to Bracknell in 1961, it seemed to him wise to volunteer for a Aden posting, thus selling his Dunstable house before the prices dropped, and taking his son Peter abroad before schooling was vital. Bushby's stay in Aden might explain Knighting's absence from the major NWP symposium in Tokyo 7-13 November 1960. Britain was at this time imposing strict currency regulations for foreign travel[96]. Mavis Hinds thinks that if Fred Bushby had still been in England he might have fought enough to enable someone to go. Knighting mailed an article to the symposium reporting on NWP progress at the UKMO.

4.23 From METEOR to COMET

In June 1961 the METEOR computer was moved to Bracknell. By then it had become

[96] I spent summer 1960 at a camping site close to St Malo and I can confirm there there was not one single British tourist in the area, except a family of five who arrived late one night and put up their small single roof tent while the rain was pouring down. Next day they crept out of their overcrowded lodging soaking wet. They had been allowed out with £50 since the father was a war invalide. Although poor, wet and freezing they were always cheerful and in good spirit which impressed on us Continentals.

more and more obvious that the Meteor lacked the speed and capacity to enable forecast charts to be prepared regularly within the short time interval required by the forecasters. A decisions was therefore taken in 1963 to install a faster machine of greater capacity, the English made LEO KDF9. Most of the work in 1964 was to prepare for the new computer, already christened COMET. Staff training was started in summer 1963. Most of the testing of the machine was done at Kidsgrove, near Stoke-on-Trent, either on personal visits or by courtesy of British Railways. Paper tapes and computer output were conveyed overnight via Reading in special adapted tool-boxes.

The capacity of the new computer allowed the UKMO to extend the computational area to include the whole of Europe and the Atlantic north of latitude 30ºN. The calculations would use a three level baroclinic model with 1927 grid points in an octogonal area[97]. There were great hopes for forecast improvement of upper winds with direct application for the aviation. On Monday Evening Discussion 21 December 1964 it was reported that routine numerical forecasts should start when the new KDF9 computer was installed. It involved data extration, analysis and numerical forecasts. The discussion centered on the adaptation of these forecasts for operational use. It was pointed out that a computed chart carried no degree of confidence, and the forecaster might have a difficult decision when it differs substantially from his own expected development

The new COMET arrived in summer 1965 and in October the NWP production started again, but still on an experimental basis. After more than ten years of disappointments there was still a reluctance to take go operational. To dare this the UKMO needed another "comet", their new director John Mason!

4.24 A wind of change

> -But, Director General, suppose that the first forecast is a bad one, what then will you do?
>
> -Well, you had better make sure that is a good one!

John Mason arrived as Director General on 1 October 1965. His impression of the UKMO was that it had "an enormous potential" but was rather bureaucratic and a staff on the operational side which was lacking in confidence. *"-They needed encouragement and leadership".*

When the scientists showed him the three-level model and some of the forecasts, particular wind and temperature forecasts for aviation (200 and 500 hPa) Mason decided to take this forecasting out of research mode and into real-time. Looking at the statistics over the North Atlantic it was clear that the results were better than from ordinary empirical forecasting:

> "-If we go operational we shall then have to perform at concert pitch as opposed to a rehearsal and everybody will tighten up…"

Mason later admitted that he met "some opposition" to that. The deputy director felt they needed 6-12 more months of testing. But Mason's idea was to obtain maximum publicity for this major landmark in British meteorology:

> "-Not only are we going operational, but we are going to make a big announcement about it. We are going to have a press conference and we shall have the press and the radio and the television down here. And they should all see our first operational numerical forecast, letting them see it coming out of the computer and giving each delegate a personal copy.

The UKMO had never held such an event before, so many were naturally nervous of exposing "The Office" in this way.

The press conference went ahead and caused big headlines:

"£500 000 computer speeds up weather forecasting – Comet feeds on isobars"

"If the weather is bad – blame the computer in future".

According to The Daily Express, Guardian and Daily Telegraph Mason made clear that the human staff had "just about" reached the limit where their minds just could not absorb any more information, not if they are to continue working against the clock with their present "high degree of accuracy". In a few more months the COMET will be 'such a trusted

[97] UKMO Annual Report 1964. Perhaps there is a typo, because according to my maths it ought to have been 1921 points.

member of the staff' that its charts will be accepted without a qualm:

> "-It will relieve the forecasters of a lot of donkeywork. It will eventually increase the accuracy of weather prediction."[98]

This was, said Mason, a landmark in the history of British weather forecasting. He stressed that, computer or no computer, weather forecasting was still almost as much of an art as a science. The computer's production of a pressure chart was only the first step. The forecasters still had to interpret, using techniques from intuition to personal experience. The British people could look forward to a steady improvement in the accuracy of forecasts.

The most extensive coverage was in "The Times" by an anonymous "Science Correspondent". The insight and balanced views presented are the hallmark of John Sawyer or Ernest Knighting. On the other end of the spectrum was The Daily Mail whose readers were told that the output from the computer would be used only as "a second opinion" by forecasters who claim 80% accuracy using traditional methods.

The whole press conference was a great success. The real-time forecast had predicted cold winds and the first real frost for the winter. And so it was. It was also one of the best forecasts before, and for some months afterwards.

John Mason had got a flying start as Director General for the UKMO. One of the first to congratulate him was one of his deputies, Dr Best:

> "-Well, I must say, Director, I thought you were taking a great risk and I didn't think you should have done that. But I must say yesterday was a great day for the Office.

Yes, it was. To paraphrase David Brunt's word seventeen years earlier: *British NWP had "quite definitively got out of the doldrums".*

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[98] See Met Mag (1966) for a summary of Mason's talk.

-But, Director General….

