10

The ENIAC integrations

The role of the enormous weather factory envisaged by Richardson (1922) with its thousands of computers will . . . be taken over by a completely automatic electronic computing machine. (Charney, 1949)

It is without question that Richardson's attempt to predict the weather by numerical means, while visionary and courageous, was premature. In his review of WPNP, Exner (1923) expressed the view that Richardson's method was unlikely to lead to progress in weather forecasting and even recommended that he should write a book on theoretical meteorology that was free from the aim of a direct application to prediction. This negative response was echoed by several other reviewers. Later, the renowned meteorologist Bernhard Haurwitz wrote in his textbook *Dynamic Meteorology* that efforts to compute weather changes by direct application of the equations was not promising, adding that

> . . . a computation of the future weather by dynamical methods will be possible only when it is known more definitely which factors have to be taken into account under given conditions and which may be neglected (Haurwitz, 1941, p. 180).

Haurwitz understood that a simplification of the mathematical formulation of the forecasting problem was required but he was not in a position to propose any specific solution. In fact, there were several obstacles preventing the fulfilment of Richardson's dream, and progress was required on four separate fronts before it could be realized.

Firstly, in order to develop a simplified system suitable for numerical prediction, a better understanding of atmospheric dynamics, and especially of the wave motions in the upper atmosphere, was required. Major insights were provided by the work of Jack Bjerknes and Reginald Sutcliffe, amongst many others, and especially by Rossby's mechanistic description — powerful in its simplicity — of atmospheric waves. This was followed by Charney's explanation of cyclonic development in terms of baroclinic instability and Kuo's work on barotropic instability. Secondly, the development of the radiosonde made observations of the free atmosphere possible in real time. A network of surface and upper air stations, established to support military operations during World War II, was developed and strengthened to serve the needs of civil aviation. Thus, it became possible to construct a comprehensive synoptic description of the state of the atmosphere. Thirdly, an understanding of the stability properties of finite difference schemes flowed from the work of Courant *et al*. in Göttingen. This provided the key to ensuring that the numerical solution was a reasonable representation of reality. And fourthly, the development of automatic electronic computing machinery provided a practical means of carrying out the monumental computational task of calculating changes in the weather.

10.1 The ‘Meteorology Project’

John von Neumann

John von Neumann was one of the leading mathematicians of the twentieth century. He made important contributions in several areas: mathematical logic, functional analysis, abstract algebra, quantum physics, game theory and the development and application of computers. A brief sketch of his life may be found in Goldstine (1993) and several biographies have been written (*e.g.*, Heims, 1980; Macrae, 1999). Von Neumann was born in Hungary in 1903. He showed outstanding intellectual and linguistic ability at an early age. After studying in Budapest, Zurich and Berlin he spent a period in the 1920s working in Göttingen with David Hilbert on the logical foundations of mathematics. In 1930 he was invited to Princeton University, and he remained at the Institute for Advanced Studies for 25 years. He died in 1957, aged only 54. In the mid 1930s von Neumann became interested in turbulent fluid flows. The non-linear partial differential equations that describe such flows defy analytical assault and even qualitative insight comes hard. Von Neumann was a key figure in the Manhattan Project which led to the development of the atom bomb. This project involved the solution of hydrodynamic problems vastly more complex than had ever been tackled before. Von Neumann was acutely aware of the difficulties and limitations of the available solution methods:

> ‘Our present analytical methods seem unsuitable for the important problems arising in connection with the solution of non-linear partial differential equations and, in fact, with all types of non-linear problems of pure mathematics. The truth of this statement is particularly striking in the field of fluid dynamics. Only the most elementary problems have been solved analytically in this field’ (in Goldstine, pp. 179–180).

Von Neumann saw that progress in hydrodynamics would be greatly accelerated if a means of solving complex equations numerically were available. It was clear that very fast automatic computing machinery was required. He masterminded the design and construction of an electronic computer at the Institute for Advanced Studies. This machine was built between 1946 and 1952 and its design had a profound impact upon the subsequent development of the computer industry. This *Electronic Computer Project* was 'undoubtedly the most influential single undertaking in the history of the computer during this period' (Goldstine, p. 255). The Project comprised four groups: (1) Engineering, (2) Logical Design and Programming, (3) Mathematical, and (4) Meteorological. The fourth group was directed by Jule Charney from 1948 to 1956.

Von Neumann was legendary for his astounding memory, his capacity for mental calculation at lightning speed and his highly developed sense of humour. Goldstine speaks of the guidance and help that he so freely gave to his friends and aquaintances, both contemporary and younger than himself. He was described by many who knew him as a man of great personal charm, and numerous anecdotes attest to his unique genius. Von Neumann and Richardson could hardly have been more different in personality or outlook. Von Neumann was socially sophisticated, extrovert and at complete ease in company, with a vast repertoire of amusing stories, limericks and jokes. Richardson was reserved and withdrawn, most comfortable when alone and even somewhat stand-offish. He once proposed listing 'solitude' as a hobby in his entry for *Who's Who* (Ashford, p. 175). Politically, the contrast was even starker: as we have seen, Richardson was a committed and immovable pacifist; von Neumann advocated preventative war, favouring a pre-emptive nuclear strike against the Soviet Union. It is fortunate that his views on this issue did not prevail.

The 'Conference on Meteorology'

Von Neumann recognized weather forecasting, a problem of both great practical significance and intrinsic scientific interest, as a problem *par excellence* for an automatic computer. His work at Los Alamos on hydrodynamic problems had given him a profound understanding of the difficulties in this area.

> Moreover, he knew of the pioneering work. . . [of] Lewis F Richardson. . . . [which] failed largely because the Courant condition had not yet been discovered, and because high speed computers did not then exist. But von Neumann knew of both (Goldstine, p. 300).

Von Neumann had been in Göttingen in the 1920s when Courant, Friedrichs and Lewy were working on the numerical solution of partial differential equations and he fully apreciated the practical implications of their findings. However, Goldstine's suggestion that the CFL criterion was responsible for Richardson's failure is wide of the mark. This erroneous explanation has also been widely promulgated by others.

Von Neumann made estimates of the computational power required to integrate the equations of motion of the atmosphere and concluded tentatively that it would be feasible on the IAS computer (popularly called the *Johnniac*). A formal proposal was made to the U.S. Navy to solicit financial backing for the establishment of a Meteorology Project. According to Platzman (1979) this proposal was ‘perhaps the most visionary prospectus for numerical weather prediction since the publication of Richardson’s book a quarter-century earlier’. Its purpose is stated at the outset (the full text is reproduced in Thompson, 1990):

> The objective of the project is an investigation of the theory of dynamic meteorology in order to make it accessible to high speed, electronic, digital, automatic computing, of a type which is beginning to become available, and which is likely to be increasingly available in the future.

Several problems in dynamic meteorology were listed in the proposal. It was clear that, even if the computer were available, it could not be used immediately. Some theoretical difficulties remained, which could only be overcome by a concerted research effort: this should be done by ‘a group of 5 or 6 first-class younger meteorologists’. The possibilities opened up by the proposed project were then considered:

> Entirely new methods of weather prediction by calculation will have been made practical. It is not difficult to estimate that with the speeds indicated. . . above, a completely calculated prediction for the entire northern hemisphere should take about 2 hours per day of prediction.

Other expected benefits were listed, including advances towards ‘influencing the weather by rational, human intervention. . . since the effects of any hypothetical intervention will have become calculable’. The proposal was successful in attracting financial support, and the Meteorological Research Project began in July, 1946.

A meeting—the Conference on Meteorology—was arranged at the Institute the following month to enlist the support of the meteorological community and many of the leaders of the field attended. Von Neumann had discussed the prospects for numerical weather forecasting with Carl Gustaf Rossby. Indeed, it remains unclear just which of them first thought of the whole idea. Von Neumann had tried to attract Rossby to the Institute on a permanent basis but succeeded only in bringing him for short visits. Having completed a brilliant PhD thesis on the baroclinic instability of the westerlies, Jule Charney stopped off on his way to Norway, to visit Rossby in Chicago. They got on so well that the three-week stay was extended to almost a year. Charney described that spell as ‘the main formative experience of my whole professional life’ (Platzman, 1990, referenced below as *Recollections*). As an inducement to Charney to stay in Chicago, Rossby arranged for him to be invited to the Princeton meeting. Charney was at that time already somewhat familiar with Richardson’s book. Richardson’s forecast was much discussed at the meeting. It was clear that the CFL stability criterion prohibited the use of a long time step such as had been used in WPNP. The initial plan was to integrate the primitive equations;

but the existence of high-speed gravity wave solutions required the use of such a short time step that the volume of computation might exceed the capabilities of the IAS machine. And there was a more fundamental difficulty: the impossibility of accurately calculating the divergence from the observations. Charney believed that Richardson's fundamental error was that

> his initial tendency field was completely wrong because he was not able to evaluate the divergence. He couldn't have used anything better than the geostrophic wind. . . [which] would have given the false divergence. I thought that maybe the primitive equations were just not appropriate (*Recollections*, p. 39).

