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Early operational Numerical Weather Prediction outside the USA: an historical Introduction. Part 1: Internationalism and engineering NWP in Sweden, 1952–69

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1. Introduction

The story of numerical weather prediction (NWP) is a great story. It deals with the first successful attempts 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 cannot be fully understood unless non-mathematical and non-technological facts are also included. Meteorological understanding, political considerations and human emotions are difficult to quantify, but they 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 in 1922 (Ashford 1985) to the start of operational primitive equation models some 40 to 50 years later. But Man is an impatient animal, reluctant to sit passively and wait. The years 1950–70, when computers were in their infancy, saw many attempts to construct NWP systems using available resources. Some were successful, others gave up, but many kept struggling just for the sake of it.

These three articles (Part 1: Sweden; Part 2: Other centres; Part 3: United Kingdom) have benefited from contributions from and discussions with many colleagues. For this Part I am particularly grateful to Pal Bergthorsson, Germund Dahlqvist, Bo R. Döös, Jean-François, Nils Gustafsson, Eero Holopainen, Ernest Hovmöller, Per Kållberg, Olof Lönnqvist, Lars Moen, Alf Nyberg, Jussi Rinne, Daniel Söderman, Bengt Söderberg, Aksel Wiin-Nielsen and the historians Anders Carlsson and Kris Harper.

If, over the years, I have sometimes 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.

The very first operational, real-time +72 h NWP was run in Sweden during a military manoeuvre in late September–early October 1954 and operational production started in December. (fig. 1) 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 (May 1955). The success placed Sweden at the forefront of NWP, together with the USA, Japan and the UK, a position it would maintain until the early 1970s.

2. Meteorology in post-war Sweden

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

2.1. 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. (Sutcliffe 1957, 1958)

Why did Carl Gustaf Rossby leave the USA and return 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 get on well with the administrators at the University of Chicago. Others have said that he found life in the 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)

Figure 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 using the Swedish computer BESK was, from 1955, displayed on a cathode ray screen or through a facsimile machine. See Döös & Eaton (1957) and Wippermann (1958) for contemporary details. The picture is taken around 1956. (Copyright Pressens Bild.)*

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. After the Second World War it was 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 1930s with the intention of working with the famous oceanographer Vagn Walfrid Ekman at the University of Lund. Ekman was due to retire in 1939, so the idea may have been for Rossby to replace him. Before the war some leading scientists had approached Rossby to bring him back 'home'.[1] With the war was over, and these approaches were renewed.

In 1945 the Swedish government initiated a new commission to look into meteorological education. The two commissioners, Harald Norinder, Professor of Atmospheric Electricity at Uppsala University and Hans W:son Ahlmann, Professor of Geography at Stockholm University, had good contacts with Swedish meteorologists, in particular with their old friend Rossby. Their report concluded that it was important to get him 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. (SvD and *Dagens Nyheter* 31 January 1946)

Rossby visited Sweden in January 1946 on his return from a visit to the USSR.[2] He and three other meteorologists met Tage Erlander, then minister responsible for education.[3] We know from Erlander's published diary that this meeting was on 19 January

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

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

[3] Erlander would soon be appointed prime minister and serve for 23 years, 1946–69.

1946. The story is that Rossby had brought with him weather maps, which he displayed on Erlander's desk, over ringing telephones. Erlander had a solid education in mathematics and physics, and was in fact married to a mathematician. So he was, unusual for a politician, well informed about the scientific problems Rossby laid out for him.

Rossby recommended that a new chair for dynamic or theoretical meteorology should be created at 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 (Wiin-Nielsen 1997: 44). We do not know from Erlander's diary what Rossby's answer was, but in a newspaper interview (*Dagens Nyheter* 3 February 1946) Rossby expressed doubts about a permanent move, but was quite willing to consider a 'temporary employment' in the 'forthcoming years'.

2.2. Foreign influences

> Increase the heterogenity! 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: *Dagens Nyheter* 3 May 1950)

When the Big Bird from the Great Lakes landed in the Swedish pond, he created waves from which Swedish meteorology would draw energy for decades to come. His arrival was very timely: Swedish meteorology was just starting to change to meet the post-war challenges.

This was post-war transition period was a stimulating one, with international exchanges of observations and new opportunities. But there were also new demands. In 1944 an official commission found that SMHI lacked sufficient numbers of educated personnel. Tor Bergeron, then aged 50 and still working as a chief forecaster, was promoted to a post at the university and made responsible for education. An embryonic 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 Swedish Air Force. Under its new leader, Oscar Herrlin, it expanded to become the 'Military Weather Central' (MVC). The Air Force itself 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 flights over the Atlantic started in 1945 the aviation authorities had to hire British and American forecasters to prepare the upper-air aviation forecasts. The problem was raised in the Swedish 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 at the UK Met Office; Ernest Hovmöller came from Denmark; and Lauri Vuorela and Arturii Similä from Finland.

When Rossby arrived in Sweden in the autumn of 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 1946–7 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 for the rest of his life.

The presence of Rossby and his foreign visitors at SMHI could not but have an impact on the Swedish meteorologists. The daily weather briefings, with hemispheric surface and height maps, took on an extra dimension when they were attended by the world's leading scientists (Berson 1991). Modern dynamical concepts were picked up by the operational forecasters, integrated into their synoptic thinking and applied in fruitful ways in their routine work.[4]

3. New dynamical concepts

Two of the new concepts, the barotropic model and the notion of energy dispersion ('downstream development') would play important roles in the future development of NWP.

3.1. 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, and thus the upper air patterns would be responses to the influence of strong baroclinic systems over the Atlantic to the west. (Jerome Namias in Roads 1986 p. 17)

Rossby's wave equation (Rossby et al. 1939, 1940) shows the relation between the wavelength L, the mean zonal flow $\overline{U}$ and the phase velocity c

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

For short waves the last term is small, $c > 0$ and the waves move eastward. For very long waves the last term dominates, $c < 0$ and the waves move westward,

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

'retrogressively'. There is, for $c = 0$, a corresponding stationary wave length L_0

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

The equation was originally intended to be used on time-averaged fields. Cressman (1948) and others found that it actually worked best on daily upper-air patterns. The main idea in Rossby's wave concept was that if there were a discrepancy between the actual wavelength and the stationary one, there would be an adjustment towards the stationary. It was assumed *a priori* that semi-permanent 'anchor troughs' over the western parts of the oceans should remain in their positions due to the diabatic forcing of strong SST gradients.[5] This would allow the positions of the downstream non-permanent troughs to be calculated.

Rossby's principal synoptic expert, Jerome Namias, visited SMHI for six months in 1949. He had hardly arrived when the Easter Holidays (14–18 April) began and the media requested a forecast from the SMHI. The management was reluctant go beyond the normal two-day outlook, and passed the buck to the newly arrived 'American expert'. The weather was unsettled with a strong south-westerly 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 of a fine and sunny Easter was given to the press and shortly verified. (Roads, 1986)

This successful prediction immediately established the barotropic concept among meteorologists in Sweden. It was shown to be more than just a 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 made the point earlier 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: 1, 13). The long planetary waves were created by all kinds 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 being the ability to describe what was to become known as 'downstream development'.