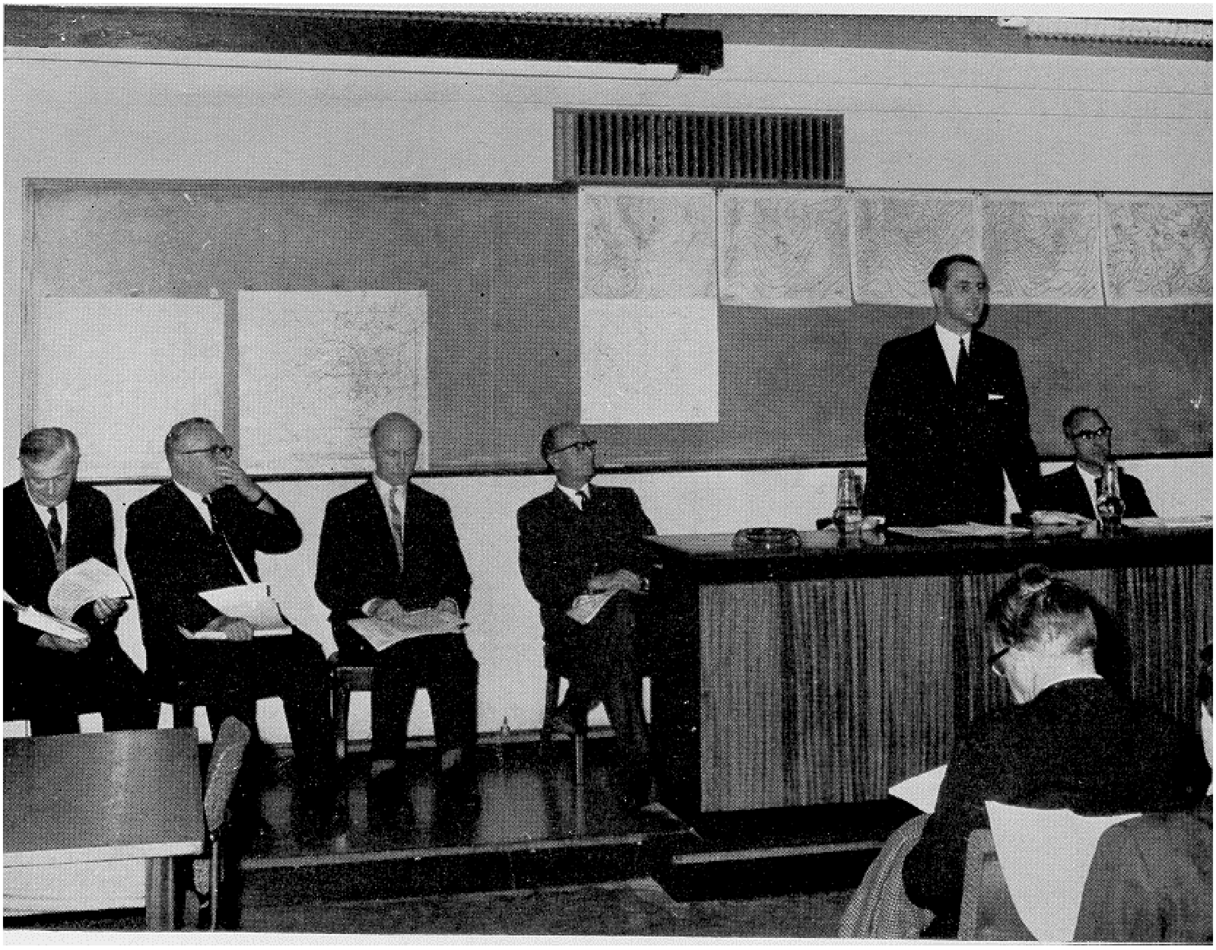

Fig. 13: One can almost sense the unease displayed by the UKMO management as they listen to John Mason during the historical press conference 2 November 1965. Left to right seated: V. R. Coles, T. N. S. Harrower, J. K. Bannon and N. Bradbury (Assistant Directors) and E. Knighting (Deputy Director). The charts on the wall display the current weather situation and the numerical forecasts.

5. Epilogue

"The curse of group velocity." Bo R. Döös, 2004

In a letter to the George W. Platzman, 29 October 1948 Carl Gustaf Rossby pointed out the importance of group velocity and energy dispersion since it dealt with "the very heart" of the Princeton NWP project (see also Platzman, 1979, p. 308 for a deeper discusion).

At this early stage of planning for NWP an understanding of the mechanism and relevance of group velocity ("energy dispersion" or "downstream development") was important for two reasons. First, it clarified the fact that the barotropic models were not just advecting wave patterns, but were able to modulate the amplitudes of these waves in a non-trivial way. Meteorological centres that *a priori* ruled out the barotropic model as basis for NWP were left with no other alternative than a baroclinic solution. This would unavoidably tax the computer's limited resources, which made compromises with the computational area necessary. This brought into light another crucial inference from the group velocity concept: the locations of the models' boundaries were defined by the speed of energy transport and thus by Nature. *There were no compromises to be made.*

Norman A Phillips in his 1990 WMO monograph on energy dispersion in atmospheric models (Phillips, 1990, p. 4) made the following reflection:

> "If Charney and his collaborators had chosen too small an area in which to make their computations, the first modern attempt at numerical weather prediction would have been severely degraded by the spread of errors from outside the small forecast area. If this had happened, the attempt at numerical weather prediction with the newly developed electronic computer of von Neumann might have been as discouraging as was Richardson's attempt 30 years earlier. Furthermore, the significance of the newly invented quasi-geostrophic theory of atmospheric motion would have received a tremendous setback. On the other hand, if a needlessly large area had been selected, the limited capacity of the electronic computer might have been exceed. Fortunately, Charney was able to apply group velocity arguments in a quantitative manner so that a reasonable decision could be made about the minimum forecast area."

What Phillips saw as a hypothetical mishap actually occurred, at least at one NWP centre. Circumstantial evidence suggests that it happened at other centres as well, although any negative experiences were never reported. We have also seen that what caused the misinterpretation was not the mathematics, but *their physical interpretation*. Everybody agreed that the effects of constant boundaries would spread into the computational area; the dispute was instead about the speed of this influence.

But whereas the followers of the "Chicago School" saw the energy transport as regulated by a "group velocity", others regarded it to be caused by an advection by the flow itself. But since this advective flow was considered to be the mean tropospheric flow represented by the 500-hPa winds, the two camps reached different conclusions.

Both sides did not realise that they were essentially looking at the same physical process that could be described by different mathematical formalisms: either by the concept of group velocity or advection by the wind. But in the latter case the main advection was carried out by *the upper tropospheric wind*.

This division of understanding is still with us today. The topic of energy transport in the atmosphere is treated differently by different authors that either look at it from a wind advection or from a group velocity perspective. It would then appear as if there are two completely separate phenomena. But, as we will see, the results of the authors' different interpretations do show striking similarities...

Independent of any theoretical models, synoptic investigations from Hovmöller (1949) and onwards agree that the transport speed is about 30º per day, which in the mid-latitudes corresponds to 2500 km/day. A *maximum* downstream development of 40º per day would correspond to 3000-3500 km/day. *This defines the distance to the western boundary for 24 hour forecasts*[99].