(Recall how it was shown in Chapter 3 that the divergence of the geostrophic wind does not faithfully reflect that of the flow). Thus, two obstacles loomed before the participants at the meeting: how to avoid the requirement for a prohibitively short time step, and how to avoid using the computed divergence to calculate the pressure tendency. The answers were not apparent; it remained for Charney to find a way forward.

Jule Charney

Jule Charney (Fig. 10.1) was born on New Year's Day, 1917 in San Francisco to Russian Jewish parents. He studied mathematics at UCLA, receiving a bachelor's degree in 1938. Shortly afterwards, Jack Bjerknes organized a programme in meteorology in Los Angeles, and Charney became a teaching assistant. The great unsolved problem at that time was the genesis and development of extratropical depressions. The Norwegian scientists Bjerknes, Holmboe and Solberg had studied this problem but with inconclusive results. Charney's starting point was a wave perturbation on a zonally symmetric basic state with westerly flow and north-south temperature gradient. He reduced the problem to a second order ordinary differential equation, the confluent hypergeometric equation. He found solutions that, for sufficiently strong temperature gradients, grew with time, and thereby explained cyclonic development in terms of baroclinic instability of the basic state. His report (Charney, 1947) took up an entire issue of the *Journal of Meteorology* and was instantly recognized as of fundamental importance.

Charney's many contributions to atmospheric dynamics, oceanography and international meteorology are described in Lindzen *et al.* (1990). We focus here on his role in the emergence of numerical weather prediction. His key paper *On a physical basis for numerical prediction of large-scale motions in the atmosphere* (Charney, 1949) addresses some crucially important issues. Charney considered the means of dealing with high frequency noise, proposing a hierarchy of filtered models which we will shortly discuss. Using the concept of group velocity, he investigated the rate of travel of meteorological signals and concluded that, with the data coverage then available, numerical forecasts for one, or perhaps two, days

Fig. 10.1. Jule G. Charney (1917–1981). From the cover of EOS, Vol. 57, August, 1976 (©Nora Rosenbaum).

were possible for the eastern United States and Europe. Finally, Charney presented in this paper the results of a manual computation of the tendency of the 500 hPa height which he and Arnt Eliassen had made using the barotropic vorticity equation. To solve the associated Poisson equation, they used a relaxation method originally devised by Richardson. The results are shown in Fig. 10.2. As the verifying analysis was available only between 140°W and 20°E, we show only a detail of the figure in Charney (1949). The letters R and F indicate points of maximum *observed* height rise and fall, and R′ and F′ the corresponding points for the *computed* tendencies. The agreement is impressive. The X’s mark the extrema of vorticity advection. The centres of rise and fall are located approximately at these points.

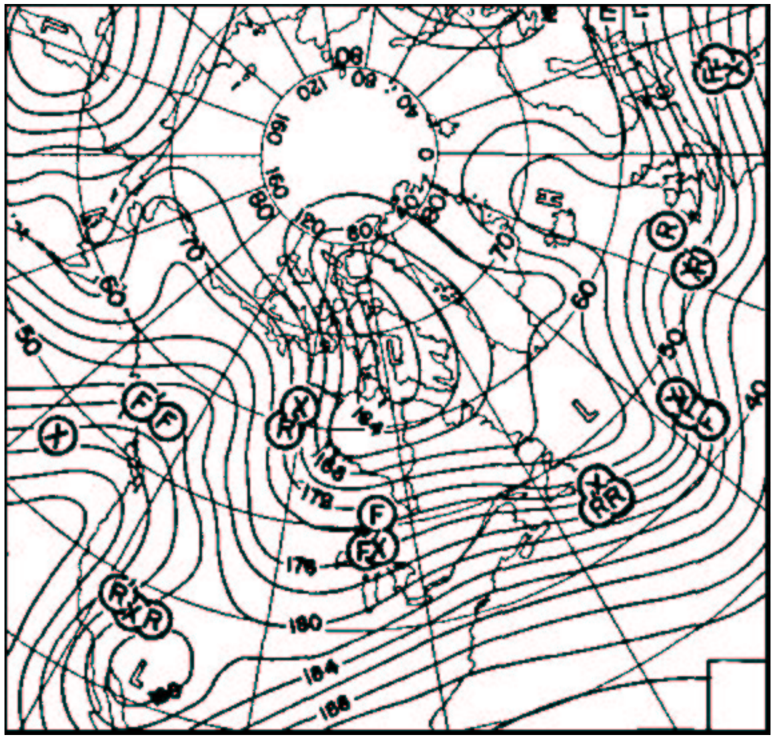

Fig. 10.2. Manual integration of the barotropic vorticity equation. R and R′ indicate points of maximum *observed* and *computed* height rise. F and F′ indicate points of maximum *observed* and *computed* height fall. X's mark the extrema of vorticity advection. (Detail of figure from Charney, 1949).

10.2 The filtered equations

In his baroclinic instability study, Charney had derived a mathematically tractable equation for the unstable waves 'by eliminating from consideration at the outset the meteorologically unimportant acoustic and shearing-gravitational oscillations' (Charney, 1947). He considered linear perturbations of infinite lateral extent and removed the high-speed waves by assuming certain quantities (related to the internal and external Froude numbers $c^2/\Re T$ and c^2/gH) were small. Thus, he had the idea of a filtering approximation at an early stage of his work. He realized that a general filtering principle was desirable:

> Such a principle would be useful for eliminating what may be called the 'meteorological noises' from the problems of motion and would thereby lead to a considerable simplification of the analysis of these problems (*loc. cit.*, p. 234).

The advantages of a filtered system of equations would not be confined to its use in analytical studies. The system could have dramatic consequences for numerical integration. The time-step dictated by the CFL criterion varies inversely with the speed of the fastest solution. Fast gravity waves imply a very short time-step; the removal of these waves leads to a far less stringent limitation on Δt.

Philip Thompson was assigned to the Meteorology Project in Princeton in the autumn of 1946. He described his experiences in his historical review of the development of numerical prediciton (Thompson, 1983). He had been working at UCLA on the Divergence Project, the objective of which was to deduce the surface pressure tendency by integrating the continuity equation, and he had become aware of a major difficulty: the horizontal divergence δ is comprised of two large terms which almost cancel, so that small errors in the reported winds cause errors in δ as large as the divergence itself (he did not mention the Dines compensation mechanism, which further complicates the calculation of surface pressure tendency). Thompson abandoned the original objectives of the Divergence Project and sought an alternative approach, in the course of which he derived a diagnostic equation for the vertical velocity—unaware that the same equation had been derived by Richardson some thirty years earlier. Thompson had met Charney when they were in Los Angeles. He wrote to him in Chicago in early 1947 asking him about some problems associated with gravity waves. In his response (dated February 12, 1947), Charney outlined his ideas about filtering the noise. First he explained that, since the primary excitation mechanisms in the atmosphere are of long period, the resulting disturbances are also of low frequency. To illustrate the situation, he employed the musical analogy quoted above (p. 136). He then sketched the derivation of the dispersion relation for the wave phase-speeds:

$$\bar{u} - c - \frac{\beta L^2}{4\pi^2} = \frac{L^2 f^2}{4\pi^2} \frac{c}{gH - (\bar{u} - c)^2} \tag{10.1}$$

This cubic equation for c had appeared in his baroclinic instability study and also, much earlier, in Rossby (1939). The three roots are given approximately by

$$c_1 \approx \bar{u} - \frac{\beta L^2}{4\pi^2}\,, \qquad c_{2,3} \approx \bar{u} \pm \sqrt{gH}\,, \tag{10.2}$$

the slow rotational wave and the two gravity waves travelling in opposite directions. ‘Since most of the energy of the initial disturbance goes into long period components, very little... will appear in the gravitational wave form’.