3.2. 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: 1080–9)[6]

Rossby's wave equation indicated that shorter waves moved faster than longer waves, that they were dispersive. In the summer of 1944 Rossby, while 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 (Rossby 1945). 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 travelled faster than the wave. This implied that new waves could be formed downstream, generated by the energy arriving from upstream. Meteorologists 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 1945: Namias-Clapp 1944: Cressman 1949).

In Sweden Rossby was told that Scandinavian forecasters had also noted a similar process, for example the strong anticyclones that frequently formed over the British Isles downstream from an intense storm development over the North Atlantic. Evjen (1937), a Norwegian forecaster, explained this as a result of huge quantities of air being 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 Hovmöller (1949, see also Rossby 1949a) constructed his famous 'Hovmöller Diagram': a time-longitude matrix of daily 500 hPa geopotential heights averaged between 30°N 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.

[5] The retrogression rarely, if ever, involved individual troughs moving westward. Instead the retrogression developed when an original trough weakened and moved eastward, and was quickly replacred by a new trough, arriving from upstream which deepened in a more westerly position.

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

[7] See Frædrich & Lutz (1987) for a way of measuring the group velocity using Hovmöller diagrams.

In 1948–9 the mechanism of this energy dispersion process was thoroughly investigated and discussed (see Persson 2000 for an extensive biography). 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 though the understanding of the physical mechanism behind 'downstream development' was vague among Swedish forecasters, they realised that the barotropic concept was not trivial. They knew from experience that it worked, so anything derived from it would probably work as well.

3.3. 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. 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. For the NWP pioneers this was a crucial issue. Owing 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, 1949a, 1949b) 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?' (Phillips 1990).

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 Rossby (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 40° per day.

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

4. Preparations for NWP

To pave the way for NWP many scientific, technical and political preparations had to be made, in close contact with the international meteorological community.

4.1. The start of international cooperation, 1949–51

> *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 that Rossby undertook after his return to Sweden was the establishment of a new international journal *Tellus*.[10] Before the war there had been two leading theoretical journals in Europe: the *Quarterly Journal of the Royal Meteorological Society* (*QJRMS*) and *Zeitschrift für Meteorologie* (*Z. Meteorol.*). The war had left *QJRMS* dominating the scene.[11] Just a few years earlier, Rossby

[8] Phillips (1990: p. 66), 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 amplification of disturbances downstream will take place in the wake of the zonal trajectory.'

[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).

had started the *Journal of Meteorology* (*J. Meteorol.*) in the USA.

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

Two years later Rossby went 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 post-war 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 similar in 1923. According to the French Director, André Viaut, there were plans to erect such an international institute just outside Paris. The negative British attitude cannot only be explained by their general foreign policy at the time.

Another obstacle might have been a certain reluctance towards Rossby personally. He 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. (UGGI 1951: 27–28)

Not only the British delegation but also others in the traditionally conservative meteorological establishment might have been reluctant to have Rossby at large in the heart of Europe.[14]

4.2. 1951: Discussions in the aftermath of ENIAC 1950

> *George Platzman*: Do you think, looking back, that the practical usefulness of the barotropic equation was generally underestimated?
>
> *Jule Charney*: I think so indeed. And, of course, I think we were all rather surprised that the predictions were as good as they were. (Platzman interviewing Charney, cited in Linzen et al. 1989 p. 49)

It was in the November 1950 issue of *Tellus* that the results of the successful ENIAC experiment (Charney et al. 1950; Platzman 1979) were published and reached readers in the spring of 1951 (March in Europe, May in the USA). 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 with work on baroclinic models, based on 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 stick with the barotropic model and explore its potential further.

Although the developments in Sweden and the USA took different paths they would interact and mutually benefit 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. Soon leading European and American scientists were invited to join. An arrangement was made with the US Air Force Geophysics Research Directorate (GRD) in Cambridge, Massachusetts. Several US Air Force officers visited the Institute of Meteorology at the University of Stockholm (MISU) for one or two years.[16] Several organisations in the USA established semi-permanent positions in Stockholm, which were changed annually, or every

[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 relationship with the German Hans Ertel, neither during the Nazi epoch or when Ertel sided with DDR. He also had good relations with Soviet scientists 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!

[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 gave Charney 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.

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 MISU.

In 1951 Bert Bolin (Taba 1988b) spent six months at the Institute for Advanced Study in Princeton. There he finalised his work on the effect of mountains on atmospheric flow. Together with Charney he manually calculated 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 × 15 grid points. The manual calculations took four to five hours for each case. The results were positive, the calculated tendencies had a correlation of 0.74 or 0.77 against observed tendencies, depending on the routine or charts used.

In Bolin's absence in autumn 1951 the group at MISU reviewed some 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 to be one of the most inspiring people at the institute.

4.3. 1951: The choice of NWP model

> Purely barotropic processes define the stage upon which the thermal play is performed. (Bolin 1951)

It was when Bolin returned in the autumn of 1951 that the work on the Swedish NWP really started.[17] On his return Bolin gave about 25 lectures, not only at MISU, but also at SMHI, the Air Force and elsewhere. In an internal memo he provided his arguments for restricting the work to the barotropic model. Instead of starting from the general atmospheric equations and simplifying them *mathematically* in order to make them numerically solvable, 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, he suggested, was a thin two-dimensional incompressible fluid of constant temperature and density, 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 described in principle as the conservation of *potential vorticity*.[19]

But the model that formed the basis of the 1950 ENIAC forecasts, the 1952 tendency calculations and the subsequent Swedish NWP system was based on the conservation of *absolute vorticity*. Physically it meant that an upper lid constrained 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 would remain so. The speed, however, varied linearly from the bottom of the troposphere ($p = p_0$) to the 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. (fig. 2) 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 this equivalent barotropic model came from both theoretical and practical considerations. Limitations on 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. Added to this was the limited capacity of electronic computers.

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

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

18 Bolin, undated memo, probably from 1951. This was in line with the 'Rossby School's' general physical approach to the meteorological sciences (McIntyre 1951).

19 Bolin & Charney (1951) invoked G. I. Taylor's classical experiment from 1921, which demonstrated that the relative motion in a rotating fluid becomes 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 a long time. Rossby and some students at the institute had developed graphical NWP based on this concept (Rossby et al. 1940). Charney's derivation was a justification for an already trusted concept.