According to Norman Phillips (1990, p. 23), who argues from a group velocity perspective,

[99] Numerical aspects of lateral boundary conditions have been excellently treated in Kalnay (2003 ch.3.5 with references).

the state of the atmosphere at the beginning of a 24h forecast, must be known 3 400 km to the east and to the west[100], totally 6 800 km.

The British meteorologist Andrew Staniforth (1994, p.44, 1997) is one of the few who in depth has dicussed the problem of the computational area in relation to limited area models. He and his co-workers found from empirical tracing experiments, independent of any group velocity considerations, that the computational area should be 6000-6500 km wide (Chouinard et al 1994). Staniforth repeatedly made the point that the influence of information propagating inward from the lateral boundaries are "often overlooked" by modellers:

> "Care must be taken to ensure that the limited area is sufficiently large to guarantee that boundary-generated errors do not have sufficient time to reach the area of interest before the end of the forecast period."

Not only are the advice and concerns of Staniforth's "advective school" identical to Phillip's "group velocity school", their recommended area of 24 hour influence are almost the same!

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As we have seen, one reason for the success of early NWP in Japan and Sweden was that they restricted themselves to explore the barotropic forecasts on a properly defined limited area. But how much was this decision based on a deeper understanding of the group velocity?

There is no doubt that the Japanese experiments were guided by a profound understanding of the problem. Gambo (1951) provided an analysis of group velocity, inspired by the work by Rossby (1945, 1949) and Yeh (1949).

An indication that this understanding was genuine, and not merely a formal manipulation of equations, was Gambo's awareness that Charney (1949) did not exactly apply to Japan. Here, on the *western* parts of an ocean, little energy *arrives*, but considerable amounts of energy is generated and *advected away* (see ch.3.2.4 above)

So, what about the Swedes? Being on the eastern, *receiving*, side of an ocean they had an easy choice when defining their computational area. They could more or less take the values suggested by Charney's group velocity estimates, i.e. 53 º per day upstream and 32º per day downstream. And so they did – and that could be the end of the matter.

But did they really know what they were doing? If we look at the written evidence there is nothing to suggest that they did!

As a matter of fact, during the compilation of this overview, Bo Döös told me that although there was a general knowledge about group velocity as such in the Stockholm group, they were unaware that it applied to defining the size of the NWP area.

Bolin once touched upon this at an Air Force seminar 1951:

> "-How fast will the conditions within one region of the map affect the development at another place? Charney has here been able to show that the effective 'signal velocity' is of the same order of magnitude as the characteristic velocity of the flow itself. This corresponds to a velocity of 15-20 m/s [or about 1500 km per day]. This is an encouraging result…"

Perhaps too encouraging, because 1 500 km per day is a clear underestimation of the influence zone.

Arnt Eliassen who was strongly linked to the Stockholm group, in particular in 1951-53, some years later wrote two brief introductions to NWP (Eliassen, 1956, 1957) where it is obvious that he has no understanding of the energy propagation process, at least not in relation to NWP areas.

In an English speaking chapter in "Handbuch der Physik" (Eliassen, 1957) he made a vague reference to the ENIAC calculations and to an unpublished lecture by Hinkelmann at MISU in 1951. For "actual forecast problems" he only said that the effects are "most strongly felt in the vicinity of the boundaries, in particular where there is strong inflow". His only advice is to place the boundaries in dynamically less active regions such as the subtropical high-pressure belt! In his second text, in Petterssen's "Weather Analysis and Forecasts" (Eliassen, 1956) he severely underestimated the effect by suggesting that boundary influences in a 24-hour forecast "do not penetrate more than about 1000 km" into a region.

[100] The boundaries should be shifted westward with an amount corresponding to the mean tropopsheric wind speed.

So nowhere in the written documentation from the MISU/IMI experiments is there any group velocity discussions. What might have occurred is that the Stockholm group just followed their Master. But Rossby died in 1957, which appears to be the year when group velocity disappears in the meteorological science[101].

A frequent guest at MISU was Phil D. Thompson. In 1961 he, together with Norman N. Richardson from the US Air Force, in some papers discussed the problem of how rapidly information from surrounding data rich areas would positively affect an inner large data void area, what Thompson called a "hole". His mathematical model describing how the "hole" was "filled" did not make use group velocity concept. It was purely advective using the 500 hPa mid-tropospheric flow. His co-worker's barotropic simulations on BESK showed that the "hole" was influenced and "filled" slightly faster than Thompson's model predicted. Richardson (1961) suggested that boundary influence and "region of influence effects" contributed to the reduction of the errors.

During a longer stay at MISU Phil Thompson wrote his well-known textbook on NWP (Thompson, 1962). There is a lot of valuable information in that book, but not any mention about the computational area problem. James Holton, who started to write his textbook (Holton, 1972, pp. 176-77) during a stay at MISU, had no reason to go deeper into NWP. He mentioned group velocity, but only with respect to ocean waves (where the energy travels with half of the phase speed).[102]

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There is a final twist to the story: *the Swedes were double-lucky!* Not only did they define the NWP area large enough, they also avoided as well making it *too large*! Due to their limited computer resources they could not, as the Americans did in the late 1950's, run their barotropic model on a hemispheric scale. Because of this they also avoided the serious problem of retrogression of the ultra-long planetary waves. In the Swedish NWP system these waves remained quasi-stationary, locked in position by the constant boundaries!

To be fair, the Swedes knew about their fortune - indeed it was Bo Döös who told us when I was his student in 1966!

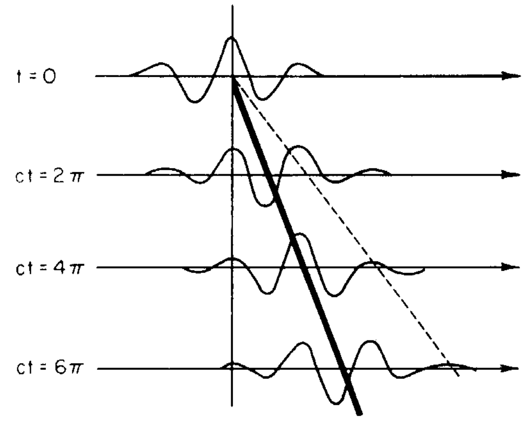

Fig. 14 a: The group velocity figure from Holton (1972, 1979 and 1992) shows schematically the propagation of *ocean* wave groups, for which the group velocity (thick line) is half of the phase speed (dashed line).