He then considered the question of how to filter out the noise. He drew an analogy between a forecasting model and a radio receiver, and argued that the noise could be either eliminated from the input signal or removed by a filtering system in the receiver. He described a method of filtering the equations in a particular case, but concluded ‘I still don’t know what types of approximation have to be made in more general situations'. It did not take him long to find out. In a second letter, dated November 4 the same year, he wrote:

> The solution is so absurdly simple that I hesitate to mention it. It is expressed in the following principle. Assuming conservation of entropy and absence of friction in the free atmosphere, the motion of *large-scale* systems is governed by the laws of conservation of potential temperature and potential vorticity *and* by the condition that the field of motion is in hydrostatic and *geostrophic* balance. This is the required filter!

Charney's two letters are reproduced in Thompson (1990). A full account of the filtering method was published in the paper 'On the Scale of Atmospheric Motions' (Charney, 1948); this paper was to have a profound impact on the subsequent development of dynamic meteorology. Charney analysed the primitive equations using the technique of scale analysis which we have described above (§7.2). He was able to simplify the system in such a way that the gravity wave solutions were completely eliminated. The resulting equations are known as the *quasi-geostrophic* system. The system boils down to a single prognostic equation for the quasi-geostrophic potential vorticity,

$$\left(\frac{\partial}{\partial t} + \mathbf{V}\cdot\nabla\right)\left[f + \zeta + \frac{f_0}{\rho_0}\frac{\partial}{\partial z}\left(\frac{\rho_0}{N^2}\frac{\partial p/\rho_0}{\partial z}\right)\right] = 0\,, \tag{10.3}$$

where N^2 is the Brunt-Väisälä frequency and p is the deviation from the reference pressure $p_0(z)$. The wind is assumed to be geostrophic and the vorticity is related to the pressure by $\zeta = (1/\rho_0 f)\nabla^2 p$. All that is required by way of initial data to solve this equation is a knowledge of the three-dimensional pressure field (and appropriate boundary conditions).

A filtered system quite similar in character to the quasi-geostrophic system, and subsequently christened the semi-geostrophic equations, was derived independently by Arnt Eliassen, by means of a substitution of the geostrophic approximation into the acceleration terms (this is the geostrophic momentum approximation). In the same volume of *Geofysiske Publikasjoner*, Eliassen (1949) presented the full system of equations of motion in isobaric coordinates. The idea of using pressure as the vertical coordinate has been of central importance in numerical modelling of the atmosphere.

In the special case of horizontal flow with constant static stability, the vertical variation can be separated out and the quasi-geostrophic potential vorticity equation reduces to a form equivalent to the nondivergent barotropic vorticity equation

$$\frac{d(f+\zeta)}{dt} = 0\,. \tag{10.4}$$

In fact, Charney (1949) showed that, under less restrictive assumptions, the three-dimensional forecast problem may be reduced to the solution of a two-dimensional equation for an ‘equivalent barotropic’ atmosphere. Charney began a manual integration of this equation while he was still in Norway; the results of this were shown in Fig. 10.2 above. The barotropic equation had, of course, been used by Rossby (1939) in his analytical study of atmospheric waves, but nobody seriously believed that it was capable of producing a quantitatively accurate prediction of atmospheric flow. Charney now saw it as the first member of a hierarchy of models of increasing complexity and verisimilitude (Charney, 1949). Its position as a direct specialisation of the more general quasi-geostrophic equation made it more credible for use as a first test-case of numerical weather prediction.

Charney became the leader of the Meteorology Group in mid-1948 and remained until the termination of the project eight years later. Arnt Eliassen arrived slightly later than Charney. He returned to Oslo after a year and was replaced by Ragnar Fjørtoft. By the time Charney arrived in Princeton, he had the quasi-geostrophic equations ‘in his pocket’. He also saw how to integrate them numerically: it was a matter of advecting the potential vorticity and then solving a Poisson equation for the stream-function. He felt that one should begin with the simplest model, the barotropic equation, and gradually introduce physical and mathematical factors one at a time. The intention was to progress rapidly to a baroclinic model, since the prediction of cyclogenesis was considered to be the central problem. In fact, the practical usefulness of the barotropic equation had been greatly underestimated: in *Recollections* (p. 49) he says ‘I think we were all rather surprised that the predictions were as good as they were’. We will now describe the trail-blazing work that culminated in the successful numerical integration of that simple equation.

10.3 The ENIAC integrations

In 1948 the Meteorology Group adopted the general plan of attacking the problem of numerical weather prediction by investigating a hierarchy of models of increasing complexity, starting with the simplest, the non-divergent barotropic vorticity equation. By early 1950 they had completed the necessary mathematical analysis and had designed a numerical algorithm for solving this equation. The scientific record of this work is the much-cited paper in *Tellus*, by Charney, Fjørtoft and von Neumann (1950). The authors outlined their reasons for starting with the barotropic equation: the large-scale motions of the atmosphere are predominantly barotropic; the simple model could serve as a valuable pilot-study for more complex integrations; and, if the results proved sufficiently accurate, barotropic forecasts could be utilised in an operational context. In fact, nobody anticipated the enormous practical value of this simple model and the leading role it was to play in operational prediction for many years to come (Platzman, 1979). As the IAS machine was still two years from completion, arrangements were made to run the integration on the only computer then available. The Electronic Numerical Integrator and Computer (ENIAC), which had been completed in 1945, was the first multi-purpose electronic digital computer ever built. It was installed at the Ballistic Research Laboratories at Aberdeen, Maryland. It was a gigantic contraption with 18,000 thermionic valves, massive banks of switches and large plugboards with tangled skeins of connecting wires, filling a large room and consuming some 140 kW of power. Program commands were specified by setting the positions of a multitude of 10-pole rotary switches on large arrays called function tables, and input and output was by means of punch-cards. The time between machine failures was typically a few hours, making the use of the computer a wearisome task for those operating it.

The numerical algorithm

The method chosen by Charney *et al.* to solve the barotropic vorticity equation

$$\frac{\partial\zeta}{\partial t} + \mathbf{V}\cdot\nabla(\zeta + f) = 0 \tag{10.5}$$

was based on using geopotential height as the prognostic variable. If the wind is taken to be both geostrophic and non-divergent, we have

$$\mathbf{V} = (g/f)\mathbf{k}\times\nabla z\,; \qquad \mathbf{V} = \mathbf{k}\times\nabla\psi\,.$$

The vorticity is given by $\zeta = \nabla^2\psi$. These relationships lead to the linear balance equation

$$\zeta = g\nabla\cdot(1/f)\nabla z = (g/f)\nabla^2 z + \beta u/f\,. \tag{10.6}$$

Charney *et al.* ignored the β-term, which can be shown by scaling arguments to be small. They then expressed the advection term as a Jacobian:

$$\mathbf{V}\cdot\nabla\alpha = -\frac{g}{f}\frac{\partial z}{\partial y}\frac{\partial\alpha}{\partial x} + \frac{g}{f}\frac{\partial z}{\partial x}\frac{\partial\alpha}{\partial y} = -\frac{g}{f}J(\alpha, z)\,. \tag{10.7}$$

Now using (10.6) and (10.7) in (10.5), they arrived at

$$\frac{\partial}{\partial t}(\nabla^2 z) = J\left(\frac{g}{f}\nabla^2 z + f, z\right)\,. \tag{10.8}$$

This was taken as their basic equation (Eq. (8) in Charney *et al.*). It is interesting to observe that, had they chosen the stream-function rather than the geopotential as the dependent variable, they could have used the equation

$$\frac{\partial}{\partial t}(\nabla^2\psi) = J\left(\nabla^2\psi + f, \psi\right)\,, \tag{10.9}$$

thereby avoiding the neglect of the β-term in (10.6). The boundary conditions required to solve (10.8) were investigated. It transpires that to determine the motion it is necessary and sufficient to specify z on the whole boundary and ζ over that part where the flow is inward. (The appropriate boundary conditions for (10.9) are ψ, or the normal velocity component, everywhere and ζ at inflow points).

