The Start of Numerical Weather Prediction in the United States

By Norman A. Phillips 1 September 1999 (Revised 1/20/20)

Abstract

Approximately 13 years of activity in the United States is reviewed, beginning with initial attempts to interest J. von Neumann in using his forthcoming computer for numerical weather prediction, and ending with 3 years of operational prediction in Washington. Input from five leading figures is emphasized, each enthusiastic about the possibility and possessing special scientific and administrative talents. Several major meteorological problems were encountered in the early operational years; the failure of Charney's 3-level model, and the unexpected retrogression of long waves. In several situations, optimistic theoretical predictions of success were realized, but for reasons unknown at the time those predictions were made.

A great improvement is demonstrated in forecasts of the November 1950 Appalachian storm with modern analysis and prediction systems.

1. The initial steps.

Operational numerical weather prediction in the United States began in April 1955, when the Joint Numerical Weather Prediction Unit (JNWP) made its first forecast. But

the incubation period for this event began ten years earlier, with input and support from four outstanding people. I will start with the youngest person, Jule Charney, and refer to the other three as the story unfolds. My recitation will be organized chronologically, but I will take this opportunity to comment on two interesting meteorological aspects and problems of the first years of operational work.

Jule Charney (Figure 1) was the person most responsible for the initial success of numerical weather prediction. As a graduate student in mathematics at the University of California at Los Angeles, he changed to meteorology during World War II, and studied under J. Bjerknes, Jørgen Holmboe, and Morris Neiburger. His 1946 doctoral thesis was a hydrodynamical analysis of wave instability in the baroclinic westerlies. The mathematics was beyond the knowledge of almost all dynamical meteorologists of that day. Figure 2 shows a cartoon in the UCLA student newspaper at the time. It records a glimpse of Charney conversing with a fellow meteorology student, possibly the evening after Jule had derived his final differential equation!

Before Charney arrived at a recognizable differential equation, he had to make certain specific approximations. These later turned out to be equivalent to the assumption of hydrostatic and geostrophic flow. All of this development in his thesis was completely his; he received no help from the faculty at UCLA. (Wurtele, 1990; Platzman, 1990; Phillips, 1994)

However, numerical weather prediction was not yet on his mind. This changed quickly when he came under the influence of Carl-Gustav Rossby in 1946 at the University of Chicago. (Figure 1) Jule was then on his way to a fellowship in Norway, when Rossby talked him into spending much of the 1946-7 school year at Chicago. Jule later said that this short period with Rossby was the most formative period of his life.

Rossby had started his meteorological career at Bergen after World War I when the Norwegian frontal model was being formulated by J. Bjerknes. He then spent several years as a forecaster in the Swedish Hydrometeorological Service and went to sea three times as a meteorologist and forecaster, before immigrating to the United States in 1925. (Byers, 1960; Bergeron, 1959; Phillips, 1998) Throughout his life Rossby had a strong interest in improving weather forecasts, and many of the theoretical developments for which he is best known had weather prediction as a stimulus. There can be little doubt that he influenced Charney by showing how theory could be applied successfully to the challenging problem of prediction.

But as yet, there was no feasible way to continue the path that L. F. Richardson had started 25 years earlier; both theory and computational power were missing. Both items were about to change. In August 1946, early in Jule's stay in Chicago, Rossby arranged for Charney to accompany him to a meeting at the Institute for Advanced Study in Princeton, New Jersey. This meeting was called by John von Neumann (Figure 1) to discuss the work of a small meteorological group that he had assembled, with government support, at the suggestion of Rossby. (Aspray, 1990, pp. 130-133.) von Neumann had emigrated from Europe in 1930, at the age of 27 as a Hungarian mathematician of genius stature. He was a Permanent Member of the Institute for Advance Study in Princeton, along with others such as Albert Einstein. From his initial interests in quantum theory and the theory of games, he had become interested in the theory of computers and other automata. With financial support from the Institute, RCA, and the US Office of Naval Research, he and a small group of engineers were designing the logic and physical structure for a computer that could modify its own instructions. (Platzman, 1979; Aspray,1990, Nebeker, 1995) Rossby,

Vladimir Zworykin of RCA, and Weather Bureau Chief Francis Reichelderfer, had succeeded in convincing von Neumann that weather prediction was a good candidate for his computer. Although the August 1946 meeting was attended by about 20 of the biggest names in American meteorology, only two things of consequence came from it; Charney and von Neumann met one another, and Jule learned of the forthcoming electronic computer at Princeton. Because of this, Charney's interest in the theory of weather prediction was firmly established before he arrived in Norway in 1947 to begin his fellowship in Oslo.

