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Early operational Numerical Weather Prediction outside the USA: an historical introduction Part III: Endurance and mathematics – British NWP, 1948–1965

Anders Persson
Swedish Meteorological and Hydrological Institute (SMHI), SE 60176 Norrköping, Sweden
E-mail: Anders.Persson@smhi.se

1. Introduction

> I do not think your views on [the UKMO] in Dunstable will ruffle any feathers of those who worked there. (John Sawyer, personal communication 1993)

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

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

The lack of historical accounts of the UKMO work on NWP might be explained by lack of information. The British civil service has a reputation for secrecy. And since the UKMO belongs to the Minstry of Defence (previously the War Office) we should not be surprised if most of the documents relating to NWP development in Britain were classified. But the opposite is the case. The published material on early British NWP is overwhelming.

From the start, more than fifty years ago, anyone in the world would have been able to follow the work in great detail, almost month by month. The UKMO's monthly *Meteorological Magazine*, the Royal Meteorological Society's *Quarterly Journal* and its popular monthly magazine *Weather*, gave extensive coverage of the NWP work during the 1950s and 1960s. Apart from publishing reviewed papers they also reported on meetings and, uniquely, on the discussions that followed.

And there was much to report on. The British road to operational NWP, 1948–65, was marred by problems and emotions worthy of a BBC drama or a Hollywood blockbuster. Here we find meteorologists with a mixture of admiration for the computer and fears about the future of synoptic forecasting. There are frustrated mathematicians who saw their forecasts getting worse despite improvements to the model. We can, almost verbatim, listen to eminent scientists who put the right questions but got the wrong answers. And finally, the whole drama is imbued with feelings of national pride and independence.

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

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2. The background to British NWP

> [If] charts could be constructed showing the atmospheric conditions prevailing both at sea level and at some suitable fixed altitude above it, and if the nature of atmospheric circulation were at the same time thoroughly understood, the present [worthless]

> empirical method of long-period weather forecasting, would soon become resolved into one involving the scientific process of inference and deduction. (Bonacina 1905)

When the time was 'ripe' for NWP after the Second World War, few countries or meteorological institutes were so well equipped and prepared for NWP as the United Kingdom and its Meteorological Office (UKMO). A number of factors contributed to these advances.

2.1. The growth of the organisation during the war

From 750 employees in 1939 the UKMO had expanded almost tenfold during the war to 6,800 in 1945. The increase brought some consternation to the peacetime organisation and 1946–47 saw a general reorganisation of the whole activity, with greater emphasis to be placed on research. In 1941 the Director, Nelson K. Johnson, had set up the 'Meteorological Research Committee' (MRC). Its purpose was not only to give advice on the general lines of meteorological research, but to assist in such research, discuss reports and make recommendations for further action. Its members, who were paid a small fee, included leading meteorologists such as S. Chapman (chairman), D. Brunt, G. M. B. Dobson, C. Normand, G. I. Taylor, O. G. Sutton and P. A. Sheppard, together with the Director of the UKMO and representatives from the Royal Navy, Royal Air Force and civil aviation (Scrase 1962). In 1945 five more meteorologists joined the MRC. In 1942 another group, the 'Gassiot Committee', was set up as a link to the broader scientific community in the Royal Society. Last, but not least, there was the Royal Meteorological Society (RMS) with its prestigious *Quarterly Journal* and, from 1946, its popular journal *Weather*.

2.2. The development of British made computers

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

- Manchester University, constructed its first computer in 1947 with the help of some of the Bletchley Park team. Its first program ran in June 1948. The government provided funding for a scaled-up version, the 'Ferranti Mark 1'. Alan Turing joined in 1948 to become assistant director of the work on the MADAM (Manchester Automatic Digital Machine).
- Cambridge University started to operate its own Electronic Delay Storage Automatic Calculator (EDSAC) in May 1949. It was constructed by Maurice Wilkes and was the first machine to use delay lines to store information. When the catering firm J. Lyons decided to investigate the use of electronic office computing they chose EDSAC as the prototype. The first Lyons Electronic Office (LEO) machines ran in September 1951.
- The National Physical Laboratory in Teddington established a mathematical division under Alan Turing in 1945 to continue and co-ordinate various computing projects initiated during the war. In 1946 James H. Wilkinson joined the group. The plans were for an ambitious Automatic Computing Engine (ACE). However, delays meant that the first calculations were not made until 1950.

It is not clear how much of this was known to the outside world. In 1952 it came as a surprise to the audience at a NWP conference in Stockholm that the UKMO had access to computers (Persson, 2005, p. 144).

2.3. The development of advanced dynamical concepts

To put L. F. Richardson's famous 1922 work into perspective one must first acknowledge V. Bjerknes' declarations in 1904 and 1913 about the need to calculate the weather mathematically. These sentiments are also present in a series of articles by Leo C. W. Bonacina (1882–1975) who strongly championed Bjerknes' programme (Bonacina 1904, 1905, 1913).[1] The Director of the UKMO, Napier Shaw, toyed with the idea of objective forecasts and even wrote an outline of a meteorological version of Newton's *Principia* (Shaw 1913, 1914). In 1905 Shaw had instigated the 'Monday Evening Discussion' to give UKMO staff members, their friends and others interested in meteorology an opportunity to discuss current scientific papers of general interest.

2.4. Early planning: 1948

On 25 May 1948 a meeting was held jointly by the UKMO and Imperial College to discuss 'The Possibilities of Using Electronic Computing Machines in Meteorology'. Present at the meeting were Drs G. C. McVittie, R. C. Sutcliffe, C. S. Durst and E. T. Eady.

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

Charles S. Durst (1888–1961) joined the UKMO in 1919 and became instrumental in the 1920s in introducing the Hollerith punch card system for the storage and

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

analysis of observations from the British Empire. He gradually acquired a deep knowledge both of the world's synoptics and of the state-of-the-art data handling.[2] In 1947, learning that the EDSAC computer was to be built in Cambridge he re-read Richardson's book.[3]

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

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

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

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

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

After further discussion it was agreed that the UKMO should recruit or establish contact with someone who was familiar with the methods of computational mathematics and synoptic meteorology, with emphasis on the first aspect. (See Appendix 1 for the complete minutes of the meeting.)

Eric Eady had been asked to keep the top scientists of the MRC informed. This seems to have happened at its meeting on 24 June, because then the discussion was about the possibility of computing in connection with short- and medium range weather forecasting.[4] Another result of the meeting was that electric desk-calculators were obtained. The staff in the Forecast Research Division then spent 'many a boring hour', using these calculators, which lacked even the facility of automatic multiplication (Hinds 1981).

3. Sutcliffe versus Rossby

After the war Jule Charney, Reginald Sutcliffe and Carl-Gustaf Rossby developed three different simplified models of the large-scale atmospheric circulation. Charney's model was still too demanding for the British computers; but the other two were taken further.

3.1. The Sutcliffe development equation, 1939–50

> Rudimentary though [my theories] were, they were ahead of anything published at that time in that they were dynamically based, quantitative in principle, dealt with the atmosphere as a baroclinic continuum and were not obsessed with the cyclone problem. (Sutcliffe to Pedlosky 1982, in Phillips, 1990)

The problem now was to define what kind of mathematical model to present to the computer. This was a crucial issue. There were two schools of thought: the Chicago school under Rossby favoured a barotropic model,

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

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

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

whereas the UKMO under Sutcliffe were inclined to a baroclinic approach. Forecasts of precipitation were seen as the essence of weather forecasting and demanded forecasts of vertical motion, something of which only baroclinic model were realistically capable. On the other hand they demanded a lot of the limited computer capacity.

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

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

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

which allowed him to infer 'development' from the surface and upper-air patterns (the first term) and the time evolution of the thermal pattern (the second term).[5]

After the war Sutcliffe 'dusted off' his 1939 paper and presented it with pressure coordinates[6] and, probably influenced by Rossby's Chicago school, with vorticity instead of wind components (Sutcliffe, 1947).[7] He also included the latitudinal variation of the Coriolis parameter (the 'β-effect'):

$$\begin{aligned} Development &= f(div\,V - div\,V_o) \\ &= -V_T\,\frac{\partial\,\zeta_T}{\partial s} - 2\,V_T\,\frac{\partial\,\zeta_0}{\partial s} - V_T\,\frac{\partial f}{\partial s} \end{aligned}$$

where the first term was the vorticity advection or 'thermal vorticity effect', the second the thermal steering and the third the 'β-effect'. Disregarding this term, which he had 'so far' not found to be 'particularly noticeable', Sutcliffe established practical rules to distinguish areas of cyclonic and anticyclonic development from the upper-air charts.

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

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

$$f(div\,V - div\,V_0) = -V_T\frac{\delta\zeta_T}{\delta s}$$

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

3.2. Rossby's barotropic concept

> I have often wondered what a Rossby wave was. (Lord Harold Jeffreys to Michael McIntyre 6 February 1987, Royal Meteorol. Soc. interview)

As with the Coriolis effect, about 90% of the intuitive, qualitative textbook explanations of the Rossby wave are either wrong or misleading. As with the Coriolis effect, they are wrong in particular in relation to the mathematics they are supposed to clarify. Part of the blame for the confusion falls on Rossby himself and his classic paper of 1939. To explain his wave equation in a qualitative, intuitive way he borrowed an isobaric channel idea that Jack Bjerknes had made use of at a meeting in Germany 1937. To explain the kinematics of cyclones Bjerknes had used the gradient wind approximation. But whereas Bjerknes had just taken the curvature into account, Rossby et al. (1939) also considered the latitudinal variation of the Coriolis parameter.[8]

[5] The interpretation of the first term led to the works on cross-frontal circulations by Sawyer (1956) and later Eliassen (1962). The second term led, via a wartime memorandum (Sutcliffe, 1941), to the 'confluence theory' by Namias & Clapp (1949) where the Sutcliffe reference is mentioned at p. 331.

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

[7] According to Vincent Oliver (personal communication 1993) Sutcliffe made an inofficial and unrecorded visit to Rossby's institution in Chicago in 1944.

[8] Using gradient wind balance, Rossby et al. (1939) argued that for short waves the curvature effect will dominate and there will be convergence of winds west of the troughs, divergence east of the troughs: consequently the wave would move eastward. On the other

Figure 1. *A CAV-trajectory (above) and (below) the corresponding progressive, short wave length streamlines (long dashed lines) and the retrogressive, long wave-length streamlines (short dashed lines) it satisfies.*

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

At a conference in Toronto, on the eve of the Second World War, he worked out a second version of his paper (Rossby 1940). Here he made use of fact that the conservation of absolute vorticity for individual air parcels makes them follow a Constant Absolute Vorticity trajectory. Such a CAV-trajectory would, for eastward motion in the sub- or extra-tropics, follow a quasi-sinusoidal path. From the kinematic relation between trajectories and streamlines Rossby could now show that a specific CAV-trajectory was satisfied by either long waves moving westward, or small waves moving eastward. For a certain zonal flow there would be a stationary wavelength L_0 (see Figure 1). In a later paper Rossby (1942) elaborated further on this kinematic approach.