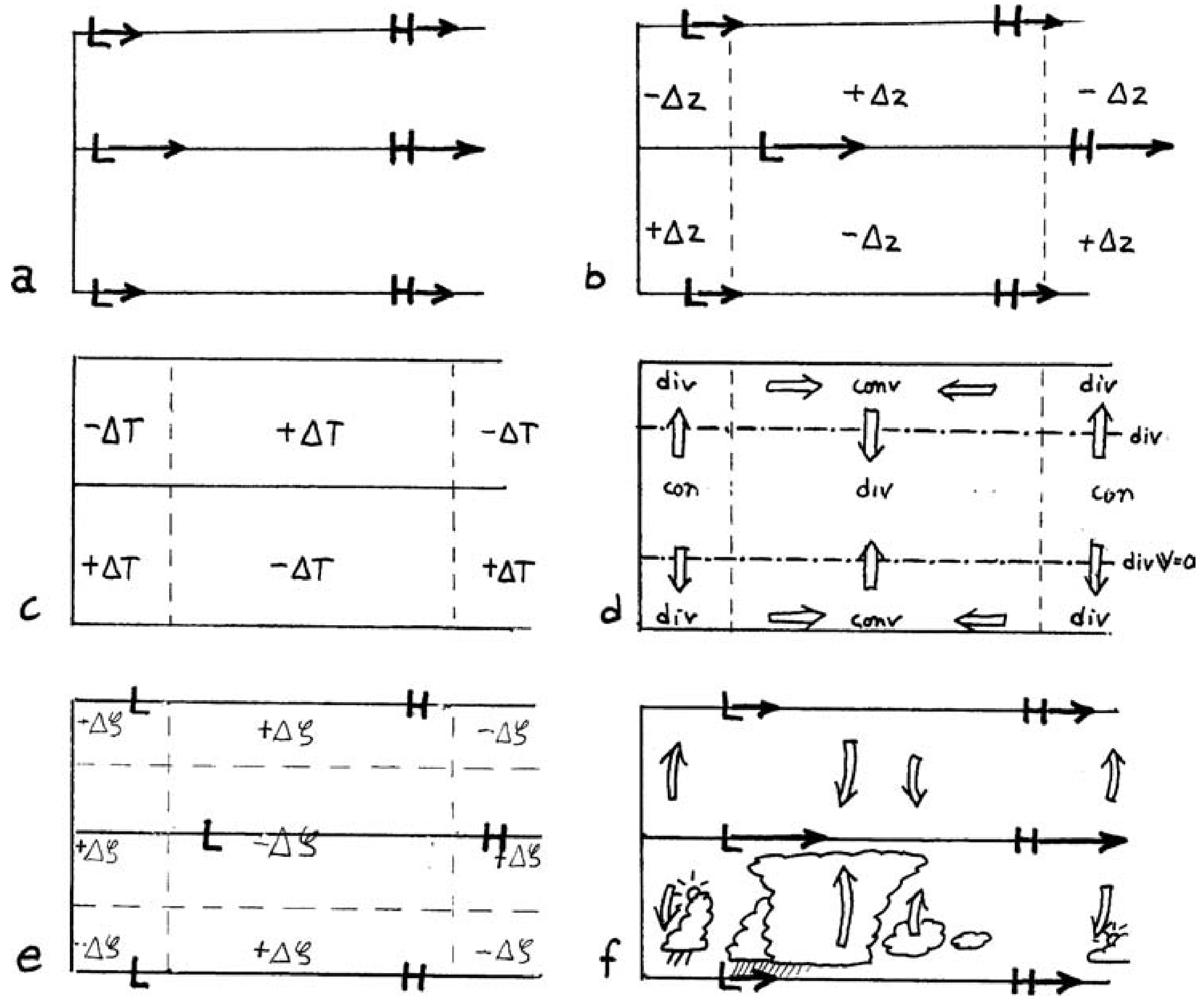

Figure 2. *The equivalent barotropic model – a qualitative explanation.*
In an equivalent barotropic model the wind direction is constant with height, but the speed varies vertically (a). During a short moment of time the flow patterns is advected downstream, most rapidly in the jet level, typically 200–300 hPa. Assuming geostrophy 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) thermal changes can be accomplished only by vertical motion in a stable stratification (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, typically 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.

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

> 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).

5. From hand to computer calculations

In the early 1950's the Swedes developed what would in a brief interlude be the most powerful computer in the world-just in time for Rossby's group to use it for their NWP.

5.1. 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 devices, which belonged to technicians from Chalmers Institute of Technology. One of the technicians, Stig Ekelöf, had just returned from a fact-finding mission to the USA where he had studied advanced technical laboratories on behalf of the Swedish military.[21] During and after the war it became known that fast computing machines were under construction at various institutions in the USA and there was great interest in Sweden, particularly in military circles, to acquire a computer system.

Ekelöf's loss, however, was to some extent compensated by the novelties that he had been exposed to during 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). The head of its decoding department was Åke Rossby, younger brother to Carl Gustaf.[22] Both brothers were present at a meeting with the Swedish Board of Computer Machinery (SBCM) on 4 October 1947 when the possible use of 'mathematics machines' was discussed and the application in meteorology was mentioned.

Parliament granted money and in May 1947 five young experts were sent to the USA, 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 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 on account of their 'highly strategic significance'. This was part of a general policy, and not directed only at Sweden.

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

The first computer to be constructed by SBCM, BARK (Binary Automatic Relay Calculator), 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 1800 hours to perform manually. In 1951–2, BARK was heavily in use, mainly for military purposes, but also for research and industrial applications.

The second and improved Swedish computer, financed by SBCM, was BESK (Binary Electronic Sequence Calculator), which became operational in 1953. The construction was influenced by John von Neumann's Princeton machine. Initially it had an internal memory of 25,640 binary digital numbers, but its capacity was expected to increase later with a magnetic drum memory. Compared with 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 × 20 grid points with a 300-km grid 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).

5.2. 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 in an interview in *Dagens Nyheter*, 17 April 1952)

A truly international group had now been 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 good training to repeat Bolin and Charney's tendency computations, but this time over Europe and eastern Atlantic. The tendency calculations were cumbersome.

[21] Dahlquist and Fröberg, 1971, Carlsson, 2000. The last time Ekelöf 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, and thus the Swedish military intelligence, informed about American progress in 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 to FRA in autumn 1943 through other channels than from Boris Hagelin [a Swedish manufacturer of decoding machines] and C.G. Rossby. Both were then in the USA 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 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 in fact it was a state-owned technological enterprise.

While the calculations of the advection term, expressed as a Jacobian, are straightforward, the solution of the final Poisson equation by an iterative procedure, to provide the tendency in the geopotential field, is time consuming and require energy, patience and ingenuity.[24]

In March 1952 they had completed 14 cases of 12-hourly tendency calculations from November to December 1951. The grid length was 310 km at 50°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 correlation of 0.81 (Staff Members 1952)

In a newspaper interview Rossby could confidently present ‘a new meteorological method’ to make more accurate and ‘some time in the future’ also more extended forecasts (*Dagens Nyheter* 17 April 1952). The new method excluded the element of subjective calculations (‘guesswork, but guesswork based on long experience’) that the current meteorological prediction of the upper-air flow demands. He also outlined his plan for the coming two to three 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 of 1952–3 a numerical program would be developed for running on BESK.
(3) In the spring of 1953 experimental runs would start on BESK.
(4) Operational forecasting by the ‘official weather service’ would probably start in 1954.

As it turned 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 12 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 & Newton 1952).