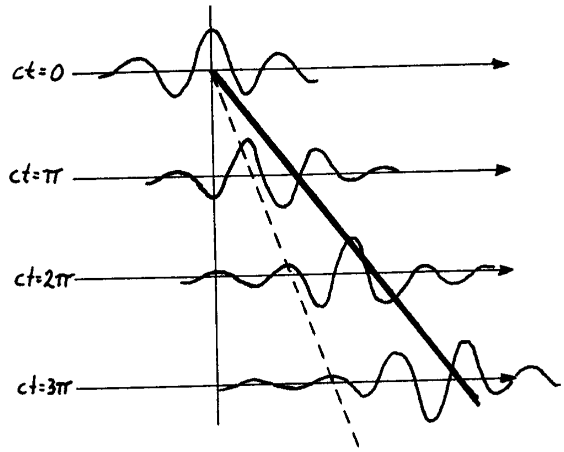

Fig. 14b: The exact figure (Persson, 1993, 2000) that was intended to appear in Holton's 4th edition. The group velocity (heavy line) is faster than the phase velocity (dashed line) for large-scale motion in the atmosphere. Unfortunately, the figure that was printed (fig 7.4) is still not quite accurate.

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[101] Persson (2000). Group velocity was "re-discovered" in the late 1970's by two young English meteorologists at Reading University (see papers by Hoskins and Simmons and others 1977-81). See also Miles (1959) for an independent UKMO detection of the process. The phenomenon of "group velocity" as such was actually a genuine British discovery (by Rayleigh in 1881 to explain apparent inconsistencies in Michelson-Morley's experiments to determine the speed of light).

[102] "If there ever iss a 4th edition I will certainly take your comments into account. Of course, what is really needed is a good synoptic meteorology book that would address [downstream development] instead of describing 49 different kinds of occlusions" (Letter J. R. Holton to A. Persson 13 March 1995)

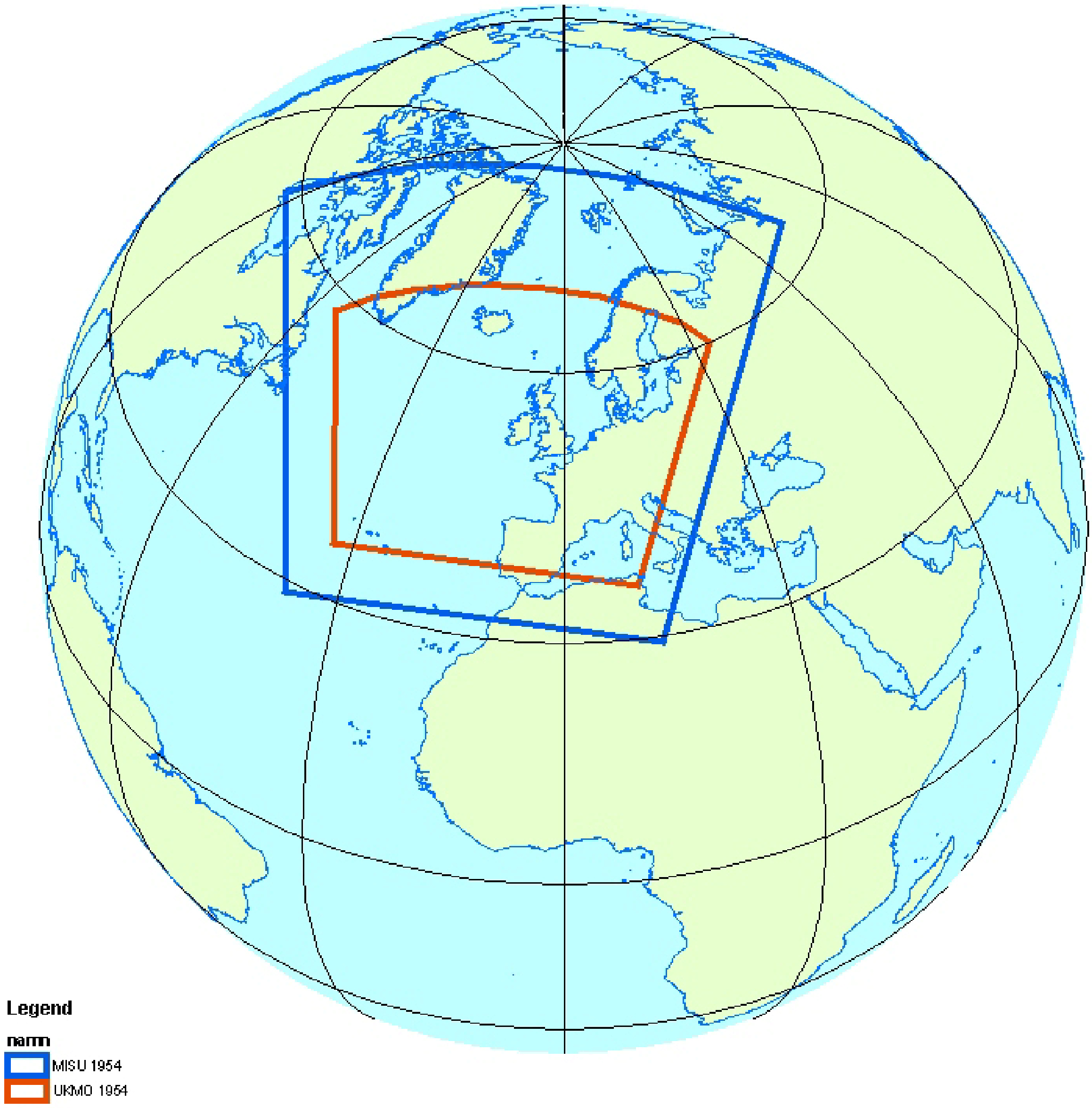

Fig. 15a: The computational areas, for which the boundary conditions were constant, for the MISU +24 hour forecast experiment 1953-54 (Staff Members, 1954) and for the UKMO +24 hour forecasts (Bushby and Hinds, 1954). While in the UKMO area (red line) the British Isles are just 40 longitude degrees or 2 500 km away from the western boundary, in the MISU/IMI area (blue line) it is more than 60 longitude degrees or 3 500 km from Sweden (at 50º N). In other words, while the effects of the western boundary in order to reach the UK only have to travel with about 30 m/s in the UKMO area, they will have to travel with 40 m/s to reach Sweden in the MISU area. Charney's recommendation (Charney, 1949) was to design the area after the *maximum* group velocity.