Judging from scientific papers, textbooks and popular articles from around 1950, which set out to explain Rossby's intentions, one gets the impression that Rossby had not been fully understood. Most readers only seem to have conceptually understood the sinusoidal trajectory of the CAV-trajectory. But by itself it does not define the streamlines of the wave.[9] Secondly, this image does not convey any reason why the flow pattern should move *westward*. This half-understanding turned out to have serious negative consequences since it gave the false impression that a Rossby wave was either a stationary feature or a simple wave moving downstream.

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

3.3. The 1950 Royal Meteorological Society Centenary debates

> The session brought out the differences of the points of view of several eminent meteorologists and was stormy. (John Sawyer, 1950, reporting the Centenary celebrations in *Weather*)[10]

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

The whole of the next day, Thursday 30 March, was dedicated to 'The general circulation'.[11] Rossby had to repeat that, although the ultimate driving force of the atmospheric circulation derived from the temperature difference between pole and equator, nevertheless the pattern of the circulation was determined primarily by mechanical forces in conjunction with the rotation of the earth.

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

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

hand, for long waves the latitude variation in the Coriolis parameter (the β-effect) would dominate and there would be convergence of winds east of the troughs and divergence west of the troughs: the wave would move westward, *against the flow*.

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

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

[11] A year earlier (24 January 1949), Rossby had lectured on the general circulation for the Royal Geographical Society (*Meteorol. Mag.* 1949. 80–82, *Weather* 1949: 71–73).

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

Perhaps it was Sawyer's unassuming manner that made the content of what he said fail to receive the attention it deserved. So, rather strangely, two forecasting cultures developed in which forecasters around the world looked at essentially similar patterns, but interpreted them differently depending on whether they were brought up under Rossby's or Sutcliffe's spell. Both schools applied simple vorticity advection: Rossby's students worked on black-coloured geopotential 500 hPa fields, Sutcliffe's on red-coloured 500–1000 hPa thickness fields.[12]

Whereas Rossby's forecasters knew they were working with a simple barotropic model, which only represented the average tropospheric flow, Sutcliffe's forecasters felt they were dealing with the troposophere in its full three-dimensional baroclinic complexity. In fact, the two groups were pursuing more similar paths than either appreciated at the time. The Rossby and Sutcliffe schools might in the end have found common ground – but a wider gulf opened up between them with respect to another feature: *group velocity*.

4. Defining the computational area

One reason why the 1950 ENIAC forecasts were such a success was the proper definition of a sufficiently large computational area, using the concept of group velocity in large-scale atmospheric motion. At the time of the Royal Meteorological Society's Centenary in 1950 a series of crucial misinterpretations had developed among British meteorologists. These would have serious repercussions both in Britain and in some other countries.

[12] Compare the use of potential and absolute vorticity. Although the fields look pretty much the same and are about equally conserved, the former invites more sophisticated discussions than the latter.

4.1. Group velocity

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

There were, at least, three ways of misunderstanding the group velocity concept:

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

(2) Statements were frequently made to the effect that the group velocities were in excess of the advective velocities in the free atmosphere. This is just not true.

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

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

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

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

4.2. The Scorer debate

> I knew that the barotropic model was dear to the heart of Rossby at that time. (Richard Scorer, personal communication 1994)

At an early stage British meteorologists considered the problem of computational area in NWP. Galloway (1948) had made a translation of the relevant papers by

Ertel (1941, 1944, 1948). Galloway had also raised the problem at an RMS meeting on 18 May 1949. Shortly after the Charney & Eliassen (1949) paper appeared, they introduced an 'influence region' in agreement with 'signal' or 'group velocity' discussions from a previous paper by Charney (1949) on the feasibility of numerical weather prediction.

Dr Richard S. Scorer at Imperial College was one of the leading British experts on waves, albeit acoustic waves, different types of gravity waves and lee waves. He was a good mathematician, but still could not make sense of Charney's paper. In July 1950 he sat down and wrote a letter to the *Journal of Meteorology* to ask what kind of disturbance the 'signal velocity' was propagating or how it arose (Scorer 1951). Any energy that may be derived from the mean motion was, according to Scorer, retained locally and did not propagate stable wave motion: 'No meteorological information traverses the ground faster than the wind at some level. No stable waves are ever observed that do not either travel with about the speed of sound or a stationary relative to their cause.'

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

In his answer, dated 6 October 1950, Charney (1951) wrote that he was not concerned with gravity waves. The essential argument was that the large-scale motions, whether stable or not, are approximated by the barotropic motion at a certain mean level, the equivalent barotropic level. Their phase velocities and dispersive properties, if not their amplification rates, may therefore be studied barotropically. The dispersed character of the large-scale motions, the deepening of a trough and the subsequent intensification of a downstream ridge, had been established theoretically and synoptically.[13] For the large-scale motions there are no steering currents: the currents are part of the motions themselves: 'If the barotropic model proves inadequate as a forecasting tool, the remedy is not to return to the antediluvian techniques of isobaric extrapolation, but to extend theory and computational method until the relevant non-barotropic dynamical factors can be taken into account.'

For an impartial reader it is obvious that Scorer was trying to find physical processes which transported the energy as rapidly as Charney's group velocity showed. But Charney could not answer him the way he wanted, with physical arguments.[14] He said essentially: We have good reasons to trust the barotropic model, and that is why we also trust group velocity.

It is easy to see where Charney and Scorer could have arrived at a common view. When Scorer was looking for advective causes, Charney repeatedly denied that 'influences are propagated advectively', although he must have known that there was by 1951 a widespread opinion that the energy indeed was transported by the upper-tropospheric flow. This was no contradiction of his own equivalent barotropic model of the atmosphere.

The advective zonal wind, represented by the 500 hPa wind, is the average of weaker winds below and stronger winds above. It was the latter which *physically* propagated the energy, whereas the *mathematical* group velocity formalism appointed the 500 hPa winds to this role. As in the case with Rossby and Sutcliffe's disagreements, the differences in Charney's and Scorer's *mathematical* formalisms took centre stage and disguised the similarity in the underlying *physical* processes.

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

4.3. The Royal Meteorological Society meeting of 17 January 1951

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

On 17 January 1951 an important meeting on 'Dynamical methods in synoptic meteorology' took place at the Royal Meteorological Society (*Weather* 1951: 61–62,

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

[14] In the summer of 1951 Scorer submitted a paper in which he elaborated his view that all large-scale meteorological effects are advected with the wind, and none is propagated from one part of the earth to another through the air as a medium: 'Charney, however, suggests that a developing unstable disturbance might change into a propagated stable one. Such an idea is not new though it has never been shown by experiment or theory how such a change could occur. Charney instances certain synoptic studies which are claimed to contain evidence of a propagated influence, but the reader is free in all these cases to disagree with the given interpretation of the observations' (Scorer 1952).

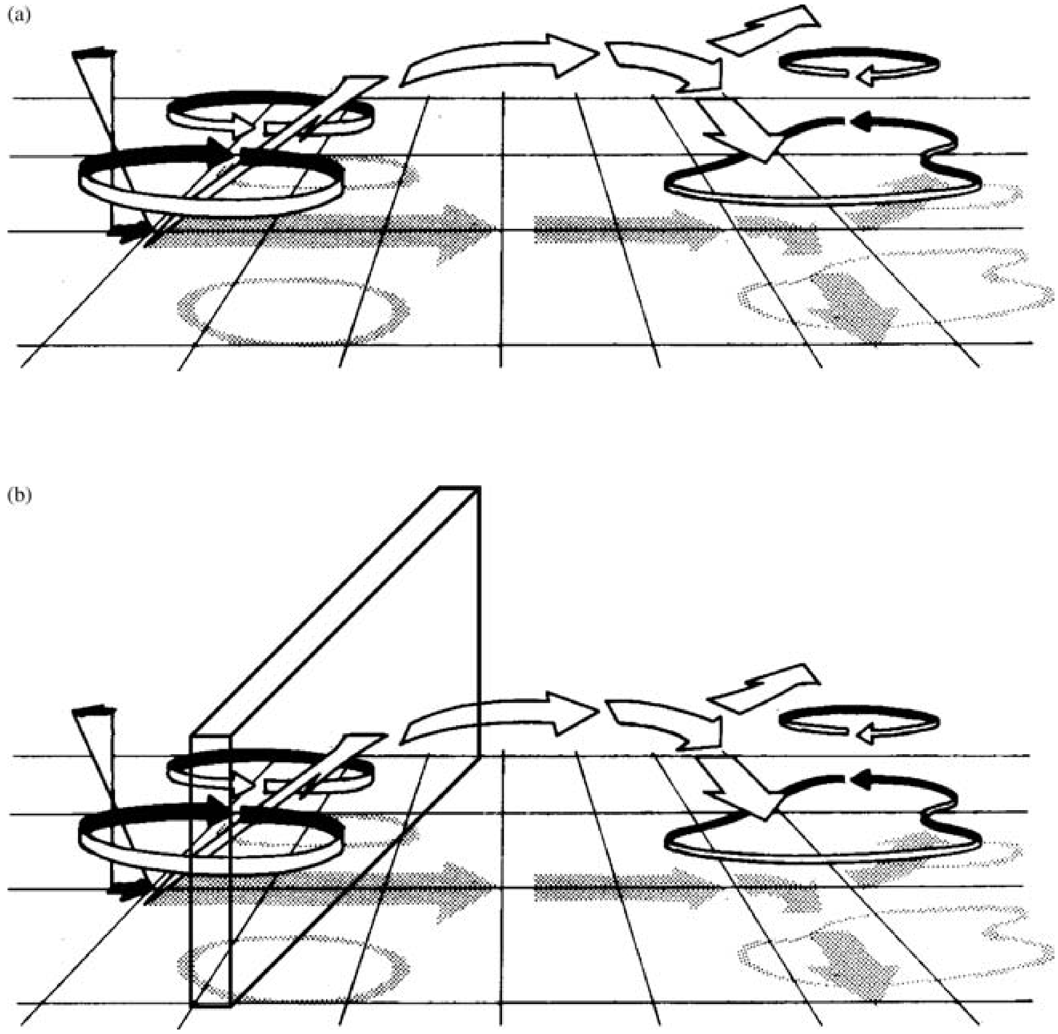

Figure 2. *The mechanism behind 'downstream development'. (a) A schematic 3-D image of upper-tropospheric energy transport (from Hoskins, James & White 1983). It illustrates how kinetic energy (in this case represented in terms of E-vectors and wave activity) released in one upstream baroclinic system feeds through the upper-tropospheric flow into the next, downstream baroclinic system which is under development. (**b**) Inserting any type of artificial, more or less transparent 'curtain' would block or diffuse the energy flux from the upstream system to the downstream. This becomes most serious when the 'curtain' in reality acts like an impenetrable 'wall', which is the case for constant boundary conditions. But even for nested models, any loss of energy at the boundaries, due to smoothing or diffusion, will be detrimental for the simulations of dynamic developments inside the computational area.*

Q. J. R. Meteorol. Soc. 1951: 457–73 ff, *Meteorol. Mag.* 1951: 112–114). Sutcliffe opened by saying that there were only two approaches to the forecasting problems: Rossby's and his own. He hoped his method would ultimately lead to improved methods of forecasting and that the approach to numerical forecasting 'might be along these lines'.