A second conference, devoted to NWP, cloud physics and atmospheric chemistry, was held 13–18 October 1952 in Stockholm (Rossby 1952; *Tellus* 1952). Among the attendees were Charney, Lingelbach, Arnason (1953), Eady, van Mieghem and Sutcliffe. (figs 3 and 4) According to a newspaper report it came as a surprise to the audience that Britain had computers available for meteorological forecasts (*Dagens Nyheter* 14 October 1952).

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

5.3. 1953: pre-operational runs on BESK

Although the first steam boats were not particularly fast, the invention of the steam engine implied

Figure 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 the University of Chicago (Vincent Oliver, personal communication). This picture is taken at the October 1952 conference in Stockholm.*

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

[25] The average of 0.69 seems to be a mistake. A recalculation 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 recalculation yields 51 and 52 m.

NY MODELL FÖR VÄDERPROGNOS

En del av det nya meteorologiska institutets forskarstab samlad över en väderkarta med en intressant "insnörning", vars framtidsutsikter tydligen förtjänar diskuteras: fr. v. William Hubert, Washington, dr Lauri Vuorela, Helsingfors, fröken Christa Steyer, Hamburg, institutionschefen professor Rossby, dr Eric Eady, London, och professor Jacques van Mieghem, Bryssel.

Figure 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'.*

> possibilities which eventually would be possible to utilise. Furthermore, the sailing ships, in competition with the steamboats, made progress, not imagined beforehand. Allegorically, the mathematical machine is our steam engine. (Rossby in an interview in SvD 30 December 1953 and to Pierre Welander, personal communication 1994).

During 1953, while waiting for BESK to become operational, the group put the barotropic model into a numerical code. The key figures in this undertaking were Germund Dahlqvist and Norman A. Phillips. Dahlqvist was in his late 20s and got involved in 1952 when he worked at the Swedish Computer Board (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, and thereafter was 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 set about advising them to use less complicated and more stable numerical schemes. As his colleague Dahlqvist observed:

> 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 to 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

[26] Phillips' grandparents had all come from different parts of Sweden during the second half of the nineteenth century: Victor Pettersson (Öland) and Anna Hjertstedt (Östergötland) had the son Alton; Fritz Larsson (Dalsland) and Eva-Lena Salmén (Västergötland) had a daughter, Linnea Marie Larsson. Alton Pettersson was a carpenter in Chicago during the 1920s, 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.' Fifty years later, Norman's Swedish is still very good.

[27] Phil Thompson reported in summer 1953 that the Europeans were six months behind in theory and one to two 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' (Harper 2003). Perhaps it was because he visited Sweden before the BESK became operational that his report was so far off the mark.

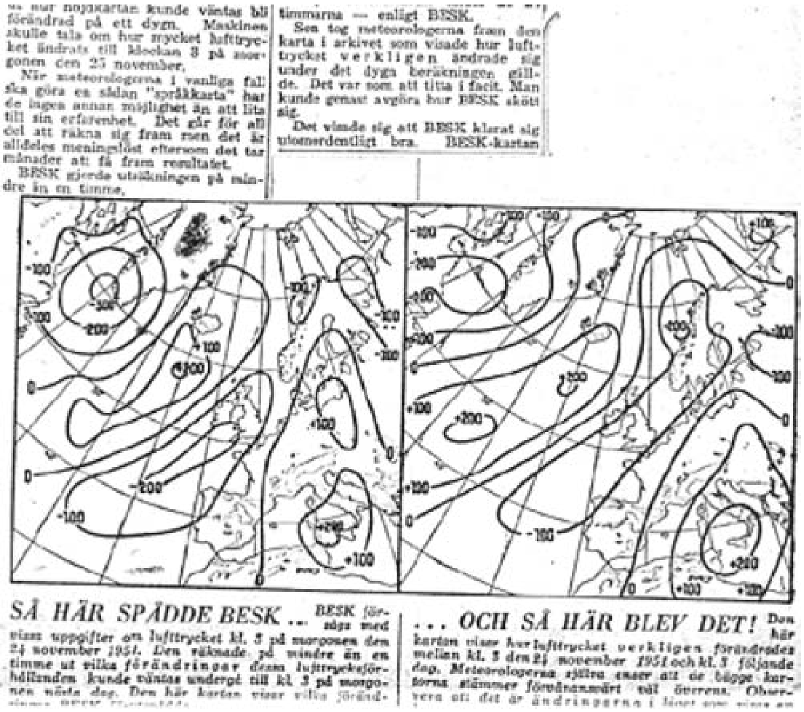

Figure 5. *The first 12-hour tendency calculations on BESK grabbed the headlines of the popular press. The daily tabloid* Expressen *even published the 24-hour tendency maps well ahead of* Tellus*. The caption reads: 'BESK prophesised like this . . . and it turned out like this.'*

(JNWPU) was founded with George Cressman as Director (Harper 2003).

BESK became operational in December 1953. The opening was dominated by meteorological applications. One newspaper, the tabloid *Expressen*, ambitiously featured a map of Europe with the forecast and observed 24-hour changes of the 500 hPa geopotential field (Figure 5). 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 (non-real time) forecasts were completed before Christmas 1953. The group sent Rossby a wire to Capri to tell him that this milestone had been passed. Back in Sweden he told the press enthusiastically:

> 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. (SvD 30 December 1953)

Phillips left in January 1954 and spent February in Norway. He was replaced by Bo Döös (Taba 1997). The leader of the BESK runs was Major Harold (Art) Bedient, US Air Force, who arrived in Sweden in the autumn of 1953. He learned about the practicalities of NWP through self-study and manual calculations. It was Bedient who, together with Dahlqvist, developed the 'zebra plots' of output fields.[28] They would remain at SMHI until the late 1970s.

A second milestone was passed in two runs on 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' (Wiin-Nielsen 1991)

6. The start of operational NWP in 1954

A major military manoeuvre in 1954 provided the rationale, sponsor and background for the first real-time NWP.

6.1. Preparations for operational runs

> I am sure it is of great importance to inform just the man in the street about these [NWP] 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! (Páll Bergthorsson in a letter to Rossby 11 August 1954; cited in Harper 2003)

The BESK calculations of 24-hour barotropic forecasts were summarised in a report (Staff Members 1954) that was sent to *Tellus* in May 1954 by a group consisting of

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

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 North 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.[29] 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 30 m
Analysis of 500 mb map	1h 30 m
Reading values in grid points	1h 20 m
Punching of input data	40 m
Checking and correction	20 m
Machine tine for forecast	40 m
Plotting and analysis	30 m
Total time	6 h 30 m

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 × 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!)' (Staff Members 1954). BESK was powerful but expensive. To run for an hour cost almost the same as the monthly salary for a civil servant. So who would be willing to pay?

6.2. SMHI versus MVC

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

At this time Rossby had no financial support from SMHI, and there was no immediate prospect of finding the money that would be needed (Alf Nyberg to Taba 1984). Indeed, it seemed SMHI was not yet totally convinced that the project was realistic.[30] The official line at SMHI was that basic research was the task of the universities whereas SMHI's role was practical implementation. Nyberg told the press that 'for the time being' they 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 Erik Palmén and the SMHI Director (since 1949) Anders Ångström.