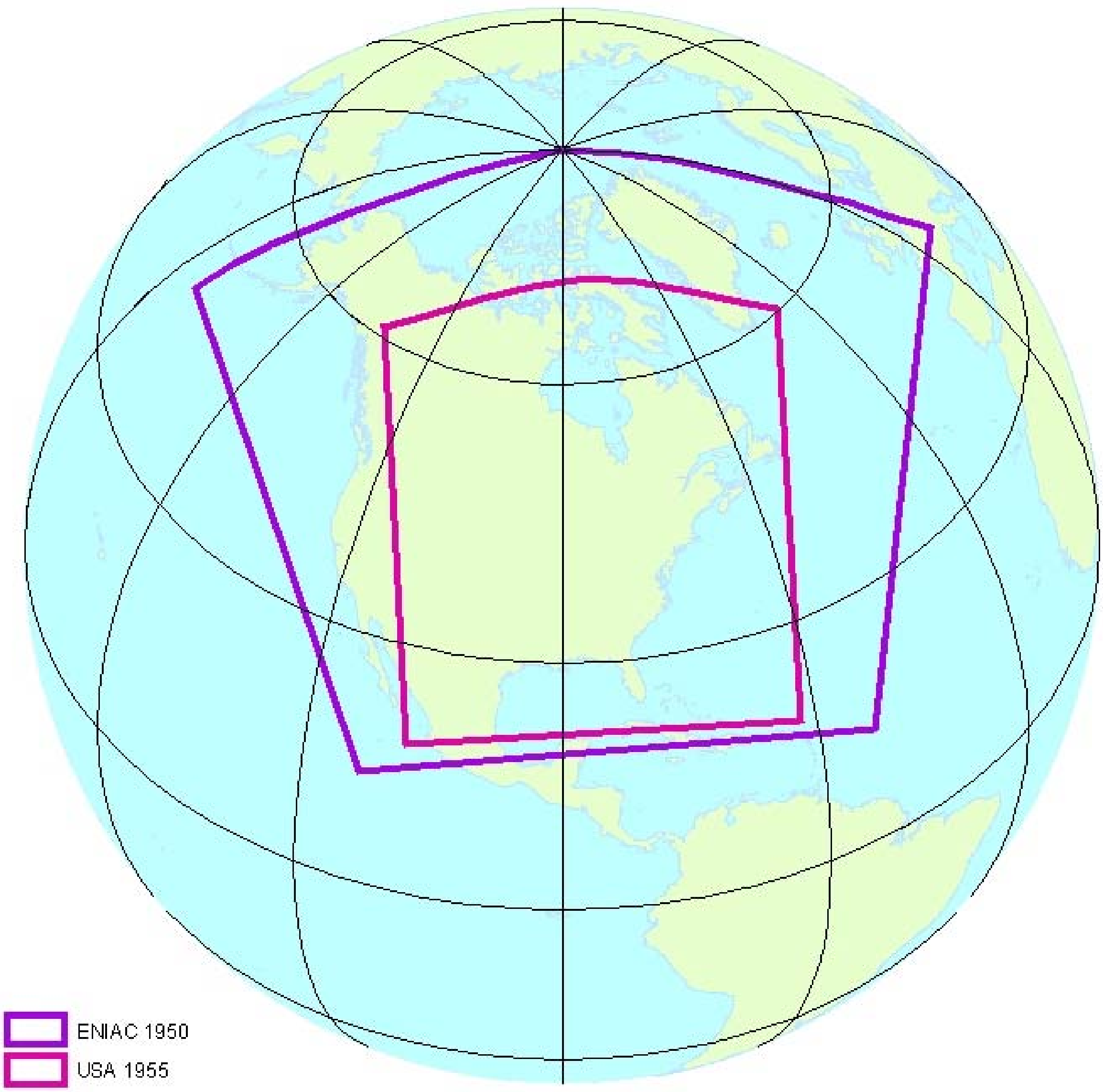

Fig. 15 b: The corresponding +24 hour integreation areas for the 1950 ENIAC (outer area) runs, and Charney-Phillips, 1953 (inner area).