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

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

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

The head of the long-range forecast section, Forsdyke, testified that the Rossby wave formula did not apply to waves in the usual sense of energy-propagating disturbances. Sumner objected that Rossby waves could be both moving and stationary, and that Cressman (1948, 1949) had arrived at slightly better results. Sutcliffe ended the debate by expressing 'considerable doubts' about the barotropic theory and 'broadly agreeing' with Scorer's view about the extrapolation: 'When a satisfactory solution to the 3-D problem emerges it will derive little or nothing from the barotropic model – which is literally sterile.'[15]

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

5. Defining the computational system

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

5.1. Fred Bushby

> Fred Bushby and I always seemed to cooperate easily and effectively . . . . Fred was probably more interested in the techniques of computer programming (a novel field at the time) than in the fundamentals of meteorology. Also one must remember that only a favoured few could make a career in the Met. Office in research alone, and Fred Bushby, no doubt, would have wished to prove himself competent in the technical and organisational side of the Office work . . . Indeed Fred did make his mark in both research and the service side of the Office. (John S. Sawyer, personal communication, 1995)

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

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

A test of the method gave results 'better than could be expected if the method was fundamentally unsound'. His report (Bushby 1951b) was received 'with interest' by the MRC. They found the method, though very crude, was an attempt at numerical forecasting, and discussed possible improvements and alternative methods of attack. Their positive judgement justified further work.

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

5.2. Preparations for NWP, 1951–53

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

Active work on numerical weather prediction at the UKMO seems to have started in the aftermath of the meeting on 17 January. A research plan for the 'Application of computing machinery to forecasting problems' was decided in March or April, and work was apparently also under way at Imperial College London.

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

[15] This is what Sutcliffe said according to unanimous reports of the discussions, which were published soon afterwards in Weather (1951, 61–62) and Meteorological Magazine (1951, pp. 112–14). The same discussion as reported in the *Q. J. R. Meteorol. Soc.* (1951, 457–73), appears much more conciliatory, probably because the text had been drafted at a later stage when the results from the first ENIAC runs had reached Britain in March 1951 through the Charney et al (1950) article in Tellus. See 4. 21 below for an analysis of Sutcliffe's changing attitudes to NWP.

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

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

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

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

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

5.3. The Sawyer-Bushby model

> The approach to numerical solution of the equations of motion could follow [Charney's] lines which recognise the essential characteristics of large-scale atmospheric motion. (Sawyer & Eady 1951)

The impact of the debates in the British meteorological community is apparent in a review article 'Dynamics of flow patterns in extra-tropical regions' by Eady & Sawyer (1951). The article is very well argued and still makes good reading. Nevertheless, with respect to the question of barotropic versus baroclinic models, the paper is rather ambivalent. Rossby was on the one hand said to have made 'arbitrary simplifications' and 'sweeping approximations' for his long wave theory. 'His' wave was explained in the common but erroneous way as a trajectory of an air parcel conserving its absolute vorticity. 'Overenthusiasts' of the barotropic model were reminded (three times) 'that we cannot hope to explain all the principal features of the large-scale motion by such means'.

On the other hand, Eady and Sawyer found that the 'qualitative success' of the ENIAC runs had encouraged further investigations of the barotropic model. Although the underlying concepts were theoretically 'naive' or 'unsatisfactory' and the model 'inaccurate', the results were seen as 'interesting': 'The aim is not a completely satisfactory theory but rather to find a way of making a start towards a realistic theory.'

Sawyer and Eady presented the concept of group velocity with the conventional geometric arguments of superimposed sine waves: 'Some investigators have inferred that there is evidence of such behaviour in the atmosphere, but as in the case of the individual waves, there are difficulties in drawing very definite conclusions.' Shortly after the article was published, two British meteorologists, Smith and Forsdyke, undertook a major synoptic investigation to find out whether there existed any such thing as 'group velocity' and if it had any synoptic significance.

It was at this time, in late autumn 1951 and early 1952, that Sawyer and Bushby developed their simple baroclinic model, inspired by Sutcliffe's theory (Sawyer & Bushby 1953). The thermal wind direction was constant in any vertical column and its speed was proportional to the vertical pressure difference through the layer. Three simple differential equations computed the height of change of a contour surface, thickness 1000 hPa to that surface and average vertical velocity.

Similar models would soon emerge from the United States and Europe.[16]

The theoretical work was finished in February 1952 and discussed by the MRC on 26 March. But it was not until 29 August that the paper was submitted to the *Journal of Meteorology*.[17] In the meantime Bushby made some calculations of the vertical velocity and thickness tendency using Sutcliffe's theory. The results showed good agreement with reality for six synoptic situations (Bushby 1952b). One year later he undertook a further evaluation of Sutcliffe's development formula using 12 cases (Bushby & Hinds 1953f). Out of 90 development calculations, 41 gave useful guidance, 36 were not misleading and 13 were misleading.

In October 1952 Sutcliffe attended the NWP conference in Stockholm and presented 'Some preliminary experiments in numerical computation at the Meteorological Office'. On his return he reported in a Monday Discussion that although the research in numerical methods was going well both at home and abroad, there were no reasons to expect 'revolutionary improvements' in forecasting by these methods (*Meteorol. Mag*. 1952: 47–50).

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

The discussion took a new turn when C. V. Smith brought up the mechanism of *downstream effects*.[18] As he understood it, the formation of a major trough in the upper westerlies should give rise to dependent wave trains downstream. Examples of such downstream trough formations, Smith argued, indicated that they were initiated by baroclinic developments. This provoked J. K. Bannon to ask why a barotropic model could anticipate *baroclinic* processes? This highly relevant question seems to have been left unanswered (*Meteorol. Mag*. 1953: 148–153).

5.4. Bushby and Hinds' tendency calculations

> It was a thrilling experience to visit the Lyon's machine. (Norman A. Phillips, personal communication about his visit to Britain in 1953)

At the end of 1951, the LEO 1, a copy of the Cambridge EDSAC machine, was built by Messrs J. Lyons, the caterers. This large organisation was beginning to explore the possibility of using a computer in office work – for scheduling deliveries, ordering supplies, etc.[19] The UKMO managed to have access to the machine and most of the computing was done during evening sessions with assistance from the staff both in operating the computer and the provision of supper in the managers' mess.

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

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

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

[16] The degree to which these other two-level models were inspired by Sutcliffe's equation is debateable. In 1939, he had already shown that a two-level model was quite powerful. On the other hand, there were few alternatives as to how these two-layer models could actually be constructed.

[17] Why this five-month delay? There are few, if any changes between the original and final version (Sawyer & Bushby 1953).

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

[19] Since rationalising of office work was politically controversial, the computer was kept in a secret location, at their Cadby Hall headquarters, rather than their normal Lyons offices, in order to prevent labour unrest by the regular office staff.

when tested with correct boundary conditions the agreement between computed and actual tendencies significantly increased:

> The effect of boundary conditions seems important if a small area is considered. Before time integration is undertaken it would seem necessary either to increase the area under consideration, so that the effect of boundary conditions would not affect the central area, or to make a preliminary estimate of expected changes around the boundary and feed this into the machine. (Bushby & Hinds, 1954a, Discussion)

To dampen the adverse effect of the wrong boundary conditions the westernmost six columns of points were excluded from the verification (Bushby & Hinds 1953a–c,e,f).[20] Some of these results were presented by Johns Sawyer at an international conferecne in Toronto later in 1953 (Sawyer 1954). By the time their tendency calculation paper was accepted for publication in *Q. J. R. Meteorol. Soc.* in December 1953, Bushby and Hinds had passed a new milestone: two 24-hour full-scale integrations of the baroclinic Sawyer-Bushby model (Bushby & Hinds 1953d).

6. Carl Gustaf Rossby's visit to Britain in February 1954

It was a significant moment in February 1954 when the British could present their first baroclinic integrations to an international audience which included Carl Gustaf Rossby from Sweden and Joe Smagorinsky from the United States. It coincided with a presentation of a synoptic investigation into downstream effects in atmospheric dynamics by C. V. Smith and A. G. Forsdyke. These events are luckily very well documented, in particular in Rossby's comments to the British work.

6.1. The Smith & Forsdyke paper, 1952–54

> Indeed, 'dispersion' seems to be a useful concept only in so far as barotropic theory is invoked. (Smith & Forsdyke 1952)[21]

By 1953 the stock of theoretical and synoptical investigations of downstream development had increased (Reichelderfer 1952; Riehl et al. 1951, 1952). Theoretically it was recognised that, in general atmospheric wave motions, kinetic energy released in baroclinic disturbances was propagated much faster than the phase speed. The transport was mainly carried out by the upper tropospheric flow, most importantly by the jet-streams. The transfer of energy was governed by the large-scale adjustment between wind and pressure gradient in 'direct' and 'indirect' circulations (McIntyre 1951).[22]

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

But all this accumulated evidence did not seem to have left any impact in the references in the memorandum Smith & Forsdyke (1952) now finalised. The authors only made references to three theoretical papers, one by Eady (1950) and two by Rossby (1945, 1949a). Smith and Forsdyke acknowledged that interaction between synoptic systems in different longitudes had been recognised for a long time. Latterly, *downstream effects* had been derived directly from Rossby's work on long-wave flow patterns as a part of barotropic or equivalent-barotropic theory.

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

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

> Rossby (1945) suggested that the process was one of energy dispersion and showed, with various assumptions, that the group velocity should in fact be greater than the current velocity. Apart from affording a plausible explanation of developments not otherwise readily interpreted 'barotropically', it is not clear what the value is claimed for the idea of 'dispersion' in synoptic meteorology and the present statistics do not afford much evidence.
>
> In the first place it may be that the question is begged by speaking of a 'primary' trough and implying that subsequent formations downstream arise consequently. There is rarely evidence to prove that a downstream oscillation would not have arisen just as well without

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

[21] In the1990s, when I lectured at the ECMWF about group velocity in the common 'interference of sine waves' way, some British students thought 'energy dispersion' only applied to spectral models, like the ECMWF one, and not to grid point models as the UKMO one. Rossby must have smiled in his heaven.

[22] In 1947–48 it had already been observed that many cyclones increased their potential energy as they developed (Carson 1948).

> the presence of the primary. (Smith & Forsdyke 1952)

Then Smith and Forsdyke referred to Eady (1950) and others who implied that long-wave oscillation may arise as a result of 'instability': 'There is no reason to look to "dispersion" for an explanation: indeed, "dispersion seems to be a useful concept only in so far as barotropic theory is invoked".