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 ranking position). But Herrlin was no fool and was an ambitious man. He had had few opportunities to grasp the complexities of dynamic meteorology and numerical mathematics, but he felt that the progress made in the technique of 1–2 days forecasting 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.

As shown by the experience of larger countries neither increased personnel nor funding could improve the weather forecasts along conventional lines to any significant extent. It was in this pessimistic mood that Herrlin was approached by Rossby in the winter of 1953–4. 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

[29] 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 at a rate of 30° per day or 25-30 m/s.

[30] 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 in ten worked out. At this time (1954) Rossby also had a campaign for cloud seeding and weather modification.

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.[31]

Compared with the overall military budget, the cost of a numerical prediction programme for the Weather Service was not too significant.[32] Herrlin had no difficulty in convincing the newly appointed commander of the Swedish Royal Air Force, Axel Ljungdahl,[33] that the project was desirable, even necessary.

In March 1954 Bert Bolin made arrangements with the US Air Force.[34] 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 was to take place early in the autumn.

6.3. 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 the largest military manoeuvre since 1944 (code-named 'Dalamanövern'), took place in central Sweden involving some 45,000 soldiers. Importantly, protection against nuclear warfare was to be tested for the first time and accurate forecasts of upper air movements were therefore of crucial importance. During the summer of 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 government 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, who would replace Anders Ångström on his retirement. 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 thus drawn into a political struggle between MVC and SMHI.

The outcome of the operational trial was a splendid success, operationally, scientifically and politically figure 6. 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 military manoeuvres, the Swedish government reached its decision and appointed Alf Nyberg as the new Director of SMHI. With the political air now cleared, the successful autumn test paved the way for operational NWP activity in December.

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

He then described how the NWP was produced. The finale is somewhat full of pride:

> This project has attracted much international interest. In particular American press has 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 . . . . (Herrlin 1954)

Considering all approximations the future for NWP appeared to Herrlin as rather 'hazy' and the road ahead 'bumpy and full of twists and turns'. But he was confident that during the winter they would, with a military metaphore, 'reach the first hill, and then 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,[35] the next

[31] The alliance between Rossby and Herrlin was probably an important factor in getting the project under way. Rossby was eager to put his theories to the test and Herrlin realised 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).

[32] Sweden was not a member of NATO but still had one of the world's most powerful air forces (after the USA, USSR and Israel).

[33] It is not clear when the Air Force was contacted. Axel Ljungdahl is mentioned as the man who gave Herrlin the green light, but Ljungdahl took over from Bengt Nordensköld, the previous commander, as late as 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 modernise SMHI (Nyberg, personal communication. See also Rossby's letter to Dahlquist 21 March 1954 in Appendix 1).

[34] 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. In August 1954 Brigadier General Th. Moorman, USAF, visited Rossby's institution.

[35] Herrlin alludes to the investigation by Leverrier in 1855, ordered by the French government, after the British and French navies had

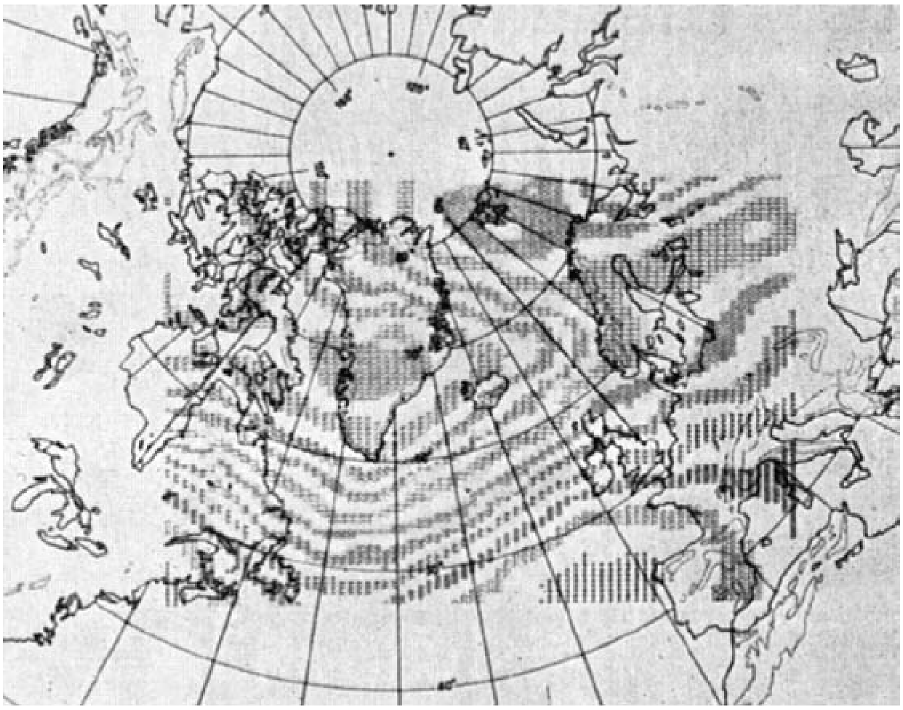

Figure 6. *An authentic 'zebra plot' from the autumn 1954 operational BESK forecasts. Photo by Guy Dady in his article in La Météorologie some months later (Dady 1955). The caption text declared that the picture showed the computational area, whereas it only depicted the verification area.*

> will be taken by the Swedish Air Force – and that is a great pleasure.

The mood was different at SMHI. Ernest Hovmöller wrote to his friend George Platzman (19 December 1954) about the first NWP runs as 'the great topic among Swedish meteorologists just now':

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

At SMHI the ongoing experiments were 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 Ångström sorted out, SMHI took a positive approach. Alf Nyberg told the press 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 (*Dagens Nyheter* 31 December 1954).

7. Operational runs 1955

During the operational runs in 1955 the system was improved on. An automatic analysis scheme was put in operation and an instruction in the use of NWP was worked out to be used by the forecasters.

7.1. Controversy over verifications

> When performing the differents tests during the course of the compuation, try to make quick descisions, since every minute's delay costs four Swedish crowns. (Instruction to the staff operating the BESK forecasts)

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

A second operational period started on 17 January and ended on 25 February 1955, with SMHI having now joined the experiment. Bert Bolin lectured on BESK for SMHI staff, who were invited to come and visit it (W. Persson 1955). Bo Döös patiently accepted all visits, his only apparent worry being that BESK would stop every so often, even when there were 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 would surreptitiously start the typewriter so that it would print online from the tape the reassuring words *COFFEE BREAK*. The group, duly impressed by this 'complete automation', went to have their coffee, later escorted back to a system 'still' in full operation, unaware of the frantic fixing efforts that had been going on in the meantime.

A report was issued in spring 1955 by P. Bergthorsson (Iceland), O. Haug (Norway) and B. Döös, S. Fryklund

been destroyed by a vigorous storm in the Black Sea in November 1854 during the Crimean War.

and R. Lindquist (Sweden). 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 authors also subjectively evaluated the charts. They labelled the forecasts from 1 (bad) to 5 (good). The subjective scoring also favoured the NWP with 4.2 and 3.7 for NWP against 3.8 and 3.0 for the manual (Bergthorsson et al, 1955).