In Norway he quickly developed his scale theory for geostrophic motion. (Charney, 1948a) This had obvious application to numerical weather prediction because it promised to avoid the difficulties that L. F. Richardson had encountered thirty years earlier. (Richardson's careful computation of an astounding pressure tendency of 145 mbs per 6 hours was grossly incorrect because the surface pressure tendency is very sensitive to error in the horizontal divergence, which even today is difficult to determine from wind observations.) Within the year, Jule had been invited by von Neumann to come to Princeton and take charge of the Meteorological Group.

2. Princeton

Jule came to Princeton as head of the Meteorological Group in von Neumann's Electronic Computer Project in 1948. Arnt Eliassen of the Norwegian Weather Service arrived soon after for a one-year stay. The computer was still more than three years from completion, and there was much preliminary work to be done. They set to work immediately, and by September Charney could write to Rossby that everyone at Princeton was now convinced that the barotropic equations should be the first step.

(Charney, 1948b) This was fully in accord with Rossby's suggested plan of attack.

Nonetheless there was much preparatory analysis to be done. Charney did an amazing job in this work. In retrospect, it is possible to see only a few places where his analysis could have been improved, and then only because we possess meteorological knowledge now that was not available in 1948.

Two major papers were written in this first year. One was published in the new journal *Tellus*, which Rossby had started soon after he returned to Stockholm from America. (Charney and Eliassen, 1949) In that paper Charney and Eliassen did two things. First, they computed the stationary disturbance pattern resulting from a basic zonal flow over mountains. They used a linearized barotropic model in a zonal channel for this. (For this computation, Eliassen introduced a new concept into fluid dynamics, an Ekman surface boundary layer in geostrophic flow.) The best prediction of the winter orographic pattern at 500 mb was obtained when they set the width of their zonal channel at 3600 km
(equal to the distance from 29 to 61 degrees latitude). The paper also introduced a method for solving the linearized barotropic prediction equations for a one-day forecast, using an influence function approach. This had surprising success and was taken up by the Weather Bureau in Washington for daily use. It was then used by Charney to explore the east-west distance needed to make a one-day forecast.

The second paper was titled " On a physical basis for numerical prediction of large-scale motions in the atmosphere. " (Charney, 1949) In it, Charney outlined an overall plan to proceed with NWP, first justifying the geostrophic system, then beginning with simple models and finally progressing to more complicated models. In his correspondence with Rossby, it is clear that both of them were agreed that development

in NWP should proceed by gradually including those factors that experienced forecasters had found useful.

Charney began by showing how a barotropic model could be defined by a process of vertical averaging of the geostrophic prediction equations.

But most of his paper addresses the question of how large a computational region is necessary to make a forecast for one day. His reasoning is based on the concept of group velocity, which had been introduced into meteorology by Rossby in 1945. Several years before World War II, H. Ertel visited Rossby at Woods Hole, probably about the time that Rossby developed his famous wave formula based on an *ad-hoc* barotropic model of the atmosphere. Ertel was impressed by the fact that the time derivative of the streamfunction ψ was obtained by inverting a horizontal Laplacian operator (Ertel, 1941) :

$$\nabla^2 \frac{\partial \psi}{\partial t} = -v \bullet \nabla(f + \zeta) \qquad (1)$$

This meant that the streamfunction tendency, $\frac{\partial \psi}{\partial t}$, at a point A depended on vorticity advection over the entire globe, not only on the advection in the region close to A. Ertel interpreted this as a pessimistic omen for the future of weather prediction with a barotropic model. Although Charney devotes several paragraphs near the end of his paper to a method for solving the barotropic equations on a sphere, he mentions Ertel's paper only in a footnote, seemingly as if it had been added in the galley stage of printing. (This surmise has been verified by the then editor, George Platzman, in private

conversation.) In that footnote he downplays Ertel's paper by emphasizing that for practical purposes there is a limit to the speed of influence propagation. (The group velocity reasoning used by Charney states that *sensible influences* would spread at a rate bounded by the maximum values of the group velocity. Therefore regions far from the forecast area would not influence the forecast for some time.) However, a careful reading with the benefit of hindsight suggests that Charney was somewhat ambivalent about this.

1. In the vertical direction Charney estimated the maximum group velocity of Rossby waves in a resting atmosphere to be 4.5 km per day. This should have been about 3 times larger, about 12 km per day, but he made an algebraic error that was not caught in the reviewing process.

(See Phillips, 1990, page 23.) If he had calculated the correct value for a resting atmosphere some serious doubt might have been created about the reasonableness of ignoring influences from the lower stratosphere on the first numerical forecasts.