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

> The impression is not the one of energy transfer with a 'group velocity' but of a local energy source such as only baroclinic theory can provide for . . . This does not mean, however, that downstream effects do not occur. Certainly on occasions the development of one oscillation is clearly linked with an earlier disturbance elsewhere, but the mechanism of the linkage probably involves the processes of baroclinic development. (Smith & Forsdyke 1952)

The Smith & Forsdyke report had been finalised on 22 September 1952, but it was not until 22 January 1953 that it was discussed by the MRC. It was submitted to the *Quarterly Journal* on 24 April where it would appear one year later very much reduced in length (Smith & Forsdyke 1953). It was also presented at a meeting of the Royal Meteorological Society, almost a year later, on 17 February 1954.

This meeting is highly interesting because the discussion of Smith and Forsdyke's paper was eclipsed by the presentation by Bushby and Hinds of the first computer-based baroclinic forecast in Europe. These were the foci of two important discussions, one on 15 February 1954 at the UKMO, the other on 16 June at the Royal Meteorological Society. The meeting gained extra significance because of the presence of two distinguished scientists: Carl Gustaf Rossby from Sweden and Joe Smagorinsky from the USA.[23]

6.2. The UKMO Monday Discussion of 15 February 1954

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

The first European baroclinic integrations were presented at a UKMO Monday Meeting on 15 February 1954. It drew together many great names in meteorology: Rossby and Joe Smagorinsky from abroad, Stagg, Sawyer, Scorer, Douglas, Ludlam and others from Britain (*Meteorol. Mag*. 1954: 175–182). Fred Bushby made the presentation. The analysis was undertaken in a 18×14 grid of 260 km, the forecasts were run in a 14×10 grid. The time-step was 1 hour and a 24-hour forecast took 4 hours to compute. The machines were the LEO owned by J. Lyons & Co and the Ferranti machine at Manchester University. The baroclinic model was tested in two cases, 14 March 1949 and 27 January 1952 (Figure 3).

For the first case, computations began with a two-hour time step. Indeed, such a long time step had been used by Charney et al. (1950) in the ENIAC runs. But the grid length then had been 600 km, not 260 km as with the UKMO model. Obviously the British were not aware of the Courant-Friedrich-Levy (CFL) condition, although it had been discussed by Charney et al. (1950) in their report on the ENiAC forecasts.

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

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

In his summary Bushby pointed to a tendency of the model to exaggerate anticyclonic developments, which were due to the geostrophic approximation and lack of friction. Errors of lesser extent were due to the arbitrary choice of boundary conditions and effects of

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

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

For unknown reasons he did not come to receive the prize in person;. it was forwarded via the Swedish ambassador, Gunnar Hägglöf.

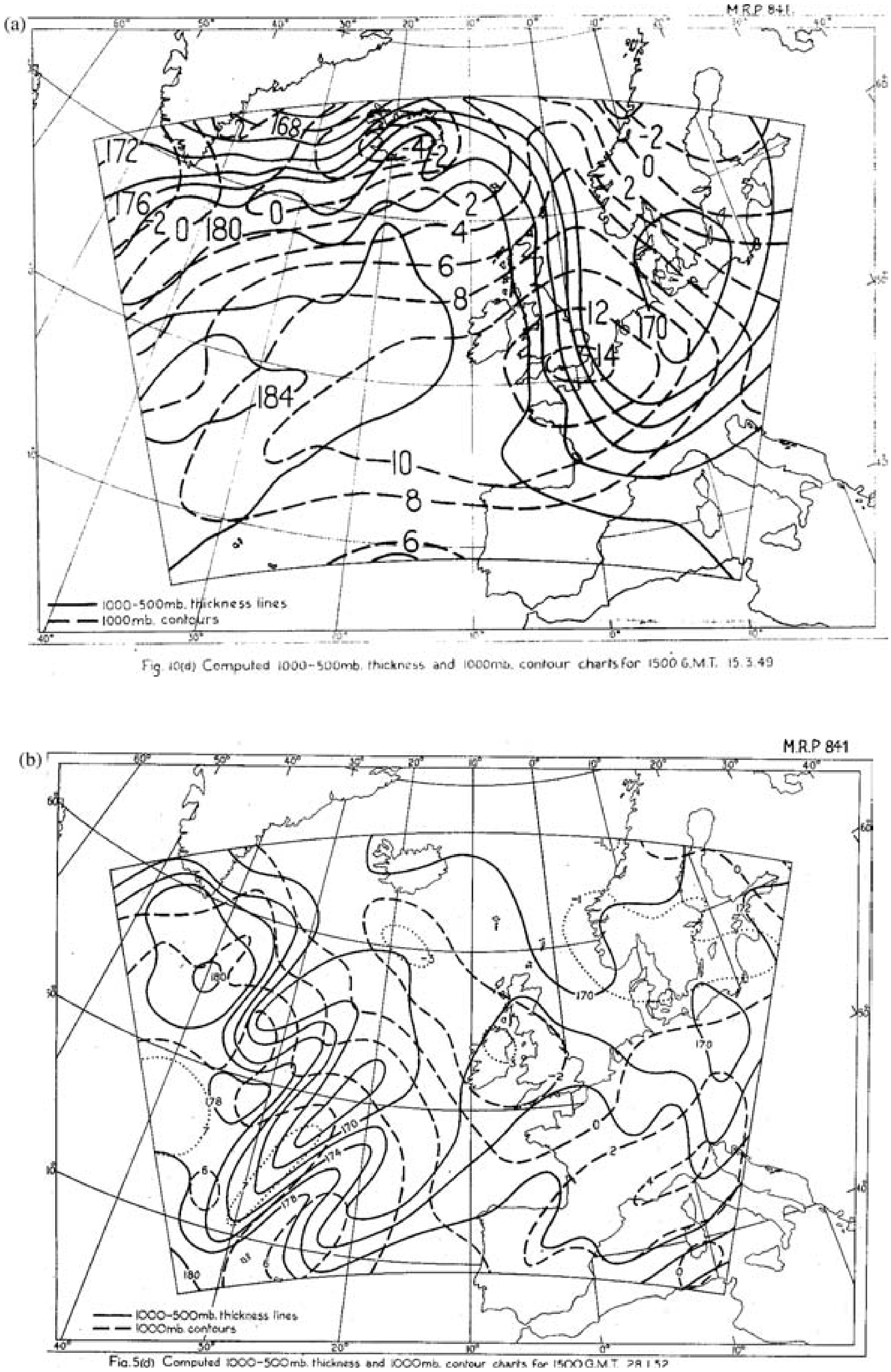

Figure 3. *Is this what Rossby and Smagorinsky saw at the UKMO Monday Discussion? (a) The first forecast from 14 March 1949 15 UTC + 24 hours, run with a time-step of 2 hours. Numerical instabilities showed up after 18 hours (**b**) This forecast from 27 January 1952 15 UTC + 24 hour 1000 hPa, was also run with a 2-hour time-step, but displayed signs of serious numerical instability after just 10 hours. It was restarted eight hours into the forecast with a time step of one hour. Both pictures are from an internal UKMO Meteorological Research Memorandum (Bushby & Hinds 1953d).*

topography. In the subsequent forecasts a time-step of one hour were used. Problems related to numerical instability were to be the subject of a special study.[24]

Rossby congratulated the UKMO on being the first official weather service to experiment with numerical forecasting. But, as he wrote to Charney some days

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

later, he was 'a bit concerned about the direction of their work, in particular their use of boundary conditions because several of the 500 mb charts showed a most remarkable evidence of some sort of instability in the border regions (wobbly contours etc.).[25]

Bushby and Hinds' results were to dominate the discussion two days later at a meeting of the Royal Meteorological Society, which was announced to focus on something else.

6.3. The Royal Meteorological Society meeting of 17 February 1954

> A lively discussion on the possibility of numerical forecasting followed. It had very little to do with the paper, but it enabled Fellows to hear Dr Rossby suggest that numerical methods should concentrate on forecasting for two or three days ahead, whereas Dr Eady and others thought that greater promise lay in the attacks on the 24-h problem. (*Meteorol. Mag*. 1951: 216, probably written by John Sawyer)

Two days later Rossby and Smagorinsky attended a Royal Meteorological Society meeting. Smagorinsky presented his work on the dynamical influence of large-scale heat sources and sinks on the quasi-stationary mean motions of the atmosphere (Smagorinsky 1953) and Smith and Forsdyke their paper on downstream effects (Smith & Forsdyke, 1953). In the short discussion Smith made the comment that 'purely barotropic evolutions' were uncommon over three to four days. To produce extended forecasts by NWP it was therefore necessary to have mathematical models, which included baroclinic developments and non-adiabatic processes.

It seems to have been Rossby who triggered the debate by turning long-held opinions on their head by stating that NWP would probably be used for forecasting the movement of large-scale features over 2–3 days before it was used for detailed 24-hour forecasting. Rossby also suggested, according to his letter to Charney, that the UKMO should, if the capacity of the machine allowed, also run daily barotropic 24- or 48-hour 500 hPa forecasts. The prediction of the sea-level charts would be considered as a research project not yet ready for routine tests.

In a letter to Dahlqvist dated 21 March 1954 Rossby came back to the issue of boundary values, a problem he had no doubt also discussed with the UKMO staff. (see Persson, 2005 Appendix 1, pp 158–59 for full text). Would it not be possible to set up a computational scheme with the observed, instead of the arbitrarily assumed boundary conditions along the periphery of the forecast area? Since the aerological observations were generally given only every 12th hour they would probably have to develop an interpolation method for time intervals between the aerological times.

For the first tests the extrapolations were to be carried out subjectively by 'experienced synopticians'. Later the machine would do this. If it turned out that the model could be run for 2–4 days without the forecasts for the inner area becoming 'too stupid', this would indicate progress towards forecasts for unknown areas, because sooner or later the influence of the initial values in those regions disappears.

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

7. Further numerical integrations, 1955–56

The mid-1950s was a period of exciting exploration of the new computational tool, and it was in this work that several female meteorologists played a significant role.

7.1. More baroclinic calculations and a quasi-adjoint integration

> We merely changed the addition instructions to subtractions! (Marvis Hinds, personal communication 2004)

At a meeting of the Royal Meteorological Society on 16 June two papers by Bushby and Hinds were presented and discussed: the tendency calculations undertaken in the summer of 1953 (Bushby & Hinds 1954a) and their baroclinic integrations undertaken in the following autumn (Bushby & Hinds 1954b). During the discussion a young meteorologist, John B. Mason from Imperial College,[26] raised an intriguing question: 'Is it possible for rapid developments on the boundaries of the grid to upset the forecast seriously for the centre of the region within 12 or 24 hours? Had they tried the effect of putting the *actual* boundary conditions into the equations after the event, and, if so, with what result?'

Bushby answered that the size of the forecast region was originally chosen so that there would be 'little

[25] The full letter can be found in Appendix 2.