The vorticity equation was transformed to a polar stereographic projection; this introduces a map-factor, which we will disregard here. Initial data were taken from the manual 500 hPa analysis of the U.S. Weather Bureau, discretised to a grid of 19×16 points with a grid interval corresponding to 8 degrees longitude at $45°$N (736 km at the North Pole and 494 km at $20°$N). Centered spatial finite differences and a leapfrog time-scheme were used. The boundary conditions were held constant throughout each 24-hour integration. Eq. (10.8) is equivalent to the system

$$\xi = \nabla^2 z \tag{10.10}$$

$$\frac{\partial \xi}{\partial t} = J\left(\frac{g}{f}\xi + f, z\right) \tag{10.11}$$

$$\nabla^2 \frac{\partial z}{\partial t} = \frac{\partial \xi}{\partial t} \tag{10.12}$$

Given the geopotential height, ξ follows immediately from (10.10). The tendency of ξ is then given by (10.11). Next, the Poisson equation (10.12) is solved, with homogeneous boundary conditions, for the tendency of z, after which z and ξ are updated to the next time level. This cycle may then be repeated as often as required. Time-steps of 1, 2 and 3 hours were all tried; with such a coarse spatial grid, even the longest time-step produced stable integrations.

The solution of the Poisson equation (10.12) was calculated by a Fourier transform method devised by von Neumann. This direct method was more suited to the ENIAC than an iterative relaxation method such as that of Richardson. Consider the Poisson equation $\nabla^2\varphi = F$ with φ vanishing on the boundary of a rectangular region with grid

$$\begin{aligned} x_m &= x_0 + (m/M)L_x \qquad m = 0, 1, \ldots, M \\ y_n &= y_0 + (n/N)L_y \qquad n = 0, 1, \ldots, N \end{aligned}$$

with $L_x = M\Delta s$ and $L_y = N\Delta s$. The standard five-point discretisation of the Laplacian is

$$(\nabla^2\varphi)_{mn} = (\varphi_{m+1,n} + \varphi_{m-1,n} + \varphi_{m,n+1} + \varphi_{m,n-1} - 4\varphi_{m,n}) = \Delta s^2 F_{mn}\,,$$

where Δs is the spatial interval. If φ is expanded in a double Fourier series

$$\varphi_{mn} = \sum_{k=1}^{M-1}\sum_{\ell=1}^{N-1} \tilde{\varphi}_{kl} \sin\frac{km\pi}{M} \sin\frac{\ell n\pi}{N}\,,$$

the Laplacian can be applied separately to each term

$$(\nabla^2\varphi)_{mn} = \sum_{k=1}^{M-1}\sum_{\ell=1}^{N-1}\left\{-\frac{4}{\Delta s^2}\left(\sin^2\frac{k\pi}{2M}+\sin^2\frac{\ell\pi}{2N}\right)\right\}\tilde{\varphi}_{kl}\sin\frac{km\pi}{M}\sin\frac{\ell n\pi}{N}\,.$$

We can equate this term-by-term to the expansion of F to deduce $\tilde{\varphi}_{mn}$ and then compute the inverse transform to get φ:

$$\varphi_{mn} = -\frac{\Delta s^2}{MN}\sum_{i=1}^{M-1}\sum_{j=1}^{N-1}\sum_{k=1}^{M-1}\sum_{\ell=1}^{N-1}\left(\sin^2\frac{k\pi}{2M}+\sin^2\frac{\ell\pi}{2N}\right)^{-1}$$
$$\times F_{ij}\sin\frac{ik\pi}{M}\sin\frac{j\ell\pi}{N}\sin\frac{km\pi}{M}\sin\frac{\ell n\pi}{N} \qquad (10.13)$$

This is the expansion required to solve (10.12): if we replace F_{ij} by $(\partial\xi/\partial t)_{ij}$, then $\varphi_{mn} = (\partial z/\partial t)_{mn}$.

The data handling and computing operations involved for each time step are shown in Fig. 10.3 (from Platzman, 1979). Each row indicates a program specification by setting upwards of 5000 switches (column 1), a computation (col. 2), the punching of output cards (col. 3) and manipulation of cards on off-line equipment (col. 4). Fourteen punch-card operations were required for each timestep as the internal memory of ENIAC was limited to ten registers. The first row of Fig. 10.3 represents a step forward in time. The next depicts the computation of the Jacobian. Then follow four Fourier transforms, corresponding to the four-fold summation in (10.13). The final row indicates housekeeping computations and manipulations in preparation for the next step.

The computed forecast

The story of the mission to Aberdeen was colourfully told by Platzman (1979) in his Victor Starr Memorial Lecture:

> On the first Sunday of March, 1950 an eager band of five meteorologists arrived in Aberdeen, Maryland, to play their roles in a remarkable exploit. On a contracted time scale the groundwork for this event had been laid in Princeton in a mere two to three years, but in another sense what took place was the enactment of a vision foretold by L. F. Richardson ... [about 40] years before. The proceedings in Aberdeen began at 12 p.m. Sunday, March 5, 1950 and continued 24 hours a day for 33 days and nights, with only brief interruptions. The script for this lengthy performance was written by John von Neumann and by Jule Charney, who also was one of the five actors on the scene. The other players at Aberdeen were Ragnar Fjørtoft, John Freeman, [George Platzman and] Joseph Smagorinsky....

The trials and tribulations of this intrepid troupe were described in the lecture. There were the usual blunders familiar to programmers. The difficulties were exacerbated by the primitive machine language, the requirement to set numerous

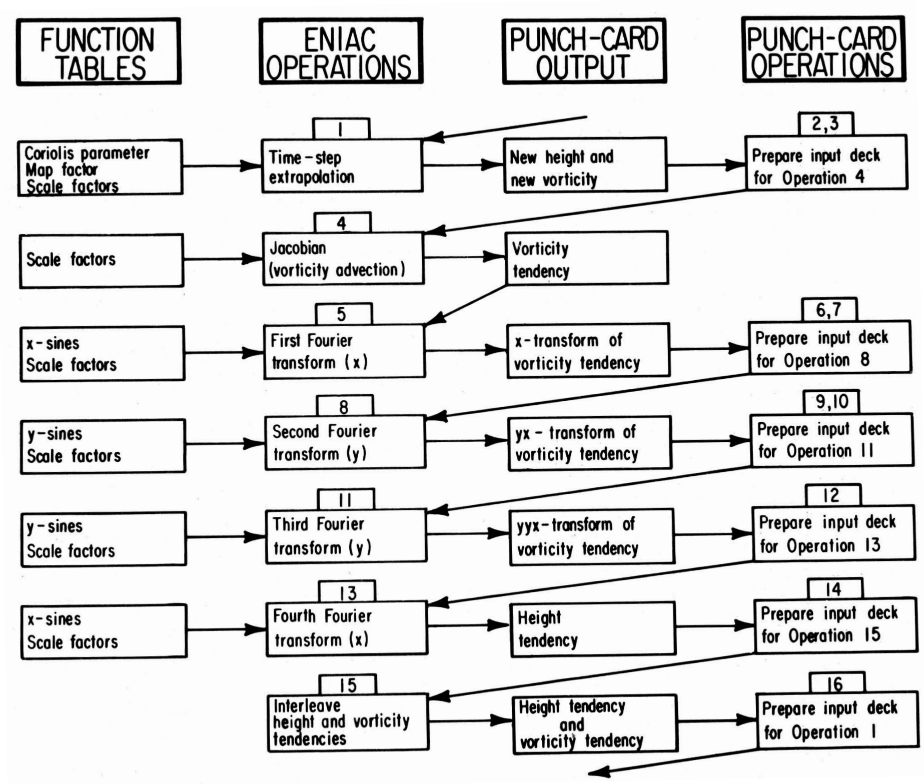

Fig. 10.3. Flow chart showing the sixteen operations required for each time step of the ENIAC forecast (from Platzman, 1979)

switches manually, the assignment of scale-factors necessitated by the fixed-point nature of ENIAC, and the tedious and intricate card-deck operations (about 100,000 cards were punched during the month). But, despite these difficulties, the expedition ended in triumph. Four 24-hour forecasts were made, and the results clearly indicated that the large-scale features of the mid-tropospheric flow could be forecast barotropically with a reasonable resemblance to reality. Each 24 hour integration took about 24 hours of computation; that is, the team were just able to keep pace with the weather. Much of the time was consumed by punch-card operations and manipulations. They estimated that when the IAS computer was ready the total elapsed time for a one-day forecast would be reduced to 1/2-hour, 'so that one has reason to hope that Richardson's dream. . . of advancing the computation faster than the weather may soon be realised' (Charney *et al.*, 1950, p. 245).