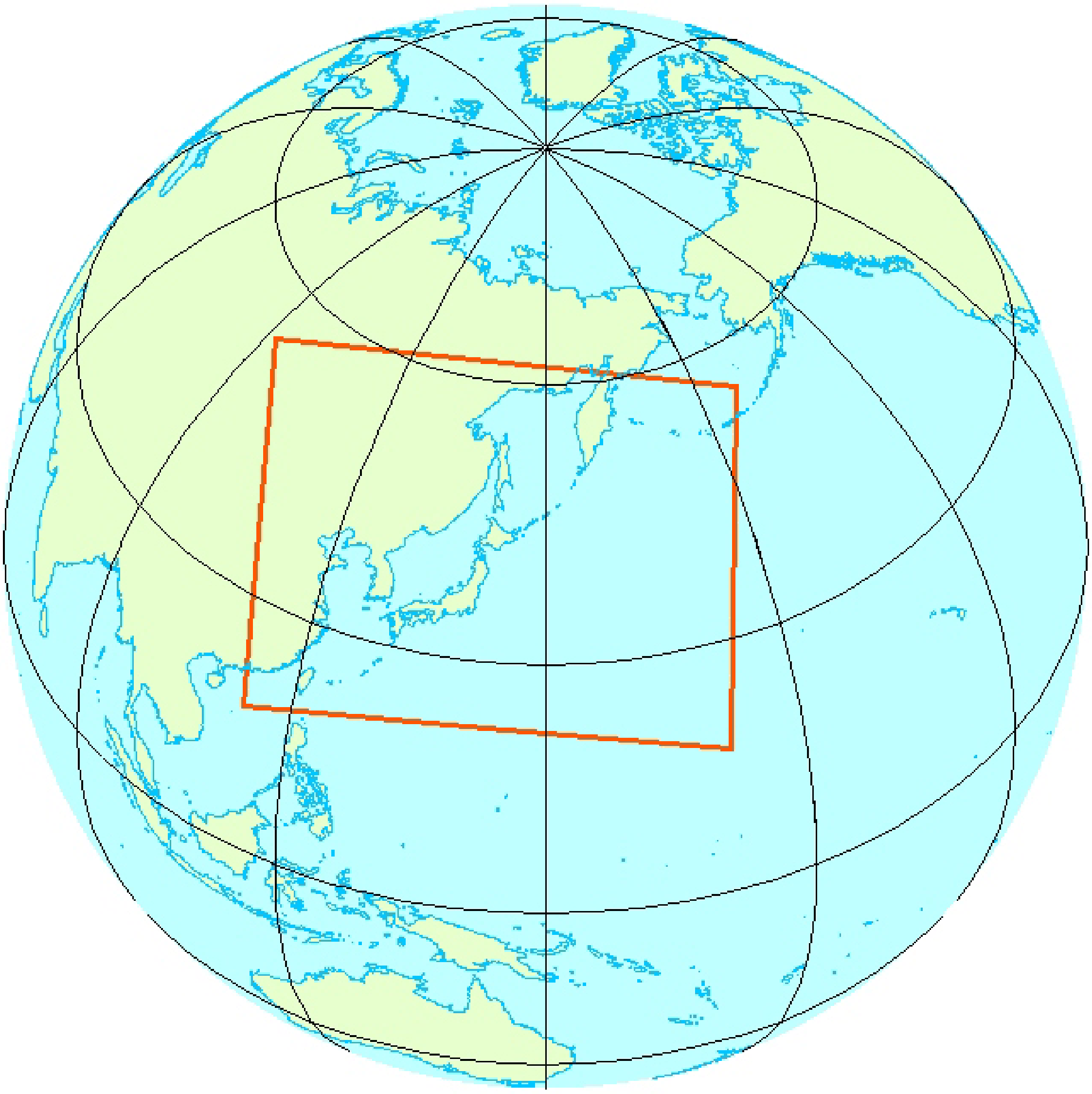

Fig. 14 c: The +24 hour integration area applied by the Japanese modellers (Staff Members, 1955). Being on the western side of an ocean (and the eastern side of a major continent) less energy arrives from upstream than departs downstream.

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APPENDIX 1

This report has not yet been published and is to be treated CONFIDENTIALLY. Its contents must not be quoted [later crossed over and stamped "Met. Office, 5 July, 1948 Library]

M.R.P. 412

S.C. II/9

17 June 1948

AIR MINISTRY

Meteorological Research Committee

1. Two meetings were held on 25 May and 17 June 1948 to report on "The Possibilities of Using Electronic Computing Machines in Meteorology" which were attended by

Dr G.C.McVittie,

Dr. R.C. Sutcliffe,

C.S.Durst

E.T.Eady.

2. At the first meeting a general discussion took place at which Mr Eady stated that he hoped to pose to the Cambridge Machine simple questions on the effect of perturbations on a uniform baroclinic flow of air. He anticipated that these perturbations would develop into disturbances similar to the depressions in the atmosphere. Dr Sutcliffe argued from the forecasting point of view that it was important that actual meteorological situations should be put to the machine to discover if it were capable of solving these situations. Further discussion took place on the difficulties that would arise in posing the boundary conditions and on the limitation of accuracy due to inherent lack of precise measurement of wind. The conclusion was reached that further progress could not be made without the presence of some expert accustomed to the use of mechanical methods in computation."

3. For the second meeting arrangements were made for the assiatnce of Mr. Wilkinson of the Mathematics Branch at the National Physical Laboratory.

4. At this meeting the general problem was outlined to Mr Wilkinson by Sutcliffe, and Mr Eady gave further details of a more limited problem. Mr Wilkinson stated that the problem had to be put to the machine in the same form as though it were to be solved by a very large number of Brunsvigs working for a very long time. The gain was merely speed. The memory of the NPL machines as designed was 4000 numbers. This would be sufficient for a set up of 10x10x10 observations. The machine now being designed might be ready in 1950, but a pilot model with a considerably smaller memory would probably be ready about March 1949. It was possible that by methods now being developed the memory of future machines would be increased by many times and that the speed of manipulation would be increased. Mr Wilkinson did not think that the machine at present contemplated would be capable of dealing with the general problem. The more limited problem suggested by Mr Eady would be within its compass. He did not think that with its present designed speed would be able to work faster than the weather.

5. After further discussion it was agreed by the meeting that the machine as designed ought to be tested by the posing of simplified problems. It was further felt that to take a long view it was advisable that the

Meteorological Office should establish contact with the computational side of mathematics in order to be prepared in the future to take advantage of the developments, which will take place. There was a strong feeling that this contact could only be obtained by someone who was familiar with the methods of computational mathematics and also synoptic meteorology.

6. There was considerable discussion as to whether it would be better for this contact to be a meteorologist who then studied computational methods or a pure mathematician versed in computational methods who then spent some considerable time learning synoptic meteorology. The latter view found most support. It was felt, moreover, that apart from the possible use of the machine, his knowledge would be extremely valuable to the UKMO on other problems.

7. The recommendations of the meeting were

(a) That limited problems should be given to the machine as soon as it was ready to deal with them

(b) That Mr Eady should keep the Meteorological Research Committee informed of his progress.

(c) That the recruitment into the UKMO should include one or more mathematicians who were specially qualified in computational methods. After gaining the necessary knowledge of synoptic and dynamical meteorology such recruits would be available to undertake research into the formulation of meteorological problems in the manner suited to calculation.

APPENDIX 2

<u>Rossby to Charney 25 February 1954</u>:

"In London I attended a staff meeting (last Monday, Feb 15) at the Met. Office, at which Bushby reviewed the five first forecasts made for 12 and 24 hours by the Dunstable group. They certainly are to be congratulated on being the first official Weather Service to undertake this, but I am a bit concerned about the direction of their work. It is of course difficult to judge from a lecture and a fleeting look at slides, but I had the impressions:

a) They must use some very peculiar boundary conditions because several of the 500 mb charts showed a most remarkable evidence of some sort of instability in the border regions (wobbly contours etc.)

b) The central portions of the 500 mb charts seemed to give quite good results

c) The predicted sea level charts appeared to be far inferior to the 500 mb charts, but again, I am not sure of this

Wednesday last week, after Joe and two Dunstable fellows had presented papers before the Met. Soc., I was asked to go back to the problem of numerical forecasting and I then made a few points which I shall state below, in the hope we may be able to discuss them when I see you in Princeton.

<u>If you base numerical forecasting on the vorticity equation</u> then, regardless of the number of parameters *1,2,3) you use, one and the first of your equations is a statement concerning the vertically averaged motion (as you have shown). The other statements are obtained, <u>in effect</u>, by differentiating the vorticity equation once or twice with respect to z.....