[26] John Mason would in time become the Director of the UKMO but in 1954 he was employed by the Meteorological Department at Imperial College. He was not yet a PhD and had the formal title 'lecturer in meteorology'. When once asked by Tor Bergeron about his education in meteorology, he said that he had none. 'But you are lecturing in meteorology?' 'No, I am not', said Mason. Bergeron laughed: 'So here is a lecturer in meteorology who is neither meteorologist nor does any lecturing!' (Sir John Mason, personal communication 2003).

likelihood' of the centre of the region being affected by rapid developments on the boundary during the forecast period. However, it now seemed likely that there were isolated occasions when boundary affects may have caused errors in the centre of the region. It was their intention to carry out some computations using the correct boundary conditions.

During spring 10 more baroclinic forecasts were made (Bushby & Hinds, 1954c).[27] They could identify shortcomings due to lack of heating from below of cold air masses over sea and the assumption of zero change along the boundaries. They would also have preferred to have 600 hPa data instead of 500 hPa. Nevertheless they recorded a skill 'only slightly less' than a conventional forecaster.

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

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

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

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

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

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

7.2. Mavis Hinds and the 'ladies of early British NWP'

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

At the opening of the 16 June meeting, Fred Busby's co-worker, Mavis Hinds, was admitted a 'Fellow' of the Royal Meteorological Society[28] as an acknowledgement of her contributions. Mavis Hinds is one of the first female meteorologists of any prominence, and the first woman to play a leading role in the development of NWP.[29] But she was not an isolated case. In contrast to what one could expect from the legendary stuffy British Civil Service, the UKMO could pride itself not only of allowing *four* women to make important contributions, but also of acknowledging this by co-authorship!

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

In 1950 most children left school at 14 with no qualifications. About a fifth left at 16 with a School

[27] Interestingly, one of these forecasts was of the Dutch storm of 31 January 1953. It was very skilfully forecast and the authors expressed the view that 'there is no doubt that if these charts had been correct the floods would have been even more calamitous than they actually were!' The results of the experiments were presented at an international ICGG meeting in Rome in September 1954 (Sawyer 1954).

[28] As it is still today, anybody can become a 'Member', but it takes personal recommendations to become a 'Fellow'. The annual fee, however, is the same.

[29] In 1950–53 the wives of Joe Smagorinsky, John von Neumann and Arnt Eliassen all contributed anonymously through programming and hand calculations.

Certificate (today's GCSEs) and very few stayed on to take Higher School Certificate (A-levels) at 18 and possibly go to university. But then the student had to be rich or get a scholarship to help with the costs. Even so, there were very few jobs for graduates, especially for female graduates, except in teaching or medicine. But Mavis was fortunate, thanks to her abilities, to be granted both a scholarship and a place at University College, London to study mathematics. This was very unusual in 1947, as all the men who had been at war were filling the college places. So her father had to let her go! She graduated in 1950 and worked in the aircraft industry for a short while, before she joined the UKMO in January 1951, as an Experimental Officer.

In the UKMO there were three 'classes' of staff: (1) Assistants, with a School Certificate, who made observations and plotted charts; (2) Experimental Officers, with a Higher School Certificate or possibly degrees, who did the largest part of the routine forecasting, and were based on airfields far and wide across the globe; and (3) Scientific Officers, much smaller in number, with good degrees, who were responsible for research, but who were also expected to be able to do forecasting. They filled the most senior posts. There were many women among the Assistants, some among the Experimental staff but practically none among the Scientific staff.

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

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

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

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

7.3. Impressions from abroad

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

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

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

In spring 1954 the new director, O. G. Sutton, attended a computer conference in California which made a great impression on him (Sutton 1954a, 1955b). Jule Charney had shown forecasts of the Thanksgiving Day Storm. Sutton also learned about the newly formed Joint Numerical Weather Prediction Unit (JNWPU). This was, in Sutton's words, 'a historic turning point of our science'. When he returned home he sent Fred Bushby to Stockholm for some weeks and Ernest Knighting for a year to the United States, first to MIT, then to JNWPU.[30]

On 15 November 1955 Bushby and Knighting reported to the 37th Synoptic and Dynamic Sub-Committee of the MRC on their visits to Sweden and the USA. Bushby had spent three weeks at Rossby's institution in Stockholm and gave an account of their electronic computations of +24, +48 and +72-h forecasts. Knighting reported on his nine-month visit to the JNWPU and his witnessing of +12, +24 and +36-h forecasts of pressure contour heights for 900, 700

[30] Ernest Knighting started as a teacher, but joined the Meteorological Office in 1940, first in the CFO, later with Sverre Petterssen's upper-air branch in Dunstable. He left forecasting in 1949 to undertake research (Knighting 1956b, 1956c, 1960a, 1960b; Knighting et al. 1959, 1960). He also wrote review articles (Knighting 1961a, 1961b), reports from international meetings (Knighting 1957a, 1957b, 1962b, 1962c, 1965) and numerous popular articles where he promoted NWP (Knighting 1951, 1956a, 1958, 1961c, 1961d, 1962a).

and 400 hPa obtained from a three-level atmospheric model.[31]

At an UKMO Monday Discussion 19 March 1956 Knighting (1956a) reported how the JNWPU ran 500 hPa barotropic forecasts over nearly the whole of the Northern Hemisphere to avoid boundary errors. These forecasts were intended to specify boundary conditions for baroclinic forecasts for smaller areas. Bushby presented early results of objective analysis. In parallel with their work on objective analysis,[32] Bushby & Huckle (1956a, 1956b) worked on replacing the geopotential field by a stream function to avoid the spurious formation of anticyclones. They were successful and the tests showed positive results.[33]

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

In 1957 the team encountered a new problem. The forecasts were not only sensitive to analysis changes, but also to changes in the size and orientation of the grid! This was seen by Knighting, Jones & Hinds (1958) as 'a serious' problem and seemed to place the prospects of accurate NWP in doubt.[34]

8. The cultural effects of the computer

In Britain, as in most other countries, the late 1950s saw wide-ranging debates about the benefits and the evils of the computer. The computer had a particular significance in meteorology because of the clear evidence of its ability to both help the professional meteorologist and to replace him in some distant future. Whereas some doubted the computer's ability to forecast weather, others wondered if we really needed perfect forecasts!

8.1. Computing and the public opinion

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

The advent of electronic computing caused controversy in many spheres of life, including meteorology. In parallel with the mathematical, computational and scientific advances, there was in Britain an ongoing debate about weather forecasting with machines. The debates followed several lines of argument.

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

Knighting (1951) pointed out that the fundamental differential equations could not in principle be adequately solved, only approximately formulated. Sutton (1951) in an article in *Weather* made clear that the initial conditions were insufficient to determine the final state. The apparently inescapable element of randomness in the atmospheric system would defeat all attempts to extend mathematical prediction beyond a certain interval of time. When Sutton, as an example, mentioned that 'we may never be able to say with complete confidence that it will not rain on the vicarage garden party', the Editor answered in the next issue of *Weather* that he couldn't see any problem with that, rather the opposite: 'Wasn't there something to be said for the unexpected depression that washed out the vicarage garden party last year?'[35]

John Sawyer (1952) wrote that whether attempts at NWP ended in success or failure, the experiment should improve our understanding of meteorological dynamics: 'If we only learn that the theoretical basis of computations is faulty, the effort will not have been in vain.'

John Sawyer was not only the deputy leader of the Met. Office scientific research and the brain behind the NWP model, but he was also an acclaimed populariser of meteorology both in newspapers and at the BBC. In December 1953 he was bestowed the honour of delivering the prestigious annual 'Popular Lecture' at the Royal Institution. A crowded audience of pupils and

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

[32] It has been difficult to form a picture of the UKMO work on objective analysis. For unknown reasons there were two approaches; see D. H. Johnson (1956, 1957), Bushby (1956a, 1956b), Huckle (1956), Bushby & Huckle (1957) and Corby (1961).

[33] Whereas in Bushby & Huckle (1956a) two forecasts out of three were said to have improved, in Bushby & Huckle (1956b) two cases were reported neutral and one positive.

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

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

teachers from 47 secondary schools heard him present a 90-minute sketch of the development of 'Weather Forecasting' ending with a brief reference to the possible use of electronic computers in long-range forecasting. So many people had to be turned away, and the talk was so appreciated that it had to be repeated two months later for 450 pupils and teachers from 90 secondary schools in the London area (*Meteorol. Mag*. 1954, pp. 26–27, 91).[36]

Sutton (1954b) in a widely published talk on 28 April 1954 saw 'encouraging' results with the new numerical technique, which were on a par with results from subjective methods:

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

Like Sutton, Eady (1955) and others, many people expressed the view that there was a 'disappointingly slow advance' in synoptic meteorology, and the 'one ray of light in the gloomy picture' came from NWP. Sutton was, however, also careful not to distance himself from the forecasters: 'This is not to say that ultimately the mathematician rather than the physicist will be the operational forecaster.'

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

In the summer of 1955 the Meteorological Office celebrated its centenary. In his speech, the Deputy Director for Forecasting and Central Services, S. P Peters, spoke of the future of forecasting and the public services, and he ended on a positive note:

> As regards the effect on forecasting of the use of electronic computers, it is too early to express any definite opinion, since the employment of such computers in numerical forecasting is at present only in the research stage. There are, however, some grounds for supposing that, so far as obtaining forecast charts for 24 hours ahead is concerned, the electronic computer will prove to be a valuable aid, and its adoption in forecasting a very significant milestone in the history of synoptic meteorology. (Peters 1955)

The most positive reactions came during a conference of Commonwealth meteorologists in May 1952. John Sawyer's presentation evoked a great deal of interest, and the ensuing discussion was mostly in the form of questions to the speaker: How long did the complete process take? Was any smoothing of the data required? Were more than two parameters necessary? Could one improve on the geostrophic relation? What hope was there for tropical regions where the geostrophic relation did not hold? (Durward 1955).

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

The new computer was expected to arrive in 1957, but it would be almost three years before it would be in operation. The years of waiting were to be rather frustrating for the meteorological staff.

8.2. The pessimistic years, 1956–59

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

Shortly after the Royal Meteorological Society meeting on 17 January 1951, where there had been heavy criticism of Rossby's barotropic concept, the November 1950 edition of *Tellus* arrived in Britain with the stunning results of the ENIAC calculations. Sutcliffe later told John Burton (1982, 1990) how amazed he was:

> It was remarkable. I could not believe really the barotropic assumption, which means a two-dimensional atmosphere treating the whole atmosphere as one uniform mass . . . I was very surprised. I think it was surprising [that if] you treat it as an incompressible fluid and extrapolate it that you get anything that appears to have anything to do with the atmosphere. It has got no vertical motion, no depressions, no anticyclones and no fronts. And yet – it was exciting from a mathematical point of view that this could be done and could be done by a computer in real time.[37]

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

> Wherein lies the special fascination of forecasting? Not in the satisfaction of success, although this is great, but in the very certainty that the prediction will fail, to a greater or less degree. We are taught to look for the day when machines will calculate the future weather with a monotonous degree of success, but if that day comes

[36] For other popular presentations see Sawyer (1951, 1959a 1962, 1964) plus numerous minor contributions in *Weather* in the section headed the 'Meteorologist's Forum'.