A third operational period started on 12 April. The weather was cold and spring refused to come. One day, when Germund Dahlquist was standing in front of BESK observing how the machine printed a 'zebra plot', an Israeli visiting scientist, E. Charrash, came up to him and asked: '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 and replied: '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 into a true believer in NWP. 'Such cases of unexpected success contributed surely to make NWP popular', remembers Dahlqvist (personal communication 1992; see Figure 7).

On 25 May the experiment was terminated. By now the report of the two first periods had been translated into English and sent to *Tellus*, probably by Rossby. It was done in some haste because the authors were not notified (S. Fryklund, personal communication 1992).

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-h forecasts from 9 and 13 December had scored 0.00 and 0.06, whereas the +48-h 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 (Nyberg, personal communication 1992).

A fourth operational test period began at the end of October 1955 and ended at the beginning of May 1956. Some changes were made in the numerical forecasting procedure, the most important being the introduction of an objective (numerical) analysis of the 500-mb chart formulated by Pal Bergthorsson and Bo Döös.

a

b

c

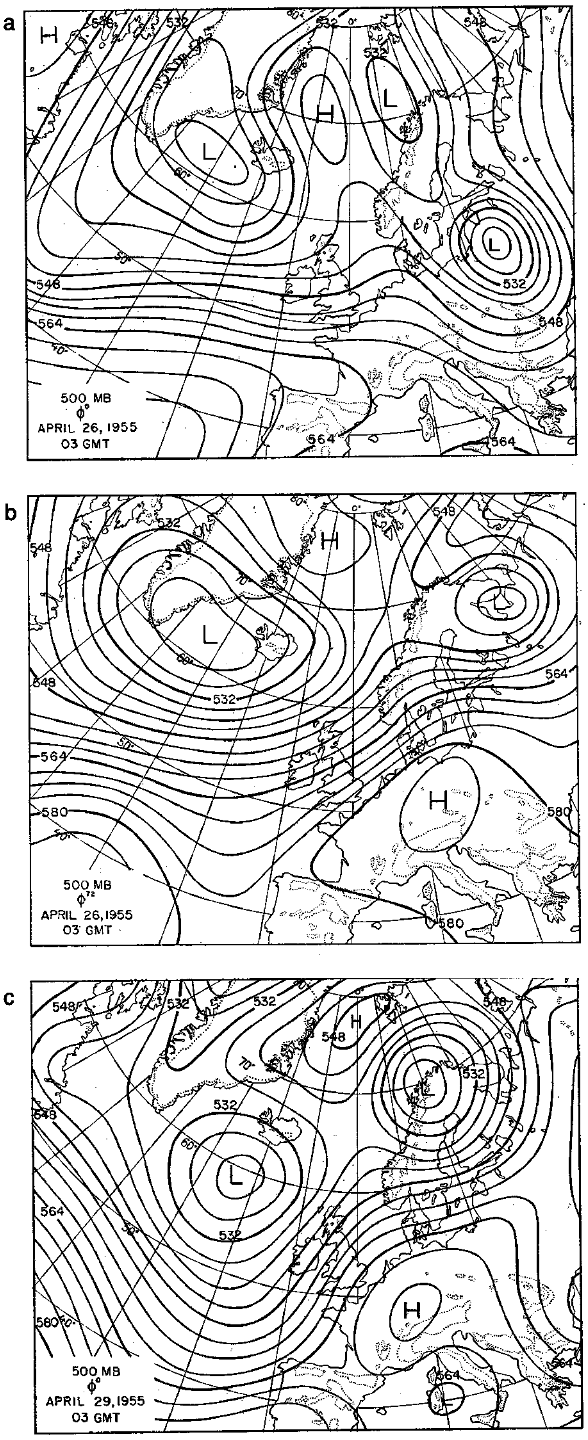

Figure 7. *The BESK forecast that foretold the coming of spring at the end of the chilly month of April in 1955. From a situation with a predominantly zonal flow on 26 April (upper chart), the simple barotropic model predicted a +72-hour change to high pressure over central Europe and mild winds over Scandinavia (middle chart), which almost perfectly verified three days later on 29 April (lower chart). The maps were published in Bergthorsson et al. (1955) and Bushby (1986). (a) Observed 500 mb contours on April 26, 1955, 0300 GMT. The height are given in decameter as unit. (b) 72-hour forecast of 500 mb from map shown in a. (c) Observed contours on April 29, 1955, 0300 GMT.*

7.2. New controversy – automated analysis

> 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 that professionals were so proud of could be simulated by a computer. (Bengt Söderberg in Bushby 1986)

It had now become quite apparent that the manual analysis was too time consuming to be an effective operational activity. Each day a subjective analysis had to be made of the circumpolar 500 mb 03 UTC. It was completed twelve hours late at about 12 or13 UTC. The geopotential height data at the 31 × 42 grid points were read and punched. The computation on BESK began at 14 UTC and generally took some 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 would take place between 17 and 18 UTC. This numerical work required daily shifts of three or four meteorologists and the same number of assistants.

Bergthorsson and Döös' new idea (1955, see also Best 1955 and Döös 1956) 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. Dahlqvist regarded the idea of a 'first guess' as a synthesis of different approaches (personal comunication 1992). The 'butterfly problem', i.e. small analysis errors amplifying during the forecasts, was quickly identified and discussed (Best 1956; Berggren 1957; Thompson 1957), with the first to bring up the issue probably being C. W. Newton (1954).

The introduction of objective analyses made it possible to save both time and labour. The number of people occupied on a daily basis with numerical forecasting during these periods was reduced to one meteorologist and two or three assistants.

Another operational alteration was that the forecasts were based on the 15 UTC analysis. The work was now done during the night and the forecast charts transmitted in the morning. They therefore offered the most current data available at that time. However, there was an interval every weekend caused by a 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. 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 more 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 themselves with the weather, creating an inner picture of the synoptic situation.

There was also the suggestion from a Swedish meteorologist, Duvedal (1961), employed by the Scandinavian airline SAS, that manual analyses should be relied upon entirely. Based on his own experience, Duvedal argued that the conventional analysis could start before all observations had arrived and be adjusted as they came in.

In the autumn of 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.

7.3. The 'User Guide'

> The forecasts should thus be accepted in the form they have been prrsented by the machine, except when e.g. fictitious anticyclones are built up or real anticyclones amplify in an unnatural way. (from the 1955 'User Guide')

Each day (in the morning) a so-called 'Commentary to the NWP' was issued on the forecast charts. It was an attempt to interpret or 'translate' the forecast charts into terms of weather to be expected over 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.

In cases where the numerical output agreed with what could be expected from experience and intuition, it would be welcomed by the forecaster because it increased his confidence. But where the NWP offered a different view, a problem arose. Whereas it was always possible to discuss such differences with colleagues, the NWP output would lie on the table or hang on the wall in mute silence. The Swedes set out to tackle this problem by putting together a 'User Guide' for the forecasters.