I am therefore inclined to believe <u>that for the time being the principal value of the higher parameter models lies in the fact that they permit us to predict the averaged (i.e. the 500 mb motion) more accurately than does the barotropic.</u>

In the Wednesday session I suggested that the Dunstable outfit (assuming the machine is available and is fast enough) should make its first task to issue, as a routine, during a limited period, daily 24 (or 48 hour) forecasts of the 500 mb chart, to accustom the forecasters to this forecast aid, but to consider the prediction of the sea level charts as a research project not yet ready for routine tests.

In Stockholm we are certainly going to concentrate on this approach and as soon as the drum is ready for use I hope we can go on the 48- or even 60-hour forecasts, using a larger net of observations (with a one- or two-parameter model).

There are also practical and tactical reasons for this.

1. The detailed plotting and analysis of several upper air maps, twice daily, plus the reading off of a large number of initial values + the punching of these data on tapes will take too much time that a 24-hour multi-parameter model forecast hardly can be completed earlier than, say 10 hours before the expiration of the forecast interval.

2. I sincerely fear that sea level pressure distribution forecasts, being extremely sensitive to the details of the temperature distribution and to a variety of assumptions implicit in the models, are going to expose us to gleeful criticism from the old-time school of forecasters

3. You must not jump (skip) stages in the mental development of the forecasters on routine duty. The routine forecasters at Dunstable do not, I fear, possess much of an understanding of the 500 mb (or any other height) chart, and it would do them a lot of good to watch these charts for a long period.

APPENDIX 3

<u>Rossby to Dahlquist 21 March 1954 (translated from Swedish):</u>

As you might already know I am now trying to get a contract with the USAF organization which Phil Thompson represented last autumn, for work with numerical forecasts. I have suggested one after Swedish conditions rather large sum of $30 000 per year. If we get the contract, it should run for a couple of years, but it is also possible that they start a half-year contract already before 30 June (to give us possibilities to expand this summer with the fund from this year). The contract may be prolonged once a year, the first time autumn 1954.

Bolin has a copy of my suggestion. If it is accepted we might have to stick close to the suggested program. I had the other day a discussion with some of the scientists and department chiefs at GRD [Geophysical Research Directory, see p.11] concerning the suggestion. Perhaps you and Bolin and Bedient and Döös and Wellander should think a bit how the program can be realized.

[Inserted in the margin: “The contract takes care about half of your salary and makes it possible for us to pay for time at the computer”]

Now I would like to ask the following:

If we work for example barotropic with a grid net of 20 x 20 points and one hours time interval, the forecasts become worse and worse the longer out in the time we go. This can be due to three different factors:

a) The model is too simple and fundamental baroclinic processes, which are not included in the model, destroy the forecast.

b) The boundary conditions are not the proper ones. The influences from outside, which are not accounted for, destroy the forecast after a while

c) The forecasts blow up due to truncation errors (finite differences)

Would it not be possible to set up a computational scheme with the observed, instead of the arbitrarily assumed boundary conditions along the periphery of the forecast area? Doing this the effects of b) would be eliminated. Unfortunately the aerological observations are generally given only every 12th hour. It will probably be necessary to develop an interpolation method for time intervals between the aerological times, and for the first tests these extrapolations should be carried out subjectively by "experienced" synopticians. Later one can, I believe, let the machine carry out the interpolation.

If it now turns out that one can keep the chosen forecast model going in the machine a longer forecast interval (2-4 days?) without the forecasts for the inner area becoming too stupid, we would be some way down the road towards forecasts for unknown areas, because sooner or later influence of the initial values must disappear. I would now be extremely grateful if your group could give some consideration to this question if

a) a suitable working area (the "unknown" area must have good observations because else we would not be able to test our results).

b) how one can introduce efficiently the correct boundary values in the computing scheme (Is the drum [=drum memory] needed for this purpose?)

c) select preliminary synoptic situations to interpolate the boundary conditions between the observing times.

d) what is the correlation between a given distribution of the contour values at a given time and twelve, twenty four, thirty six etc. hours later, in other words, to what extent is the pressure distribution dependent on the initial values within the same area? This is a statistical task and perhaps we should compute such "serial correlations" on the machine for a number of charts?

I believe that the working tasks now are growing, also our resources, so that we calmly can take the responsibility to tempt several new individuals here, as Wellander's type for example.

Eriksson needs desperately a gifted and fantasy rich man (Phil Cand) with grades in <u>experimental physics</u> and <u>chemistry</u> and either mathematics or mechanics, The main thing is, however, brightness, interest and initiative. Can you help him by interesting someone of your associates at the University? You could use the 'slogan': *"Join the Met Institute and see the World."*

Tomorrow Harriet and I travel to New York and on Wednesday morning on to Washington. Sunday 28 March we will take the train to Chicago. The 4 April we are back in New York. Saturday 10 April we fly to Paris, where we arrive 11 April. After a couple of days there we take the train to Brussels, where Harriet takes the plane to Stockholm. I fly from Brussels over London to Stockholm. It will be a delight to see you all. Harriet and I hope you and Marianne [Dahlquist's wife] are comfortable in the apartment? Have my children ruined you? In that case write immediately to Chicago. It is obvious that Carin [Rossby's daughter] has a very good time with the Dahlquist family.

Warm greetings

Yours affectionate

C.G.R

Appendix 4:

Ödesballaden

Det var strid uppå kniven om Öde-stolen
Över Anders låg molnen och skymde för solen
fanns det tro, fanns det hopp eller ej?
Och Herbert, den token och kvirrkommendanden
Han slet med sitt schema på avgrunsbrantem
för hudeli-hudeli hej

Där var Oscar den präktige stabsmeteorologen
han var fager och fin, hade Besken i hågen
och till Rossby han kila' så tätt
Där var Affe, den trotsige, vandrade sällen
som dök uppöverallt men på orätta ställen
där var C. C. Men han vägde för lätt

Där var Berschan i Bredgränd och Köhler på backen
det är pojkar som brukar vända på klacken
som gnälla och träta om trän.
Där var Valter på torpet and Trysse i Svängen
Där var Jonke med pappret och Högberg, gamängen
och kanslister med släng av migrän.

I korridorer och rum och vid papperens prassel
var det visk, var det snack, var det tissel och tassel
blir det han, blir det hin, blir det han
Det var ros, det var ris över chefskandidaten
han är inkomeptent, han är självklar för saten
och han vill nog om bara han kan.