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

> one satisfying profession will be lost to man and we must look elsewhere. There will be little more joy in the trade than there is in the repetition of the multiplication table. (Sutcliffe 1956)

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

Sutcliffe's feelings toward NWP can be sensed in the evaluations in the annual reports from the UKMO reflecting the work from 1950 to 1954.[38] At first he was optimistic although the results were not as good as those provided by the forecaster. The pessimism started to set in 1955 when he wrote:

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

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

So when the Meteorological Office celebrated its centenary in summer 1955, Sutcliffe did not mention computers at all in his talk about the future of research, science and development at the Met Office. Three years later he was more optimistic, regarding Britain as second only to the United States 'in its contributions to this revolutionary approach to forecasting'. He could already foresee teleprinted data from observing stations being fed into a calculating machine to be objectively analysed, stored and processed to produce forecast charts. 'The outcome in terms of forecasting guidance will surely be valuable; how precise and accurate may ultimately depend more on the unpredictability inherent in the unstable atmosphere than on the ingenuity of the research workers or the versatility of machines.'

The nadir came in 1959 at a public meeting:

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

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

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

The following year, the UKMO annual report contained an extensive article about the progress of the work – probably not written by Sutcliffe, because now the UKMO had something positive to report.

9. The operational breakthrough

Only when the Meteorological Office got its own computer could useful real-time forecasts be considered. But there were still many practical problems to overcome – and also a certain hesitance on the part of the scientific staff.

9.1. The Met Office gets its first computer

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

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

The first experiment was run almost every weekday from 12 January to May 1959 when 24- and 30-hour forecasts were made from midnight data. The analysis was based on a $+24$ hour preliminary field from the previous forecast. The area had been extended to 24×20 grid points covering an area from Nova Scotia to Russia and Spitsbergen to North Africa. The grid lengths were variable, with 244 km at 30°N and 320 km at 70°N. Hopes for satisfactory forecasts were limited to an inner 'verification' area – a rectangular network of 15×10 grid points covering about the same area as the CFO routine verification. There was simple allowance for heating of

[38] The annual reports covered the start of the year and 12 months prior to this: the 1956 report covered March 1955–March 1956.

cold air over warm oceans. The usual problems were identified: non-adiabatic processes, lack of topography, analysis errors, boundary conditions and numerical shortcomings. By then Sawyer (1959b) was working on introducing the effects of topography into numerical models.

In this experiment a lot of manual work was needed to check and correct observations. In the second experiment from 27 July to 13 November 1959, this was made automatic (Knighting Corby & Rowntree 1962).

A third experiment was run from 29 February to 2 June 1960 on a three-level model designed by Bushby and Claire Whitelam. It had three levels, 1000, 500 and 200 hPa and was mathematically constructed so that the 600 hPa level would represent a barotropic level (Bushby & Whitelam 1961). The results showed it to be superior to the CFO forecasts, apart from spurious anticyclogeneses and effects from the boundaries (Wallington 1962a, 1962b). It was now decided to mount a full scale real-time experiment in operational weather prediction from 21 November 1960 to 9 June 1961. Forecasts for 06 UTC were produced at 0930 UTC the day before on one-third of the days. About one-third of the time the computer failed.

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

At this time Fred Bushby had left Bracknell to stay in Aden as Chief Meteorological Officer for two-and-a-half years. This surprising move was the source of jokes and rumours for a long time. However, John Sawyer and Mavis Hinds explained to me that since the UKMO was part of the Ministry of Defence all staff (except women) had to take their turn (or turns) to man stations abroad. Fred knew that despite serving in Burma during the war, his name was near the top of the 'Overseas List' in 1960. Since the UKMO was moving from Dunstable to Bracknell in 1961, it seemed to him wise to volunteer for an Aden posting, thus selling his Dunstable house before the prices dropped, and taking his son Peter abroad before schooling was vital. Bushby's stay in Aden might explain Knighting's absence from the major NWP symposium in Tokyo, 7–13 November 1960. Britain was at this time imposing strict currency regulations for foreign travel.[39] Mavis Hinds thinks that if Fred Bushby had still been in England he might have fought hard enough to fund someone to attend. As it was, Knighting had to mail an article to the symposium reporting on NWP progress at the UKMO.

9.2. From METEOR to COMET

> Methods have been developed in this country for obtaining a forecast surface-isobaric chart by integrating the vorticity equation over time steps of about an hour using an electronic computer. Nothing further will be said about this method in this article. (M. K. Miles, 'The basis of present-day weather forecasting', *Weather* 1961)

In June 1961 the METEOR computer was moved to Bracknell. By then it had become more and more obvious that it lacked the speed and capacity to enable forecast charts to be prepared regularly within the short time interval required by the forecasters. A decision was therefore taken in 1963 to install a faster machine of greater capacity – the English-made LEO KDF9 (Sumner 1964). Most of the work in 1964 was to prepare for the new computer, already christened COMET. Staff training was started in summer 1963. Most of the testing of the machine was undertaken at Kidsgrove, near Stoke-on-Trent, either on personal visits or by courtesy of British Railways. Paper tapes and computer output were conveyed overnight via Reading in specially adapted tool-boxes.

The capacity of the new computer allowed the UKMO to extend the computational area to include the whole of Europe and the Atlantic north of latitude 30°N. The calculations would use a three-level baroclinic model with 1927 grid points in an octogonal area.[40] There were great hopes for forecast improvement of upper winds with direct application for aviation. At the Monday Evening Discussion on 21 December 1964 (*Meteorol. Mag*. 1965, pp. 156–57) P. Graystone reported that routine numerical forecasts should start when the new KDF9 computer was installed. It involved data extration, analysis and numerical forecasts. The discussion centred on the adaptation of these forecasts for operational use. It was pointed out that a computed chart carried no degree of confidence, and the forecaster would be faced with a difficult decision when it differed substantially from his own expected development.

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

[39] I spent summer 1960 at a big camping site near St Malo and can confirm there there were no British tourists in the area, except a family of five who arrived late one night in a Morris Minor. They had been allowed out of Britain, with more than the allowed amount of currency, only because the father was a war invalid. They crammed into a small single roof tent while the rain was pouring down. Although poor, wet and freezing they were always cheerful and in good spirit which impressed us Continentals in our comparatively luxurious tents.

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

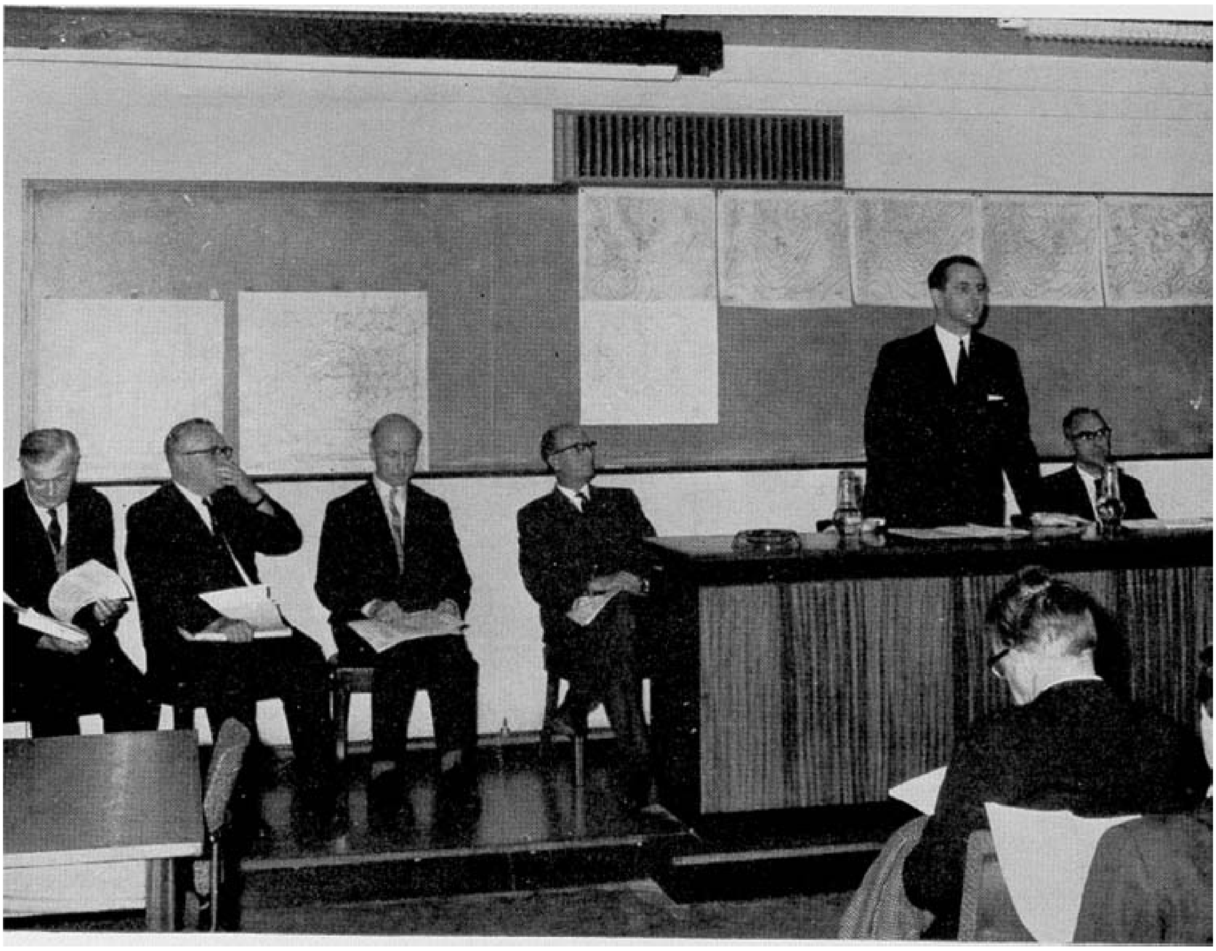

Figure 4. *One can almost sense the unease displayed by the UKMO management as they listen to John Mason during the historic press conference 2 November 1965 (Meteorol. Mag. 1966, pp. 28–30). Left to right seated: V. R. Coles, T. N. S. Harrower, J. K. Bannon and N. Bradbury (Assistant Directors) and E. Knighting (Deputy Director). The charts on the wall display the current weather situation and the numerical forecasts.*

9.3. A wind of change

> –But, Director General, suppose that the first forecast is a bad one, what then will you do?
>
> –Well, you had better make sure that is a good one!
>
> John Mason, personal communication 2003

John Mason arrived as Director General on 1 October 1965. His impression of the UKMO was that it had 'an enormous potential' but was rather bureaucratic and its staff on the operational side was lacking in confidence: 'They needed encouragement and leadership'. (Taba, 1995)

When the scientists showed him the three-level model and some of the forecasts, particularly wind and temperature forecasts for aviation (200 and 500 hPa) Mason decided to take this forecasting out of research mode and into real-time. Looking at the statistics over the North Atlantic it was clear that the results were better than from ordinary empirical forecasting: 'If we go operational we shall then have to perform at concert pitch as opposed to a rehearsal and everybody will tighten up . . . ' (Ogden, 1985, p. 36)

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

> Not only are we going operational, but we are going to make a big announcement about it. We are going to have a press conference and we shall have the press and the radio and the television down here. And they should all see our first operational numerical forecast, letting them see it coming out of the computer and giving each delegate a personal copy. (John Mason, interviewed by Ogden, 1985, p. 36)

The UKMO had never held such an event before, so many were naturally nervous of exposing 'The Office' in this way. The press conference went ahead (Figure 4) and caused big headlines:

> '£500000 computer speeds up weather forecasting – Comet feeds on isobars'

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

According to the *Daily Express*, *Guardian* and *Daily Telegraph*, Mason made clear that the human staff had 'just about' reached the limit beyond which their minds could no longer absorb information, especially not if they were to continue working against the clock with their present 'high degree of accuracy'. In a few more months the COMET was to be 'such a trusted member of the staff' that its charts will be accepted without a qualm . . . It will relieve the forecasters of a lot of donkeywork. It will eventually increase the accuracy of weather prediction.'