The forecast starting at 0300 UTC, January 30, 1949 is shown in Fig. 10.4. Panel *a* is the analysis of 500 hPa geopotential (thick lines) and absolute vorticity (thin

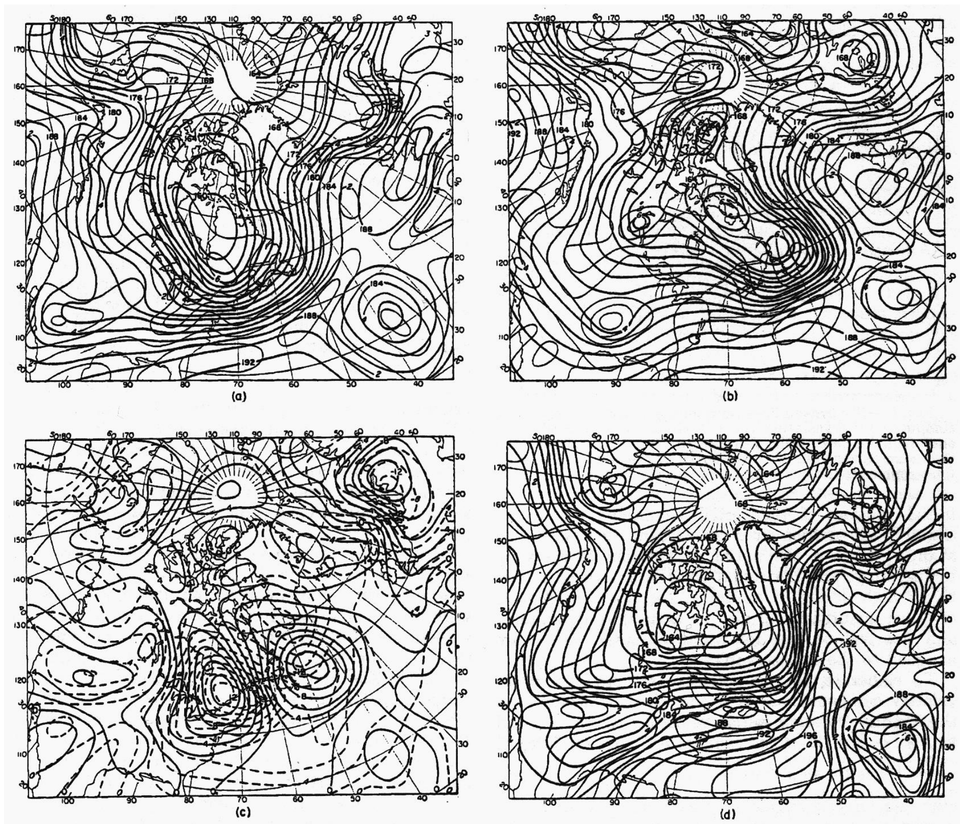

Fig. 10.4. The ENIAC forecast starting at 0300 UTC, January 30, 1949. (a) Analysis of 500 hPa geopotential (thick lines) and absolute vorticity (thin lines) (b) Corresponding analysis valid 24 hours later. (c) Observed height changes (solid lines) and predicted changes (dashed lines). (d) Forecast height and vorticity. (Charney *et al.*, 1950)

lines). Panel *b* is the corresponding analysis 24 hours later. Panel *c* shows the observed height changes (solid line) and predicted changes (dashed line). Finally, the forecast height and vorticity are shown in panel *d*. It can be seen from panel *c* that there is a considerable resemblance between the general features of the observed and forecast changes. Certainly, the numerical integration has produced a forecast that is realistic and that reflects the changes in the atmosphere.

It is gratifying that Richardson was made aware of the success in Princeton; Charney sent him copies of several reports, including the paper on the ENIAC integrations. His letter of response is reprinted in Platzman (1968). Richardson opened by congratulating Charney and his collaborators ‘on the remarkable progress which has been made in Princeton; and on the prospects for further improvement which you indicate'. He then described a 'tiny psychological experiment' on the diagrams in the *Tellus* paper, which he had performed with the help of his wife Dorothy. For each of the four forecasts, he asked her opinion as to whether the initial data (panel *a*) or the forecast (panel *d*) more closely resembled the verifying analysis (panel *b*)—in effect, whether a prediction of persistence was better or worse than the numerical prediction. His wife's opinion was that the numerical prediction was on average better, though only marginally. He concluded that the ENIAC results were 'an enormous scientific advance' on the single, and quite wrong, forecast in which his own work had ended.

10.4 The barotropic model

The encouraging initial results of the Princeton team generated widespread interest and raised expectations that operationally useful computer forecasts would soon be a reality. Within a few years research groups were active in several universities and national weather services. The striking success of the barotropic forecasts had come as a surprise to everyone. The barotropic vorticity equation simply states that the absolute vorticity of a fluid parcel is constant along its trajectory; thus, the relative vorticity ζ can change only as a result of a change in planetary vorticity f, which occurs when the parcel moves from one latitude to another. The dynamical importance of this β-effect had been clearly shown by Rossby (1939) in his linear study, and constant-absolute-vorticity or CAV trajectories were used in operational forecasting for some years, although with indifferent results. Clearly, the equation embodied the essence of mid-latitude wave dynamics but seemed too idealized to have any potential for operational use. The richness and power encapsulated in its non-linear advection were greatly underestimated.

Not everyone was convinced that the barotropic equation was useful for forecasting. The attitude in some quarters to its use for prediction seems to have been little short of antagonistic. At a discussion meeting of the Royal Meteorological Society in January, 1951 several scientists expressed strong reservations about it. In his opening remarks, Sutcliffe (1951) reviewed the application of the Rossby formula to stationary waves with a somewhat reluctant acknowledgement:

> Although the connection between non-divergent motion of a barotropic fluid and atmospheric flow may seem far-fetched, the correspondence between the computed stationary wavelengths and those of the observed quasi-stationary long waves in the westerlies is found to be so good that some element of reality in the model must be suspected.

Bushby reported that tests of Charney and Eliassen's (1949) formula for predicting 500 hPa height changes showed some success but insufficient for its use in operations. He then discussed two experimental forecasts using the barotropic equation, for which the results had been discouraging 'probably due to the fact that baroclinity has been neglected'. Finally, he described the application of Sutcliffe's (1947) development theory, showing that it had some limited success in a particular case. He concluded that much further research was needed before numerical forecasting methods could be introduced on a routine basis. Richard Scorer expressed his scepticism about the barotropic model more vehemently, dismissing it as 'quite useless':

> Even supposing that wave theory did describe the actual motion of moving disturbances in the atmosphere there is nothing at all to be gained by applying formulae derived for a barotropic model to obtain a forecast because all it can do is move on disturbances at a constant velocity, and can therefore give no better result than linear extrapolation and is much more trouble.

Sutcliffe reiterated his doubts in his concluding remarks, saying that 'when a tolerably satisfactory solution to the three-dimensional problem emerges it will derive little or nothing from the barotropic model—which is literally sterile'. These meteorologists clearly had no confidence in the utility of the single-level approach.

The 1954 Presidential Address to the Royal Meteorological Society, *The Development of Meteorology as an Exact Science*, was delivered by the Director of the Met Office, Sir Graham Sutton. After briefly considering the methods first described in Richardson's 'strange but stimulating book', he expressed the view that automated forecasts of the weather were unlikely in the foreseeable future:

> I think that today few meteorologists would admit to a belief in the possibility (let alone the likelihood) of Richardson's dream coming true. My own view, for what it is worth, is definitely against it.

He went on to describe the encouraging results that had been obtained with the Sawyer-Bushby model—making no reference to the activities at Princeton—but stressed that this work was not an attempt to produce a forecast of the weather. The prevalent view in Britain at that time was that while numerical methods had immediate application to dynamical research their use in practical foreacsting was very remote. This cautious view may well be linked to the notoriously erratic nature of the weather in the vicinity of the British Isles and the paucity of data upstream over the Atlantic Ocean.

A more sweeping objection to the work at Princeton was raised by Norbert Wiener who, according to Charney, viewed the whole project with scorn, saying that the meteorological group were 'trying to mislead the whole world in[to] thinking that one could make weather predictions as a deterministic problem' (*Recollections*, p. 57). Charney felt that, in some fundamental way, Wiener was right and that he had anticipated the difficulty due to the unpredictability of the atmosphere, which was first considered in detail by Thompson (1957) and elucidated in a simple context by Lorenz (1963).[1] It is now generally accepted that there is indeed

[1] The predictability of the atmopshere is discussed in §11.6 below.

an inherent limit to the useful range of deterministic forecasts but in relation to short-range forecasting Wiener's view was unnecessarily gloomy.