Since 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 indicated that a change of weather type was to be expected, this was to be treated particularly thoroughly in the commentary. Changes in large-scale flow were particularly difficult to forecast by conventional methods.

It was one of the advantages brought about by NWP that improvements 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. If a well-developed jet was not present it was realistic to assume that the main fronts were also 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 à la Bergen School. The forecaster could distinguish between two different stages of development:

> If the wave is not seen in the 500 mb flow, a baroclinic development is probably under way. The intensification of the wave may 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 the area within which 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 only two cases:

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

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

When the anonymous author of the 'Guide' was drawing to a close he seems to have realised that he had not given the forecasters as much guidance as they needed. So he ends by giving them something of a free hand:

> What is said above is, as the title 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 over the intervening 50 years.

8. MVC versus MHI

Although to military pushed ahead with the further development of NWP it was just a matter of time before the civilian side would catch up.

8.1. The MVC takes the lead

> For the last ten years or so, the progress made in the technique of 1–2 day forecasts has been very small. The development appears to have become almost stagnant. I believe we have literally squeezed the conventional techniques dry ... Under these circumstances it is not very encouraging to be the spokesman for meteorology within the armed forces. (Oscar Herrlin 1956)

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 towards cloud physics, artificial rain, atmospheric chemistry and global temperature changes.[36] During the coming years the NWP would pass into the hands of the two weather forecast institutes MVC and SMHI.

At the biannual 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 the responsibility of the MVC, but SMHI's involvement increased. In January–June 1956 SMHI made 28 routine computations twice a week on BESK and on a new FACIT-EDB machine.

The arrival of numerical forecasts required changes in staffing and need for more resources. A new commission looked into the matter. In contrast to the military, which argued along the lines of cheaper and quicker forecast production, SMHI chose to emphasise improvement in *forecast quality*. While waiting for the commission to report, the NWP operated according to bare necessity. The annual report for 1957 indicated that forecasts were run only in connection with the Easter, Christmas and New Year 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 cyclogenesis over the US east coast. During 1958 and 1960 SMHI continued to run barotropic forecasts on an irregular basis with forecasts up to

[36] MISU maintained an active interest in dynamic meteorology in general and numerical weather predition in particular. Phil Thompson's many visits resulted in papers in *Tellus*, and his textbook on NWP (Thompson 1961) was partly written in Stockholm. In 1964–5 another American meteorologist, James R. Holton, used his stay to prepare a new textbook on dynamic meteorology (Holton, personal communication 1995).

+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 was convinced that NWP was the modest beginning of 'a new era' within the forecasting technique, an era comparable to that 'created by the Norwegian school in the 1920s'.

> Developments within [weather] forecasting have, as I see it, and also many others, more or less got stuck... During the last few years I have also come to the conclusion that a further increase in accuracy simply is impossible with conventional methods – in any case not significantly and at reasonable cost .... We hope now that the numerical methods, which also have the advantage of starting from a good initial state, will be able to lead us out of the cul-de-sac we have ended up in. Herrlin, 1956

Herrlin 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 and part-time at the University of Ragnar Fjörtoft before he returned to Norway in 1955 (Wiin Nielsen 1997: 50). While Döös worked on problems relating to analysis, Wiin-Nielsen worked with Bengt Söderberg at MVC to develop a two-parameter model.[37] In December 1958 Wiin-Nielsen left Sweden for the USA to join the JNWPU.[38]

8.2. The first baroclinic model

> The Air Force automates the whole weather service (headline in *Dagens Nyheter* 30 April 1959)

Aksel Wiin-Nielsen's two-parameter model consisted of a 600 hPa level, advecting a 400–800 hPa thickness, with no feedback from the thermal field to the advective level. The grid size was 300 km, the time step 45 minutes, and 23 × 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 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 with the conventional forecasts the automatic NWP was 'superior'. Since the analysis for the computations 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 comparable quality to the subjective forecasts.

After the experimental 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 the 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 realised that the party had not been such a good idea after all. Then someone showed him the *Christian* daily, which had a comprehensive and correct account. This was most surprising since that particular journalist had been one of the drunkest!

But the plans for quick automation had to wait. It would take some years before the two-parameter model was implemented. In 1960 Söderberg ran more tests, now with 500 hPa as the advective level and 850–300 hPa thickness, 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). But in October 1962, with the arrival of a new computer, the IBM 7090-1401, the two-parameter model at last became operational. The area was extended to 47 × 47 grid points for the analysis and 44 × 33 for the forecasts. The centre of the area was over Greenland, with 500 hPa advecting the 300–700 hPa thickness.

This model ran at the MVC with only small changes for 11 years until 1973, when SMHI took over full responsibility for all NWP production in Sweden.

The military meteorologists had realised in about 1960 that if they insisted on being at the forefront of NWP in Sweden, it would increase the 'risk' (as they saw it) of the state authorities merging them again with SMHI (Söderberg, personal communication).

8.3. SMHI takes the initiative

> SMHI aims primarily not to improve the weather forecasts, but, given the current lack of meteorologists, to an increasing automatisation. (Bo Döös at the Nordic Directors' Meeting in Stockholm 1960)

During the late 1950s the Swedish government had finally realised the potential of NWP and given SMHI

[37] There was also an earlier attempt by Duvedal (1954).
[38] He would remain there until 1974 when he moved to England to lead the creation of ECMWF.