This was, said Mason, a landmark in the history of British weather forecasting. He stressed that, computer or no computer, weather forecasting was still almost as much of an art as a science. The computer's production of a pressure chart was only the first step. The forecasters still had to interpret, using techniques from intuition to personal experience. The British people could look forward to a steady improvement in the accuracy of forecasts (*Meteorol. Mag*. 1966, pp. 28–30).

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

The whole press conference was a great success. The real-time forecast had predicted cold winds and the first real frost for the winter. And so it was. It was also one of the best forecasts for some months afterwards. Thus, John Mason had got a flying start as Director General of the UKMO, and one of the first to congratulate him was one of his deputies, Dr Best: 'Well, I must say, Director, I thought you were taking a great risk and I didn't think you should have done that. But I must say yesterday was a great day for the Office.' It certainly was. To paraphrase David Brunt's words, 17 years earlier: British NWP had 'quite definitively got out of the doldrums'.

10. Epilogue

> The curse of group velocity. (Bo R. Döös, personal communbication spring 2004)

10.1. The 'heart' of numerical weather prediction

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

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

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

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

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

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

Both sides failed to realise that they were essentially looking at the same physical process that could be described by different mathematical formalisms: either by the concept of group velocity or advection by the wind. But in the latter case the main advection was carried out by the *upper* tropospheric wind.

10.2. Different views of the same thing

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

Independent of any theoretical models, synoptic investigations from Hovmöller (1949) and onwards agree that the transport speed is about 30° per day, which in the mid-latitudes corresponds to 2500 km/day. A maximum downstream development of 40° per day would correspond to 3000–3500 km/day. This defines the distance to the western boundary for 24-hour forecasts.[41]

According to Norman Phillips (1990, p. 23), who argues from a group velocity perspective, the state of the atmosphere at the beginning of a 24-h forecast, must be known from 3400 km to the east and to the west,[42] a total of 6800 km.

The British meteorologist Andrew Staniforth (1994, p. 44, 1997) is one of the few who has discussed the problem of the computational area in relation to limited area models in any depth. He and his co-workers found from empirical tracing experiments, independent of any group velocity considerations, that the computational area should be 6000–6500 km wide (Chouinard et al. 1994). Staniforth repeatedly made the point that the influence of information propagating inward from the lateral boundaries are 'often overlooked' by modellers: 'Care must be taken to ensure that the limited area is sufficiently large to guarantee that boundary-generated errors do not have sufficient time to reach the area of interest before the end of the forecast period.'

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

10.3. The Swedish and Japanese area definitions

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

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

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

So, what about the Swedes? Being based on the eastern (receiving) side of an ocean, they had an easy choice when defining their computational area. They could more or less take the values suggested by Charney's group velocity estimates, i.e. 53° per day upstream and 32° per day downstream. And so they did – and that could be the end of the matter.

But did they really know what they were doing? If we look at the written evidence there is nothing to suggest that they did! As a matter of fact, although there was a general knowledge about group velocity as such in the Stockholm group, they were unaware that it applied to defining the size of the NWP area (Bo Döös, personal communication 2004).

Bolin once touched upon this at an Air Force seminar in 1951:

> How fast will the conditions within one region of the map affect the development at another place? Charney has here been able to show that the effective 'signal velocity' is of the same order of magnitude as the characteristic velocity of the flow itself. This corresponds to a velocity of 15–20 m/s [or about 1500 km per day]. This is an encouraging result . . . (Bolin 1951)

Perhaps it was too 'encouraging', because 1500 km per day is a clear underestimation of the influence zone.

10.4. The interest in group velocity wanes

Arnt Eliassen, who was strongly linked to the Stockholm group, especially in the period 1951–53, some years later wrote two brief introductions to NWP where it is obvious that he has no understanding of the energy propagation process, at least not in relation to NWP areas. In a chapter written in English in

[41] Numerical aspects of lateral boundary conditions are treated in Kalnay (2003: ch. 3. 5 with references).

[42] The boundaries should be shifted westward with an amount corresponding to the mean tropopsheric wind speed.

Handbuch der Physik (Eliassen & Kleinschmidt 1957), he made a vague reference to the ENIAC calculations and to an unpublished lecture by Hinkelmann at MISU in 1951. For 'actual forecast problems' he only said that the effects are 'most strongly felt in the vicinity of the boundaries, in particular where there is strong inflow'. His only advice was to place the boundaries in dynamically less active regions such as the subtropical high-pressure belt! In his second text, in Petterssen's *Weather Analysis and Forecasts* (Eliassen 1956), he severely underestimated the effect by suggesting that boundary influences in a 24-hour forecast 'do not penetrate more than about 1000 km' into a region.

So nowhere in the written documentation from the MISU/IMI experiments were there any group velocity discussions. What might have occurred is that the Stockholm group just followed their Master. But Rossby died in 1957, the year when group velocity disappears in the meteorological science.[43]

A frequent guest at MISU was Phil D. Thompson. In 1961 he, together with Norman N. Richardson from the US Air Force, discussed the problem of how rapidly information from surrounding data rich areas would positively affect an inner large data void area – what Thompson (1961a) called a 'hole'. His mathematical model describing how the 'hole' was 'filled' did not make use of the group velocity concept. It was purely advective using the 500 hPa mid-tropospheric flow. His co-worker's barotropic simulations on BESK showed that the 'hole' was influenced and 'filled' slightly faster than Thompson's model predicted. Richardson (1961) suggested that boundary influence and 'region of influence effects' contributed to the reduction of the errors (see also Smith 1961, 1962).

During a longer stay at MISU Phil Thompson wrote his well-known textbook on NWP (Thompson, 1961b). There is a lot of valuable information in that book, but no mention of the computational area problem. James Holton, who also started to write his textbook during a stay at MISU (Holton 1972: 176–77), had no reason to go deeper into NWP. He mentioned group velocity, but only with respect to ocean waves (where the energy travels with half of the phase speed).[44]

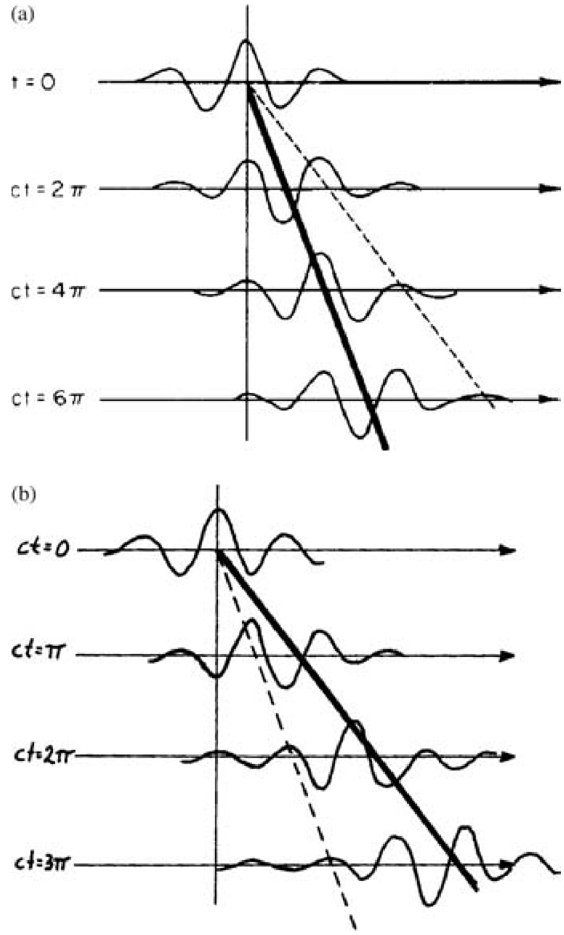

Figure 5. (***a***) *The group velocity figure from Holton (1972, 1979 and 1992) shows schematically the propagation of ocean wave groups, for which the group velocity (thick line) is half of the phase speed (dashed line).* (***b***) *The exact figure (Persson 1993, 2000) that was intended to appear in Holton's 4th edition. The group velocity (heavy line) is faster than the phase velocity (dashed line) for large-scale motion in the atmosphere. Unfortunately, the figure that was printed in the 4th edition is still not quite accurate.*

10.5. The Swedes did not make the area too large either!

There is a final twist to the story: the Swedes were doubly lucky! Not only did they define the NWP area so that it was large enough, but they also avoided making it *too large*. Due to their limited computer resources they could not, as the Americans did in the late 1950s, run their barotropic model on a hemispheric scale. Because of this they also avoided the serious problem of retrogression of the ultra-long planetary waves (Cressman & Hubert 1957; Cressman 1958; Wolff 1958). In the Swedish NWP system these waves remained quasi-stationary, locked in position by the constant boundaries.

To be fair, the Swedes knew about their fortune – indeed it was Bo Döös who told me when I was his student in 1966!

[43] See Persson (2000) for an anotated bibliography. Group velocity was 're-discovered' in meteorology in the late 1970s by two young meteorologists at Reading University (Hoskins & Simmons 1977, 1983; and Simmons & Hoskins 1979). See also Smith (1959) and Miles (1959) for an independent UKMO assessment of the process. The phenomenon of 'group velocity' as such was actually a British discovery (by Rayleigh in 1881 to explain apparent inconsistencies in Michelson-Morley's experiments to determine the speed of light).

[44] 'If there ever is a 4th edition I will certainly take your comments into account. Of course, what is really needed is a good synoptic meteorology book that would address [downstream development] instead of describing 49 different kinds of occlusions' (Letter from J. R. Holton to A. Persson, 13 March 1995).