In practice the vertical spread of influences was not great enough to interfere noticeably with the early numerical predictions. My guess is that this algebraic error did not lead to disaster because the effective vertical group velocity for Rossby waves is seriously affected by the background zonal winds. This effect was not included by Charney, and the effect of his algebraic error was in effect cancelled by the fact that Rossby waves do not propagate readily into regions of the atmosphere where the zonal wind is less westerly than in the source region.

2. In the zonal direction Jule estimated the barotropic Rossby group velocity in a resting atmosphere by limiting consideration to east-west wave lengths that were not too large. In his writings, he justifies this because he considers the energy in long waves to be small. We now know that this is not the case. When he added an average zonal current to

the east of 18 degrees per day, his conclusion was that the significant influence region for a one-day forecast was 53 degrees of longitude to the west of the forecast point, and 32 degrees to the east. However, in making this estimate it was necessary to assume a width for the zonal channel. And here, with the hindsight of many years, I believe we can conclude that Jule's value of 3600 km for the channel width was optimistic; in a wider channel forecasts for one day would require a significantly larger area. He chose 3600 km because that value gave the best results for the mountain effect in the paper with Eliassen.

Tucked away at the end of the paper is a formulation for solving the barotropic equation on the globe using a Green's function, in other words, a method to solve the elliptic equation for the streamfunction tendency that Ertel considered as being pessimistic for use of the barotropic model. Charney remarks:

"*The nature of the Green's function illustrates clearly the dependence of the signal velocity on scale. G_s decreases so slowly with increasing*

- *(the great circle distance) that apparently the motion over at least an entire hemisphere must be taken into account in evaluating the tendency at some fixed point*."

To justify ignoring this pessimistic conclusion, Charney retreats to the idea that most of the energy is in smaller scales that have smaller group velocities. But at this point, he has essentially agreed with the possibility put forth by Ertel. We will see later that barotropic forecasts did experience difficulty when they were first extended to a hemispherical region.

3. In the south-north direction, Charney used the meridional

component of the group velocity, again in the absence of a zonal current. It can be shown, however, that the effect on middle latitude forecasts of disturbances south of a west wind maximum are greater than if the west wind maximum were absent. (Phillips, 1990, pp. 86-94). Here again the initial theoretical estimates were too optimistic in the sense that later theory would have provided a more pessimistic answer. However, the limited lateral boundaries of the early forecasts undoubtedly helped to keep unrealistic low-latitude influences from corrupting the middle latitude forecasts in the early days.

It was taking a long time to construct the Princeton computer, and as a result, the fourth meteorological hero now enters my story. This is Francis Reichelderfer, Chief of the U. S. Weather Bureau for 25 years
from 1938 to 1963. (Figure 1). In 1950 he was 54 years old. His life-long career in meteorology began 30 years earlier as a Naval officer with an interest in airships, shortly after World War I. Although primarily an administrator in his later career, he was strongly interested in the application of technological developments to the improvement of weather forecasts; developments such as radar and weather satellites, as well as numerical weather prediction occurred under his leadership of the Weather Bureau, and did so during times of great budgetary restrictions. He and Rossby met in the mid-1920s at the Weather Bureau in Washington, where they both attempted to convince forecasters of the benefits of the Norwegian frontal and airmass concepts. They maintained a fruitful working relation throughout their lives. (Bates, 1989; Hughes, 1981; Namias, 1991)

I come now to the event whose anniversary we are celebrating, the barotropic computations in 1950 on the ENIAC computer. Since the Princeton computer was still several years from completion, von Neumann obtained Reichelderfer's help in securing use of the Army's ENIAC computer at Aberdeen Maryland as a temporary expedient.

The "*Electronic Numerical Integrator and Computer*" (Figure 3) used a vast number of radio tubes, with punch cards for input and output. It was large, and not very reliable. Ragnar Fjørtoft from Norway had now replaced Eliassen for a year in Princeton. Fifty years ago this month, he and four others; Charney, John Freeman, George Platzman, and Joseph Smagorinsky worked with the ENIAC in Aberdeen, Maryland for thirty-three 24-hour days, finishing in early April. (Charney, Fjørtoft, and von Neumann, 1950b) In 1978 George Platzman gave a talk at MIT, the First Victor P. Starr Memorial Lecture, at the 50th anniversary of the Department of Meteorology, in which he presented a fascinating review of those 33 days. The following particulars have been gleaned from his paper. (Platzman, 1979)

The ENIAC operated on the decimal system. It had only ten internal words of storage, although about 600 numbers were available in a fixed storage device (read-only memory). In the absence of an internal memory, approximately 100,000 punched cards were used by the Meteorology Group to record the evolving fields of geopotential and its derivatives, each card being punched under control of the ENIAC, reordered and collated with card equipment by operators, and then read into the ENIAC at a later step. This elaborate procedure was designed by von Neumann, who was familiar with the ENIAC from his service as a consultant to the Army. Only four 24-hour forecasts and two 12-hour forecasts were completed, the slowness being due to computer breakdowns. (It took about 24 hours to make a 24-hour prediction if the ENIAC functioned correctly.)