Despite various dissenting views, evidence rapidly accumulated that even the rudimentary barotropic model was capable of producing forecasts comparable in accuracy to those produced by conventional manual means. In an interview in 1988, Ragnar Fjørtoft described how the success of the ENIAC forecasts 'had a rather electrifying effect on the world meteorological community'. When he returned to Norway in 1951, Fjørtoft was without access to computing machinery. Anxious to exploit the potential of the numerical methods for routine forecasting, he developed a graphical method of integrating the barotropic vorticity equation (Fjørtoft, 1952). His idea was to advect the absolute vorticity, using a smoothed velocity field that allowed a long time-step, and to solve a Helmholtz equation for the stream-function. All operations were performed graphically, using maps drawn on tracing paper. A 24-hour forecast could be calculated in a single time-step, the whole process taking less than three hours. The Princeton forecasts were re-done by the graphical method, and the results were of comparable accuracy to those using ENIAC. Fjørtoft's method was used for a time in 1952 and 1953 in several U.S. Air Force forecast centres. Cressman (1996) regarded it as 'the first known operational use of numerical weather prediction'. Although this manual method was soon superceeded by computer forecasts, it is historically important in that it links back to the graphical methods first proposed by Vilhelm Bjerknes and also forward to current methods: Fjørtoft seems to have been the first to employ the Lagrangian advection method which is so popular today.

The first computer integrations that were truly predictions, based on recent observations and available in time for operational use, were made in Stockholm in November, 1954 (Persson, 2005a). Rossby had returned to Sweden in 1947 but maintained close links with the Princeton team, sending two of his students, Roy Berggren and Bert Bolin to the Institute for Advanced Studies. He was so impressed by the success of the ENIAC experiments that he set up a collaborative project between the newly-founded International Meteorological Institute at Stockholm University and the Royal Swedish Air Force, to carry the work over to an operational context. A barotropic model was integrated out to 72 hours on a Swedish computer, BESK, similar to the Princeton machine. The results of these trials were very encouraging: the average correlation between computed and observed 24-hour changes was significantly better than that obtained with conventional methods. When an automatic analysis scheme had been developed (Bergthorsson and Döös, 1955), regular operations were initiated in 1956. Col. Herrlin (RSAF), reporting these results at a Symposium on NWP in Frankfurt in May, 1956, described the situation thus:

> For the last 10 years, or so, the progress made in the technique of 1–2 days

> forecasts has been very small. The development appears to have become almost stagnant. I believe we have literally squeezed the conventional technique dry. . . . When therefore Prof. Rossby told me that he was convinced that a practicable program for numerical forecasts of the 500 mb surface now was available, we within the Swedish Military Weather Service were eager to develop and test this system in our general routine. As has been shown, our experiences have been most favourable and I feel convinced that this is only the modest beginning of a new era ... comparable with the era created by the Norwegian school in the nineteen twenties (Herrlin, 1956).

The Swedish work represented the inauguration of the era of operational objective forecasting based on scientific principles. It is reviewed by Persson (2005a) and by Wiin-Nielsen (1997). The humble barotropic vorticity equation continued to provide useful guidance for almost a decade.

10.5 Multi-level models

On Thanksgiving Day, 1950 a severe storm caused extensive damage along the east coast of the United States (Smith, 1950). The prediction of rapid cyclogenesis events like this had long been considered as the central problem in synoptic meteorology. The simple barotropic model is incapable of representing such explosive deepening, as it does not allow for the energy transformations that are crucial for such developments. Charney (1947) and Eady (1949) had elucidated the role of baroclinic instability in cyclogenesis. It was clear that the prediction of this phenomenon required a numerical model that accounted for vertical variations and allowed for conversion of available potential energy to kinetic energy. Several baroclinic models were developed in the few years after the ENIAC forecast (Árnason, 1952; Charney and Phillips, 1953; Eady, 1952; Eliassen, 1952; Phillips, 1951; Sawyer and Bushby, 1953). They were all based on the quasi-geostrophic system of equations. The Princeton team studied the Thanksgiving storm using two- and three-level models. After some tuning, they found that the cyclogenesis could be reasonably well simulated. Thus, it appeared that the central problem of operational forecasting had been cracked.

These results were instrumental in persuading the Air Weather Service, the Naval Weather Service and the U.S. Weather Bureau to combine forces and establish the Joint Numerical Weather Prediction Unit (JNWPU). The unit came into being in July, 1954 with George Cressman as Director, Joseph Smagorinsky as head of the operational section, Philip Thompson as head of the development section and Art Bedient as head of the computer section, and regular operations began about one year later. The first experimental model was the three-level model of Charney and Phillips. However, it transpired that the success of the Thanksgiving forecast had been something of a fluke. Shuman (1989) reports that the multi-level models were consistently worse than the simple barotropic equation. As a result, the single-level model was used for operations from 1958. There was an immediate improvement in forecast skill over subjective methods, but this had come only after intensive efforts to remove some deficiencies that had been detected in both the barotropic and multi-level models. A spurious anticyclogenesis problem was solved by replacing geostrophic initial winds by non-divergent winds derived using the balance equation (Shuman, 1957), and spurious retrogression of the longest waves was suppressed by allowing for the effects of divergence (Cressman, 1958). In fact, both these problems had also been noted by the Stockholm group and similar solutions had already been devised by Bolin (1956).

The grid resolution of the first operational model at JNWPU was 381 km (Wiin-Nielsen, 1997). This number, which may appear strange, was chosen for a practical reason. If the map-scale is $1 : M \times 10^6$ (one to M million) and the grid size in kilometres is Δ, then the distance d between grid points on the map is $d = \Delta/M$ millimetres. However, the line-printers used for displaying the zebra-chart plots were designed in imperial units. The distance *in inches* between grid points is $d = \Delta/(25.4 \times M)$. Thus, $\Delta = 381$ km gave $d = 1.5''$ on a one-to-ten-million map. There were ten print characters per inch and six lines per inch; the location of grid points at print points removed the need for numerical interpolation. Thus, model resolutions of 127 km, 254 km and 381 km were common for early models[2]. Indeed, these resolutions continued to be used even after line-printers were replaced by high-resolution plotters. The grid resolution of the early models had to be very coarse to ensure adequate geographical coverage for a one-day forecast. In his monograph *Dispersion Processes in Large-scale Weather Prediction*, Phillips wrote:

> 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 (Phillips, 1990).

Phillips observed that it was fortunate that Charney had applied group velocity concepts so that a reasonable decision could be made about the minimum forecast area and that, had the region been too small, the ENIAC results ‘might have been as discouraging as was Richardson’s attempt 30 years earlier’. The additional computational demands of multi-level models meant that the geographical coverage was more limited. This increased the risk of corruption of the forecast by errors propagating from the lateral boundaries, where the variables retained their initial values. Persson (2006) argues that this was the reason why the early baroclinic model results in Britain were unsatisfactory.

The size and scope of the International Symposium on Numerical Weather Prediction that was organized in Frankfurt in 1956 indicates the state of play at that

[2] The grid unit of 381 km was popularly called a *Bedient*, after Art Bedient who first thought up the idea.

time. There were over 50 participants from USA, from Japan and from eleven European countries. Some 27 contributions are contained in the report (DWD, 1956), including several from the German pioneers, Hinkelmann, Hollman, Edelmann and Wippermann (known colloquially as *Die Viermännergruppe* or simply *Die Männer*, The Men). Although operational NWP was not introduced in Germany until 1966, there were significant theoretical developments from much earlier. As there was no access to computers, the first integrations were done manually, using a filtered three-level baroclinic model. In an interview (Taba, 1988) Winardt Edelmann tells the story of some early work in the Autumn of 1952. First, the geopotential analysis at three levels was prepared by the synopticians.

> Then the vorticity and even the Jacobians of the quasi-geostrophic model had to be evaluated by graphical addition and subtraction and more elaborate methods, producing a whole lot of maps; that took several days and was not very precise. Then a square grid was placed over the Jacobian maps and values interpolated for each grid point. ... [A]fter several weeks we had figures we assumed to be the solution to the elliptic equation. The tendency was converted back to a map and graphically added to the initial field, giving us a forecast for 12 hours. Then the entire operation had to be repeated to give a 24-hour prediction. The result did not look totally unreasonable.