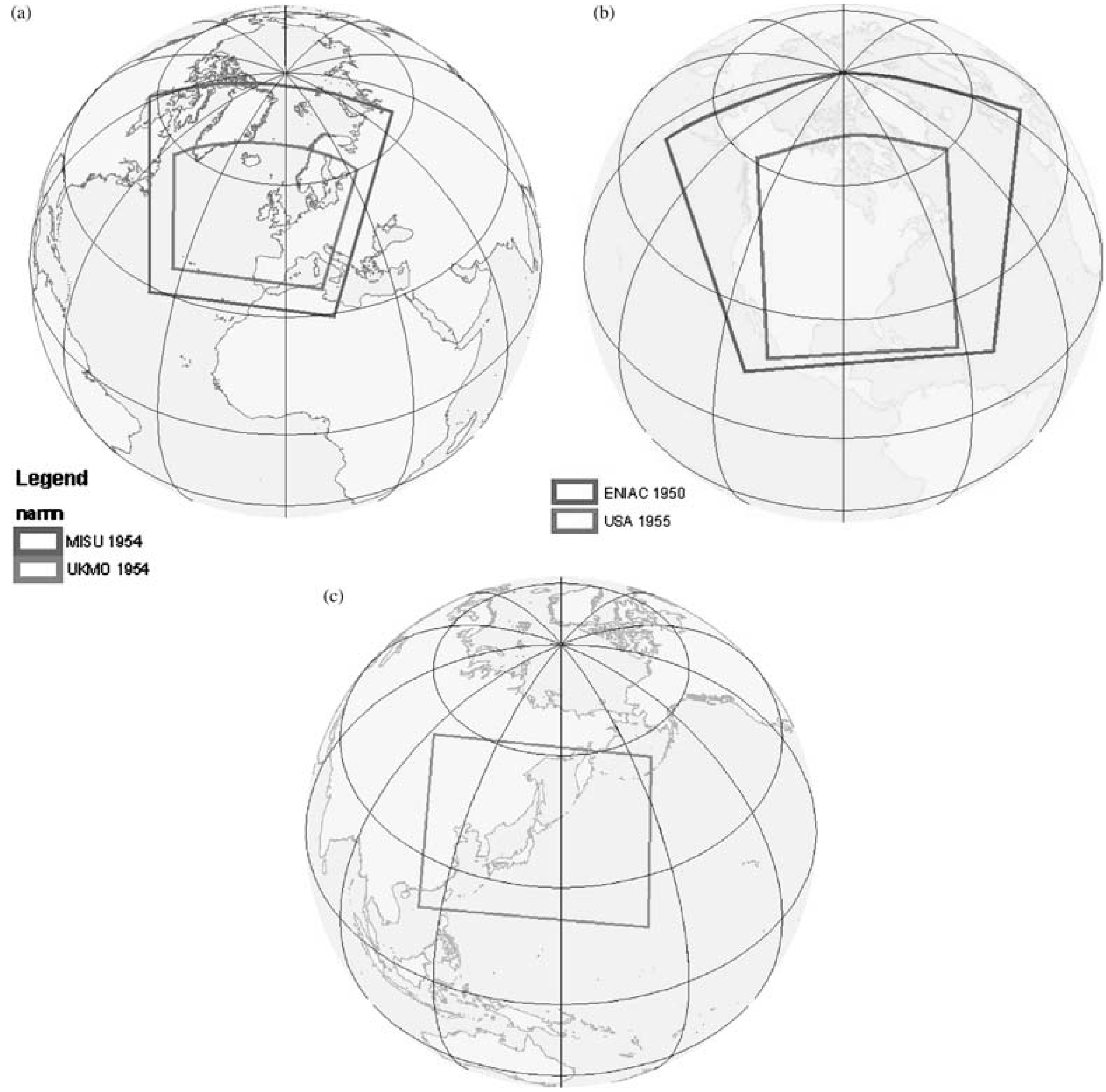

Figure 6. (***a***) *The computational areas, for which the boundary conditions were constant, for the MISU +24 hour forecast experiment in 1953–54 (Staff Members 1954) and for the UKMO +24 hour forecasts (Bushby & Hinds 1954a). While in the UKMO area (dashed line) the British Isles are just 40 longitude degrees or 2500 km away from the western boundary, in the MISU/IMI area (full line) it is more than 60 longitude degrees or 3500 km from Sweden (at 50° N). In other words, while the effects of the western boundary in order to reach the UK only have to travel with about 30 m/s in the UKMO area, they will have to travel with 40 m/s to reach Sweden in the MISU area. Charney's recommendation (Charney 1949) was to design the area after the maximum group velocity.* (***b***) *The corresponding +24 hour integreation areas for the 1950 ENIAC (outer area) runs (Charney et al. 1950), and inner area (Charney & Phillips 1953).* (***c***) *The +24 hour integration area applied by the Japanese modellers (Staff Members 1955). Being on the western side of an ocean (and the eastern side of a major continent), less energy arrives from upstream than departs downstream.*

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Appendix 1

Full text of Meteorological Research Paper 412 from 1948, UK National Meteorological Library

This report has not yet been published and is to be treated CONFIDENTIALLY. Its contents must not be quoted [later crossed over and stamped Met. Office, 5 July, 1948 Library]

M. R. P. 412
S. C. II/9
17 June 1948

Air Ministry

Meteorological Research Committee

Report on the possibilities of using electronic computing machines in meteorology

1. Two meetings were held on 25 May and 17 June 1948 to report on 'The Possibilities of Using Electronic

Computing Machines in Meteorology' which were attended by
Dr G. C. McVittie,
Dr. R. C. Sutcliffe,
C. S. Durst
E. T. Eady.

2. At the first meeting a general discussion took place at which Mr Eady stated that he hoped to pose to the Cambridge Machine simple questions on the effect of perturbations on a uniform baroclinic flow of air. He anticipated that these perturbations would develop into disturbances similar to the depressions in the atmosphere. Dr Sutcliffe argued from the forecasting point of view that it was important that actual meteorological situations should be put to the machine to discover if it were capable of solving these situations. Further discussion took place on the difficulties that would arise in posing the boundary conditions and on the limitation of accuracy due to inherent lack of precise measurement of wind. The conclusion was reached that further progress could not be made without the presence of some expert accustomed to the use of mechanical methods in computation.'
3. For the second meeting arrangements were made for the assistance of Mr. Wilkinson of the Mathematics Branch at the National Physical Laboratory.
4. At this meeting the general problem was outlined to Mr Wilkinson by Sutcliffe, and Mr Eady gave further details of a more limited problem. Mr Wilkinson stated that the problem had to be put to the machine in the same form as though it were to be solved by a very large number of Brunsvigs working for a very long time. The gain was merely speed. The memory of the NPL machines as designed was 4000 numbers. This would be sufficient for a set up of $10 \times 10 \times 10$ observations. The machine now being designed might be ready in 1950, but a pilot model with a considerably smaller memory would probably be ready about March 1949. It was possible that by methods now being developed the memory of future machines would be increased by many times and that the speed of manipulation would be increased. Mr Wilkinson did not think that the machine at present contemplated would be capable of dealing with the general problem. The more limited problem suggested by Mr Eady would be within its compass. He did not think that with its present designed speed would be able to work faster than the weather.
5. After further discussion it was agreed by the meeting that the machine as designed ought to be tested by the posing of simplified problems. It was further felt that to take a long view it was advisable that the Meteorological Office should establish contact with the computational side of mathematics in order to be prepared in the future to take advantage of the developments, which will take place. There was a strong feeling that this contact could only be obtained by someone who was familiar with the methods of computational mathematics and also synoptic meteorology.
6. There was considerable discussion as to whether it would be better for this contact to be a meteorologist who then studied computational methods or a pure mathematician versed in computational methods who then spent some considerable time learning synoptic meteorology. The latter view found most support. It was felt, moreover, that apart from the possible use of the machine, his knowledge would be extremely valuable to the UKMO on other problems.
7. The recommendations of the meeting were

   (a) That limited problems should be given to the machine as soon as it was ready to deal with them
   (b) That Mr Eady should keep the Meteorological Research Committee informed of his progress.
   (c) That the recruitment into the UKMO should include one or more mathematicians who were specially qualified in computational methods. After gaining the necessary knowledge of synoptic and dynamical meteorology such recruits would be available to undertake research into the formulation of meteorological problems in the manner suited to calculation.

Appendix 2

Rossby to Charney 25 February 1954 (from The Jule Charney papers (MC 184) in the Institute Archives and Special Collections of the Massachusett Institute of Technology Libraries, courtesy Elisabeth Andrews)

In London I attended a staff meeting (last Monday, Feb. 15) at the Met. Office, at which Bushby reviewed the five first forecasts made for 12 and 24 hours by the Dunstable group. They certainly are to be congratulated on being the first official Weather Service to undertake this, but I am a bit concerned about the direction of their work. It is of course difficult to judge from a lecture and a fleeting look at slides, but I had the impressions:

a) They must use some very peculiar boundary conditions because several of the 500 mb charts showed a most remarkable evidence of some sort of instability in the border regions (wobbly contours etc.)
b) The central portions of the 500 mb charts seemed to give quite good results
c) The predicted sea level charts appeared to be far inferior to the 500 mb charts, but again, I am not sure of this

Wednesday last week, after Joe and two Dunstable fellows had presented papers before the Met. Soc., I was asked to go back to the problem of numerical forecasting and I then made a few points which I shall state below, in the hope we may be able to discuss them when I see you in Princeton.

If you base numerical forecasting on the vorticity equation then, regardless of the number of parameters (1,2,3) you use, one and the first of your equations is a statement concerning the vertically averaged motion (as you have shown). The other statements are obtained, *in effect*, by differentiating the vorticity equation once or twice with respect to z.....

I am therefore inclined to believe *that for the time being the principal value of the higher parameter models lies in the fact that they permit us to predict the averaged (i. e. the 500 mb motion) more accurately than does the barotropic.*

In the Wednesday session I suggested that the Dunstable outfit (assuming the machine is available and is fast enough) should make its first task to issue, as a routine, during a limited period, daily 24 (or 48 hour) forecasts of the 500 mb chart, to accustom the forecasters to this forecast aid, but to consider the prediction of the sea level charts as a research project not yet ready for routine tests.

In Stockholm we are certainly going to concentrate on this approach and as soon as the drum is ready for use I hope we can go on the 48- or even 60-hour forecasts, using a larger net of observations (with a one- or two-parameter model).

There are also practical and tactical reasons for this.

1. The detailed plotting and analysis of several upper air maps, twice daily, plus the reading off of a large number of initial values + the punching of these data on tapes will take too much time that a 24-hour multi-parameter model forecast hardly can be completed earlier than, say 10 hours before the expiration of the forecast interval.
2. I sincerely fear that sea level pressure distribution forecasts, being extremely sensitive to the details of the temperature distribution and to a variety of assumptions implicit in the models, are going to expose us to gleeful criticism from the old-time school of forecasters.
3. You must not jump (skip) stages in the mental development of the forecasters on routine duty. The routine forecasters at Dunstable do not, I fear, possess much of an understanding of the 500 mb (or any other height) chart, and it would do them a lot of good to watch these charts for a long period.