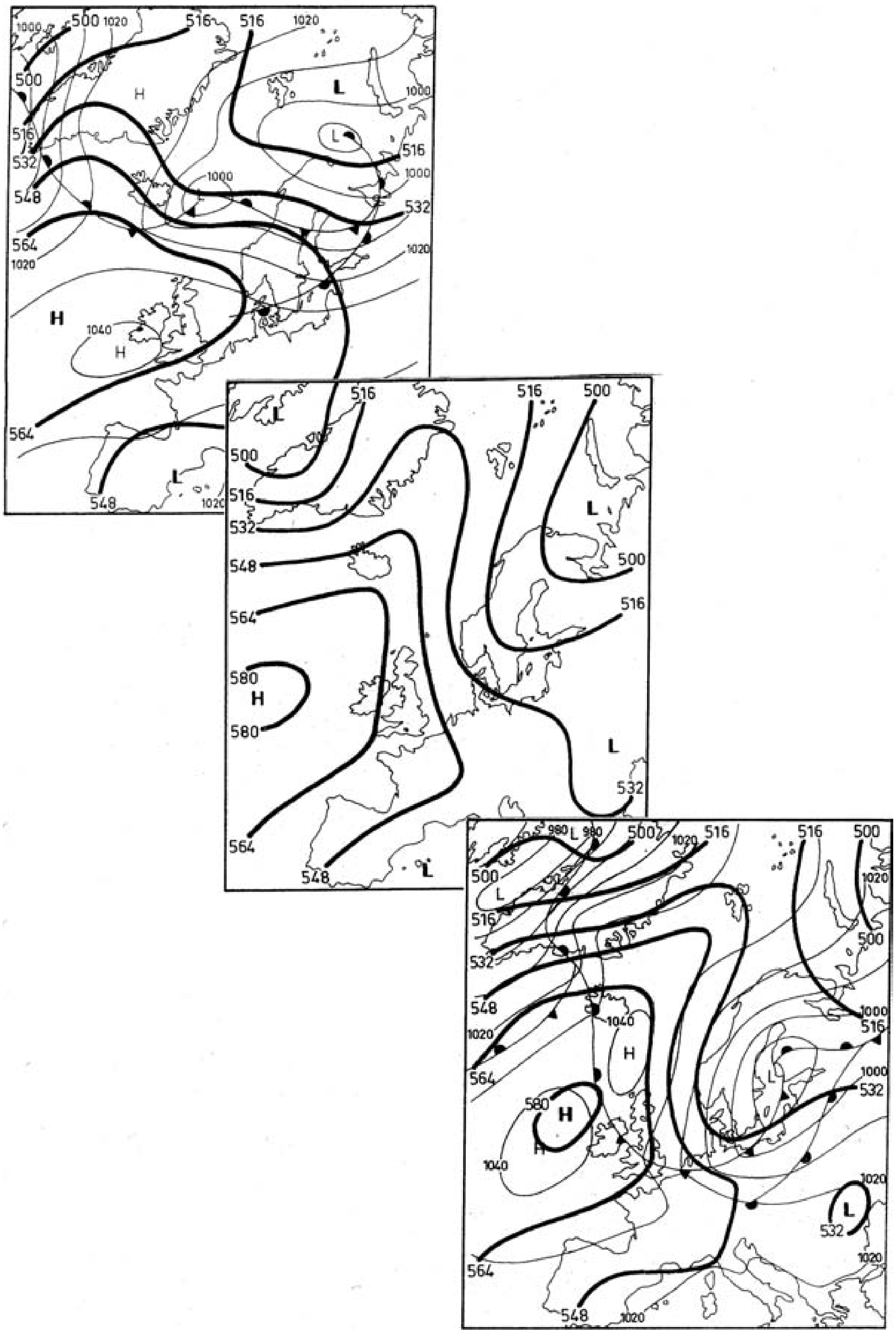

Figure 8. *In January 1954 a devastating north-easterly storm brought destruction to eastern Sweden. It had been poorly forecast. A +24-hour forecast re-run on BESK, initiated by Karl-Einar Karlssson, one of the progressive meteorologists in the aerological section, showed that a NWP system operational at the time could have provided crucial guidance.*
The upper chart depicts the 500 hPa flow 2 January 1954 00 UTC; the middle chart the +24 hour barotropic forecast; and the bottom chart the 3 January 1954 verifying analysis. The MSLP fields have been added for reference and were not part of the forecast calculations.

increased resources. In December 1960, after having visited the USA and attended the NWP symposium in Tokyo, Bo R. Döös was employed by the SMHI together with a Norwegian meteorologist, Thomas Thompson. Lennart Bengtsson, then a young meteorologist at Uppsala University, assistant to Professor Tor Bergeron, was recruited in spring 1961. Routine computations of 500 hPa barotropic forecasts on a 300 km grid were run up to +48 hours, later +60 hours (Figure 8). The machines were BESK and FACIT EDB at the Scandinavian Electricity Plant.

Their task was to set up a comprehensive system for NWP at SMHI, including automation of the many routine activities of the weather service. The small group under Döös now numbered six, and they were engaed in a very exciting and challenging task. It was brand new work, essentially starting from scratch. Since BESK had virtually no software they had to develop their own programming language, MAC (Meteorological Auto Code), and their compiler. All programs were written in machine language using an octal code.[39]

Verifications indicated that the NWP was 'on average clearly better' than the conventional methods. 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 two-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 two-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 two-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 redistribution of divergence during 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 two-parameter model operationally. This became possible only in 1965 when SMHI acquired its first own computer, SAAB D21.[40] 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 issuing *five-day* weather forecasts, twice a week, on Monday and Thursdays, to the press, radio and TV stations. This is a practice that continues to this day.

A three-parameter model was then already under way at SMHI. Work had already started by 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 operations chief), still remembers when he delivered the first three-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!'[41]

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 would eventually 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.

8.4. Changing conditions

> Sweden was in a fortunate situation. The economy was good and development rapid. There was a strong feeling of optimism about the future and the environment was conducive: schooling was free and the libraries and other facilities were outstanding. (Lennart Bengtsson in the *WMO Bulletin* interview July 2004)

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 (Bengtsson & Moen 1969). In 1977–82 a six-level balanced model was in operation. By then, plans for a European meteorological computer centre was under way, what would become the European Centre for Medium-Range Weather Forecasts (ECMWF). There were now prospects for running primitive equation limited-area models with boundary conditions from

[39] Forty years later, in his July 2004 *WMO Bulletin* inteview, Lennart Bengtsson still remembered how to perform basic calculations with eight as a base instead of ten.

[40] 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.

[41] 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).

a global model. Sweden decided to divert much of its resources into this cooperation.

It is no coincidence that three senior figures in the early years of 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 his project.

9. Background to the Study

The motivation for this narrative came in September 1999 at a meteorological conference in Denver, USA. During a break, Eugenia Kalnay, who was writing a book on numerical weather prediction, asked me to contribute a few A4-pages about the history of early NWP. It was intended simply as an aide-memoire, so I was quite flattered when it appeared as an appendix in the final textbook (Kalnay 2003). At the 50-year aniversary of the creation of the Joint Numerical Weather Prediction Unit (JNWPU) in 1954 I was invited to be part of the organisation committe with the special task of writing an historical summary of the early development of NWP outside the USA to be ready by May 2004. The definition of 'early NWP' is vague. It refers to the period 'before 1965 or at least no later than 1970', and deals almost exclusively with non-primitive equation models.

This task aroused a lot of interest and encouragement from colleagues around the world. 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. Up to the very last minute before the deadline expired, new contributions, circumstances and facts arrived by telephone and email.

Already at an early stage I had to abandon ambitions to make a comprehensive report and instead present the findings as they presented themselves. To borrow a metaphor 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' based much more information still lies ahead.

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Appendix 1

Extract of a Letter from Carl Custard Rossby to Germund Dahlquist, 21 March 1954 (translated from Swedish by the author):

As you might already know I am now trying to get a contract with the USAF organisation 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 extended 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. The other day I had a discussion with some of the scientists and department chiefs at GRD [Geophysical Research Directory] concerning the suggestion. Perhaps you and Bolin and Bedient and Döös and Wellander should think a bit about how the program can be realised.

[Inserted in the margin: 'The contract takes care of 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 × 20 points and one hour's time interval, the forecasts become worse and worse the longer out in 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 fail owing 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 for 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 the influence of the initial values must disappear. I would now be extremely grateful if your group could give some consideration to this questions of:

(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 [i.e. drum memory] needed for this purpose?)
(c) how to select preliminary synoptic situations to interpolate the boundary conditions between the observing times
(d) 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 *experimental physics* and *chemistry* and either mathematics or mechanics, The main thing is, however, brightness, interest and initiative. Can you help him by interesting one of your associates at the University? You could use the slogan: *Join the Met Institute and see the World.*