In 1955, George Platzman conducted a worldwide survey to assess the level of activity in numerical weather prediction. A report on the results was distributed the following year (Birchfield, 1956). Numerical methods were already under active investigation in USA, Britain, Sweden, Germany and Belgium. Objective graphical techniques were in use or under study in USA, Japan, Ireland and New Zealand. Preliminary activities or immediate plans were reported by Canada, Finland, Israel, Norway and South Africa. Of course, this survey could not be complete. There were also activities at an early stage in Australia, France, Russia and elsewhere. Persson (2005b, 2006) has reviewed early operational numerical weather prediction outside the USA, with particular attention to developments in Britain.

By 1960, numerical prediction models based on the filtered equations were either operational or under investigation at several national weather centres. Baroclinic models were being developed, but they did not yield dramatic improvements over the barotropic models. There were inherent shortcomings due to the approximations implicit in the filtered equations. The assumption of geostrophy gives rise to errors associated with the variation of the Coriolis parameter with latitude. An assumption of quasi-nondivergence would have circumvented this problem to some extend. In a review of early numerical prediction, Phillips (2000) wrote: ‘I believe that baroclinic quasi-geostrophic models might have been more productive ... if they had used a stream function in place of the geopotential’. While this may be true, there were other severe limitations with the filtered equations, one of which was their inapplicability in the tropics. To overcome these difficulties, a return to the method originally employed by Richardson was necessary.

10.6 Primitive equation models

The limitations of the filtered equations were recognized at an early stage. In a forward-looking synopsis in the *Compendium of Meteorology*, Jule Charney wrote:

> The outlook for numerical forecasting would indeed be dismal if the quasi-geostrophic approximation represented the upper limit of attainable accuracy, for it is known that it applies only indifferently, if at all, to many of the small-scale but meteorologically significant motions (Charney, 1951).

Charney discussed some integrations that he had performed with John Freeman using a linear barotropic primitive equation model. The computed motion was found to consist of two superimposed parts, a Rossby motion and gravity wave motion of much smaller amplitude:

> In a manner of speaking, the gravity waves created by the slight unbalance served the telegraphic function of informing one part of the atmosphere what the other part was doing, without themselves influencing the motion to any appreciable extent (Charney, 1951).

He considered the prospects for using the primitive equations, and argued that if geostrophic initial winds were used, the gravity waves would be acceptably small. Within a year or so of arriving at Princeton, Charney had realized that it would be possible to integrate the primitive equations provided the CFL stability criterion were satisfied. There would be gravity wave oscillations in the solution but their amplitude would remain bounded: ‘It would give you an embroidered tendency field which would be essentially correct. In other words, the primitive equations would be quite possible’ (Platzman, 1990 [*Recollections*] p. 38). This indicates a volte-face from the view that he had formed when he first studied Richardson’s book: ‘I thought that maybe ... the primitive equations were just not appropriate’ (*Recollections*, p. 39). In his Compendium article, Charney outlined a scheme for solving the primitive equations, but cautioned that, in the last analysis, the feasibility of using them would be determined only by actual numerical integrations.

In a letter to Platzman, Charney wrote that he had been ‘greatly encouraged’ by Richardson’s generous remarks on the first numerical forecasts on ENIAC, and had subsequently sent him a report on the baroclinic integrations:

> Sad to say... it arrived five days after his death. I wish now I had earlier sent him an article I wrote for the *Compendium of Meteorology* in which, at the end, I came to the conclusion that his approach was perfectly feasible despite the initial-value problem providing only that one was careful to satisfy the condition of computational stability. Of course, his work needs no vindication from me (Platzman, 1968).

Research with the primitive equations began at NMC (now NCEP) in 1959. A six-level primitive equation model was introduced into operations in June, 1966, running on a CDC 6600 (Shuman and Hovermale, 1968). There was an immediate improvement in skill: the S_1 score (Teweles and Wobus, 1954) for the 500 hPa one-day forecast was improved by about five points. Platzman (1967) made a detailed comparison between the Shuman-Hovermale model and Richardson's model. While there were significant differences, the similarities were more striking. Even the horizontal and vertical resolutions of the two models were quite comparable. This is all the more surprising as the NMC model was not designed by consciously following Richardson's line of development but had evolved from the earlier modelling work at Princeton, together with Eliassen's (1949) formulation of the equations in isobaric coordinates.

Karl-Heinz Hinkelmann had been convinced from the outset that the best approach was to use the primitive equations. He knew that they would simulate the atmospheric dynamics and energetics more realistically than the filtered equations. Moreover, he felt certain, from his studies of noise, that high frequency oscillations could be controlled by appropriate initialization. His 1951 paper 'The mechanism of meteorological noise' was the first systematic attempt to tackle the issue of suitable initial conditions. In it, he argued that geostrophic winds would yield a forecast substantially free from high frequency noise. Furthermore, the extra computation, necessitated by shorter time-steps, to integrate the primitive equations would be partially offset by the simpler algorithms, which involved no iterative solution of elliptic equations. His first application of the primitive equations was a success, producing good simulation of development, occlusion and frontal structure. In an interview for the WMO Bulletin he said:

> On my first attempt, using idealized initial data, I got a most encouraging result which reproduced new developments, occlusions and even the kinks in the isobars along a front (Taba, 1988).

Soon after they had done that first run with the primitive equations, Hinkelmann and his team visited Smagorinsky in Washington, D.C.

> After seeing our results, he [Smagorinsky] said that we had done a fine job, but added that his group also had good results with the primitive equations and intended to use them exclusively from that time on. So in fact our independent research efforts had both led to the same conclusion. I consider that the change from quasi-geostrophic models to primitive equations was a very important step in simulating atmospheric processes.

Routine numerical forecasting was introduced in the Deutscher Wetterdienst in 1966; according to Reiser (2000), this was the first ever use of the primitive equations in an operational setting. In an interview in November, 1987, André Robert was asked if improvements in numerical weather prediction had been gradual or if he knew of a particular change of model that produced a dramatic improvement.

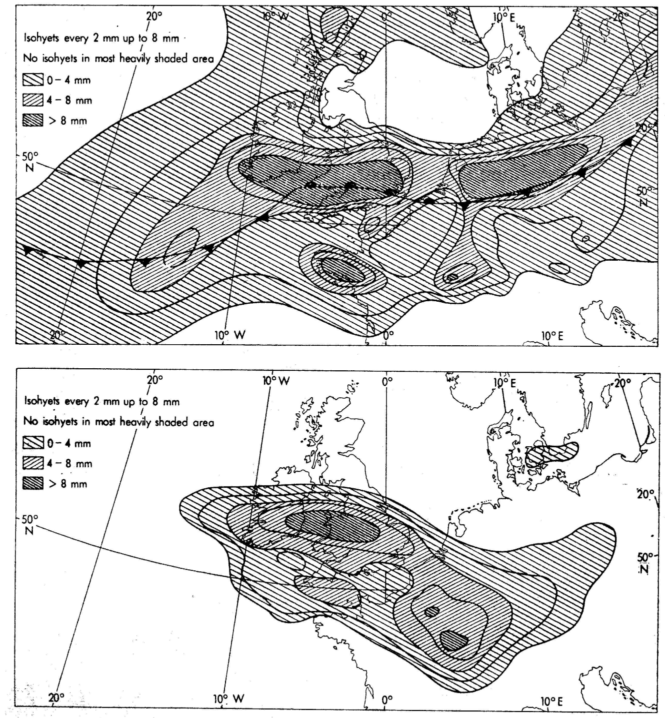

Fig. 10.5. Top panel: estimated total rainfall for 06–18 UTC on 1 December, 1961, based on weather reports. Bottom panel: forecast total rain for the same period based on the Bushby-Timpson model (from Benwell *et al.*, 1971).

He replied that with the first primitive equation model in Washington there were drastic improvements, and the decision was made immediately to abandon filtered models for operational forecasting (Lin *et al.*, 1997).