Det var fart det var eld över regnmakargänget
de var här, de var där, de var ständigt i flänget
och Oscar de hade så kär.
Det kutades friskt upp till stadsråd en skara
dock inga som leddes av hämningars fara –
Vill du ha mig så har du mig här!

Dock det bryggdes en brygd utav Anders den vise
Som ej skulle skänka sina drickare lise
Den var besk, men den gjorde sin sus
Som en blixt slog det ned ifrån rådsalsborden
Att Alf hade ansetts som bäst uppå jorden
Och Anders han sken som ett ljus.

Och en räv stämde in i den lustiga låten
Och en kuf skrek Uhu! Ifrån Lindhagensbråten
och de märkte men hörde det ej.
Men Uhu! Hördes ekot i flygvapnet skria
och till svar på Carl-Gustavs hudeli-dia
kom det hudeli-hudeli dej!

The D-ballad

They were fighting with knives over the D-chair
Over Anders hang the clouds blocking the sun
Was there any confidence? Was there any hope or not?
And Herbert[103] the joker and leading grumbler
He struggled with his rooster at the edge of the abyss
For doodeli-doodeli-dey

There was Oscar the decent Staff -Meteorologist
He was handsome and good, the BESK in mind
and to Rossby he hung so tightly
There was Affe, the obstinate, wandering chap
who turned up everywhere, but in the wrong places
There was C.C.[104] be he was too light-weighted

There was Berschan in the Alley and Köhler on the Hill.
They are guys who can kick up their heels
and grumble and quarrel about trees[105].
There was Walter in the Cottage and Trysse in the Bend.
There was Jonke with paper and Högberg the Gamin and
the assistance clerks with a touch of migraine.

In the corridors and offices to the sound of rustling papers
there was whisper, there were chats, and tittle and tattle
"Will it be him?", "That bastard?" "Will it be him?"
There were roses, there were rods over the favourite
"He is incompetent", "He is the obvious choice, damm",
"And *he* is willing, if only he gets a chance"

It was swing, it was flames with the rain-making band[106].
They were here, they were there, they were incessantly
dashing around and they were so in love with Oscar.
There were those who run up to the Ministry.
And none of them suffered too much shyness. -
Do you want me? Then you have me here!

Yet, a drink was brewed by Anders the Wise.
It was not intended to give the drinker any relief
It was bitter, but it did the trick
As a lightning came the message from the Government
that Alf was regarded as the Best in the World
And Anders shone like a light

And a fox joined in the funny tune.
And an oddball shouted "Uhu" from Lindhagensgatan[107]
And they didn't notice it, they didn't hear it
But "Uhu" the echo came from the Air Force
and as an answer to Carl Gustaf's "-Doodelia "
came a "- Doodeli-doodeli-dey

[103] Anders Ångström, the then Director General of SMHI. Herbert Henriksson, Head of the Central Forecast Office

[104] Oscar Herrlin, Head of the Military Weather Service. Alf Nyberg, Head of the Meteorological Divison at SMHI, was later to become president of the WMO. C.C. Wallén, leading climatologist, later prominent within the WMO

[105] Tor Bergeron and Hilding Köhler, professors in meteorology in Uppsala, were not on good terms. At the time they could not agree on which trees in the institutions garden that needed to be cut.

[106] Rossby and his institution were in the early 1950 heavily engaged in the problem of artificial rain making.

[107] The address of Rossby's institution

APPENDIX 5: A FEW EXTRACTS FROM THE JOHN S. SAWYER CORRESPONDENCE 1993-1999:

"I had often thought there are a lot of phoney explanations of the Rossby wave in the literature, and it is good to see them exposed", *10 Sep 1993* (commenting on my draft "Seven ways to misunderstand the Rossby wave")

"Your letter [about the UKMO contribution to NWP] seems to me to provide a penetrating analysis of the ideas of the time. I am sure that it is very near to the truth. As you realise, we did look for evidence of 'energy dispersion' when we read about it in the literature, but could not see convincing evidence for it on the weather map. As regards the size of the NWP computation area, I agree that, in retrospect, it was too small, but it was constrained by the computer storage capacity and the desire to include two or more levels." *23 October 1993*

"I do not think that your views on [the UKMO] in Dunstable of the 1950's will ruffle any feathers of those who worked there." *19 November 1993*

"The [February 1954] Rossby letter which you sent me is very interesting. However, his remark about the Dunstable forecasters is not very fair. In the 1950's there was a daily discussion of the 500 mb chart as well as the surface chart. It is true that there was not very much real understanding of what was going on, but the forecasters had a close familiarity with the 500 mb flow and experience of development." *20 December 1993*

"I remember no problems in the relation between Fred Bushby and me. We always seemed to cooperate easily and effectively. You are right to say that our personalities differ. Fred Bushby was probably more interested in the techniques of computer programming (a novel field at the time) than in the fundamentals of meteorology. Also one must remember that only a favoured few could make a career in the Met. Office in research alone, and Fred Bushby, no doubt, would have wished to prove himself competent in the technical and organisational side of the Office work…Indeed Fred did make his mark in both research and the service side of the Office." *10 July 1995*

"I have always regarded [potential vorticity] as an important tracer, and useful to illustrate and explain some features of 3-dimensional dynamics structure. However, I did not regard it as of much use in seeking the cause of meteorological disturbances – indeed it seemed rather to confuse than elucidate what is going on. The stratosphere has largely to adjust to developments in the lower atmosphere, and probably sometimes acts as a 'damper' – but not as a 'trigger'" *11 June 1996*

"As regards your draft on the history of NWP, I have no comments of any importance. Your account seems to me to be a very fair one. With the passage of time it is possible to see how the technique developed and false tracks were followed. It did not seem to be quite the same at the time. However, I think that your comments are very fair. I think that the major influence on the way we went at Dunstable was a view that the prediction of baroclinic developments should be our aim; otherwise we could not predict the surface chart. For this reason we did not regard the barotropic forecasts as very worthwhile. As you say, we were over-optimistic of finding a way to fudge the boundary problem." *30 November 1996*

"I enjoyed reading your paper about Coriolis and his theories…However, there will be readers who are not pleased to be shown the slip-shod nature of some of their dynamical explanations of atmospheric motion." *7 July 1997*

"The ECMWF has progressed far beyond what I thought was possible when I attended the early meetings at which the idea was suggested. I was somewhat of a sceptic. I remember that P[atrick] J. Meade, the Director of Services at the Met. Office was much more enthusiastic than I was, and was keen to have the new centre in the UK.*" 7 July 1997*