The first primitive equation models (Smagorinsky, 1958; Hinkelmann, 1959) were adiabatic, with dry physics. The introduction of moisture brought additional serious problems. Conditional instability leads to the rapid development of small-scale convective systems, called grid-point storms, and larger synoptic-scale depressions and tropical cyclones are starved of the energy necessary for their growth. To rectify this problem, convective instability must be reduced throughout the unstable layer. In the early diabatic models this was achieved by a process called *moist convective adjustment*, which suppresses gravitational instability. Kasahara (2000) has written an interesting history of the development of cumulus parameterizations for NWP in which he argues that cumulus schemes were a critical factor in enabling stable time integrations of primitive equation models with moist physical processes.

We have seen that the view in Britain was that single-level models were unequal to the task of forecasting. As a result, barotropic models were never used for forecasting at the UK Met Office and, partly for this reason, the first operational model (Bushby and Whitelam, 1961) was not in place until the end of 1965, more than ten years after operational NWP commenced at JNWPU in Washington. In 1972 a ten-level primitive equation model (Bushby and Timpson, 1967) was introduced. This model incorporated a sophisticated parameterisation of physical processes including heat, moisture and momentum through the bottom boundary, topographic forcing, sub-grid-scale convection and lateral diffusion. Useful forecasts of precipitation were produced. An example of one such forecast is shown in Fig. 10.5: the top panel shows the estimated total rainfall for 06–18 UTC on 1 December, 1961, based on weather reports; the bottom panel shows the forecast total rain for the same period. The maximum over Southern Ireland and Britain is reasonably well predicted. The maximum over Northern Germany is poorly reflected in the forecast. The results indicate that the model was capable of producing a realistic rainfall forecast.

Despite the initial hesitancy in Britain to give credance to computer forecasts, the following account appeared in a popular exposition of numerical forecasting in the late 1970s:

> It was the Meteorological Office of Great Britain, Richardson's own country, that was the first weather-forecasting service of any country in the world to acquire a computer, the IBM 360/195, big enough to carry out regular weather prediction with a highly detailed and refined process along ... [Richardson's] lines (Lighthill, 1978).

Such hyperbole is superfluous: the Met Office has continued to hold a leading position in the ongoing development of numerical weather prediction. A 'Unified Model', which may be configured for global, regional and mesoscale forecasting and as a general circulation model for climate studies, was introduced in 1993 (Cullen, 1993) and continues to undergo development (see http://www.metoffice.gov.uk). The forecast version, currently running on a NEC SX-8/128M16 computer system with a total of 128 processing elements, is now the basis of operational weather prediction at the Met Office. The model is also the primary resource for climate modelling at the Hadley Centre, a leading centre for the study of climate and climate change.

10.7 General circulation models and climate modelling

Norman Phillips carried out the first long-range simulation of the general circulation of the atmosphere. He used a two-level quasi-geostrophic model on a beta-plane channel with rudimentary physics. The computation, done on the IAS computer (MANIAC I), used a spatial grid of 16×17 points, and the simulation was

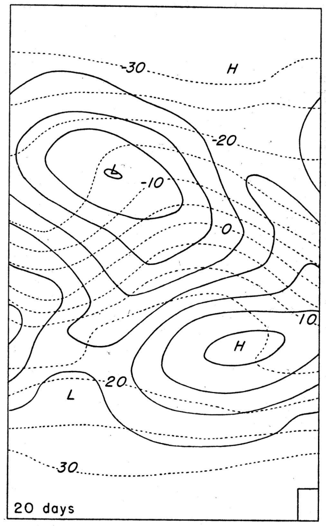

Fig. 10.6. Configuration of the flow after 20 days simulation with a simple, two-level filtered model. Solid lines: 1000 hPa heights at 200 foot intervals. Dashed lines: 500 hPa temperatures at 5°C intervals (Phillips, 1956).

for a period of about one month. Starting from a zonal flow with small random perturbations, a wave disturbance with wavelength of 6000 km developed. It had the characteristic westward tilt with height of a developing baroclinic wave, and moved easward at about 20 m s^{-1}. Fig. 10.6 shows the configuration of the flow after twenty days simulation. Phillips examined the energy exchanges of the developing wave and found good qualitative agreement with observations of baroclinic systems in the atmopshere. He also examined the mean meridional flow, and found circulations corresponding to the Hadley, Ferrel and Polar cells:

> We see the appearance of a definite three-celled circulation, with an indirect cell in middle latitudes and two somewhat weaker cells to the north and south. This is a characteristic feature of … unstable baroclinic waves (Phillips, 1956, p. 144).

John Lewis has re-examined Phillips' experiment and the circumstances that led up to it (Lewis, 1998). Phillips presented this work to a meeting of the Royal Meteorological Society, where he was the first recipient of the Napier Shaw Prize. The leading British dynamicist Eric Eady said, in the discussion following the presentation, 'I think Dr Phillips has presented a really brilliant paper which deserves detailed study from many different aspects'. Von Neumann was also hugely impressed by Phillips' work, and arranged a conference at Princeton University in October 1955, *Application of Numerical Integration Techniques to the Problem of the General Circulation*, to consider its implications. The work had a galvanizing effect on the meteorological community. Within ten years, there were several major research groups modelling the general circulation of the atmosphere, the leading ones being at the Geophysical Fluid Dynamics Laboratory (GFDL), the National Center for Atmospheric Research (NCAR) and the Met Office.

Following Phillips' seminal work, several general circulation models (GCMs) were developed. One early model of particular interest is that developed at NCAR by Kasahara and Washington (1967). A distinguishing feature of this model was the use of height as the vertical coordinate (most models used pressure p or normalized pressure σ). The vertical velocity was derived using Richardson's Equation; indeed, the dynamical core of this model was very similar to that employed by Richardson. The Kasahara-Washington model was a simple two-layer model with a 5° horizontal resolution. It was the first in a continuing series of climate models. Various physical processes such as solar heating, terrestrial radiation, convection and small-scale turbulence were included in these models. The Community Atmosphere Model (CAM 3.0) is the latest in the series. CAM also serves as the atmospheric component of the Community Climate System Model (CCSM) a 'fully-coupled, global climate model that provides state-of-the-art computer simulations of the Earth's past, present, and future climate states.' Thanks to enlightened American policy on freedom of information, these models are available to the weather and climate research community throughout the world, and can be downloaded from the NCAR web-site without cost (www.ncar.ucar.edu).

A declaration issued at the World Economic Forum in Davos, Switzerland in 2000 read: *Climate change is the greatest global challenge facing humankind in the twenty-first century*. There is no doubt that the study of climate change and its impacts is of enormous importance for our future. Global climate models are the best means we have of anticipating likely changes. The latest climate model (HadCM3) at the Hadley Centre is a coupled atmosphere-ocean general circulation model. Many earlier coupled models needed a flux adjustment (additional artificial heat and moisture fluxes at the ocean surface) to produce good simulations. The higher ocean resolution of HadCM3 was a major factor in removing this requirement. To test its stability, HadCM3 has been run for over a thousand years simulated time and shows minimal drift in its surface climate. The atmospheric component of HadCM3 has 19 levels and a latitude/longitude resolution of $2.5^\circ \times 3.75^\circ$, with grid of 96×73 points covering the globe. The resolution is about 417×278 km at the Equator. The physical parameterisation package of the model is very sophisticated. The radiative effects of minor greenhouse gases as well as CO_2, water vapour and ozone are explicitly represented. A parameterization of background aerosol is included. The land surface scheme includes freezing and melting of soil moisture, surface runoff and soil drainage. The convective scheme includes explicit down-draughts. Orographic and gravity wave drag are modelled. Cloud water is an explicit variable in the large-scale precipitation and cloud scheme. The atmospheric component of the model allows the emission, transport, oxidation and deposition of sulphur compounds to be simulated interactively. The oceanic component of HadCM3 has 20 levels with a horizontal resolution of $1.25° \times 1.25°$ permitting important details in the oceanic current structure to be represented. The model is initialized directly from the observed ocean state at rest, with a suitable atmospheric and sea ice state. HadCM3 is being used for a wide range of climate studies which will form crucial inputs to the forthcoming Fourth Assessment Report (AR4) of the Intergovernmental Panel on Climate Change (IPPC), to be published in 2007.

The development of comprehensive models of the atmosphere is undoubtedly one of the finest achievements of meteorology in the twentieth century. Advanced models are under continuing refinement and extension, and are increasing in sophistication and comprehensiveness. They simulate not only the atmosphere and oceans but also a wide range of geophysical, chemical and biological processes and feedbacks. The models, now called *Earth System Models*, are applied to the eminently practical problem of weather prediction and also to the study of climate variability and mankind's impact on it.