For his talk at MIT, Platzman arranged with the Cambridge office of IBM to use a new product, the "PC" or Portable Computer as it was then called. (We now refer to a "PC" as a Personal Computer.) The IBM engineers coded the ENIAC program on the

PC, and repeated one of the forecasts during the hour or so of Platzman's talk. This represented a 24-fold speed increase over the best operating times in Aberdeen. Nowadays of course, our PCs are many times faster. Modern operational computers, with speeds of 400 Gigaflops, are about 3×10^{8} times as fast as the ENIAC.

Platzman had to leave the ENIAC before the 33 days were finished. He closed his MIT presentation by quoting from a letter written to him by Charney shortly after the ENIAC work was completed.

" ---Thus at the end of 4 weeks of work it appeared that the barotropic model gives good results for large-scale systems, but there was too little evidence to back up this conclusion. ----If we had to stop there, we would still have been up in the air. But in the next week we ---made a 24-hour forecast for Jan. 31 1949 and a 24-hr forecast for Feb. 14, 1949, in which two cut-offs occur. The results showed---that with certain well marked exceptions, the features of the 500-mb flow can be forecast barotropically.--- "

Figure 4 is taken in the ENIAC building at the end of the effort, and shows the exhilaration of some of the participants, and perhaps the tiredness of one or two who had just finished a night shift.

Charney sent a copy of the *Tellus* article to L. F. Richardson.
(Platzman, 1968.) Richardson, who was a Quaker, had left meteorological research many years earlier to work on the causes of war. He wrote back thanking Jule for the reprints, but begged off providing a substantial review because of lack of time. However, he reported asking Mrs. Richardson to judge the four cases, selecting which most closely resembled the verification map, the 24-hour forecast maps (*d*) or the initial maps (*a*). Mrs. Richardson gave a slight edge to the forecast maps, whereupon Richardson commented as follows.

“Thus *d* has it on the average, but only slightly. This, although not a great success of a popular sort is anyway an enormous scientific advance on the single, and quite wrong, result in which Richardson (1922) ended.”

An additional interesting sidelight, that demonstrates how far ahead Charney was thinking, is found in a June 5 letter he wrote to Rossby, in which Jule writes that the primitive equations will be okeh for machine integration. (Charney, 1950a)

3. After the ENIAC

The von Neumann computer was completed for use at Princeton in 1952. Its memory consisted of 40 cathode ray scanning tubes developed by Williams of the University of Manchester in England and James Pomerene in von Neumann's Electronic Computer Group (Aspray, 1990, 83-84.) Each tube had a 32 X 32 array of spots, each spot representing one bit in a 40-bit fixed-point binary number or one bit in one of two 20-bit instructions. Each spot could be either “on” to represent a one, or “off” to represent a zero, and this was controlled by the programmed instructions. Thus it could change its instructions, a great innovation at the time. The Williams tube was an improvement over earlier memory devices, but it did have one severe drawback. If a position in the array was read or written too frequently, electrons spilled over into neighboring spots, corrupting the memory. This meant one was limited in trying to write a subroutine program that was both fast and consisted of few instructions. But since compact codes were essential to maximize the memory available for data, it became necessary on occasion to introduce dummy multiplication operations in order to slow up the arithmetic processor. FORTRAN did not come into existence until several years later; all programming was done in machine language, using hexadecimal notation. The address

modification, counting, and the final “IF” test that are required for a “DO LOOP”, had to be explicitly programmed because index registers did not exist. Input was initially by paper tape, but eventually von Neumann prevailed upon IBM to allow one of their card readers to be modified so that punched cards could be used for input and output. (One of Jule’s programmers was James Cooley, who went on to devise the Fast Fourier Transform with Professor John Tukey of Princeton University.)

The next step after the barotropic ENIAC forecasts was to use a two-level geostrophic model, as reported in Charney and Phillips (1953).
[A second ENIAC expedition was mounted in the spring of 1951 to forecast the evolution of a barotropic wave in a zonal channel, but results were never published. They did however demonstrate the first case of computational problems in the form of excessive “noodling” of the vorticity isolines. (Platzman, 1979)] Original plans had been to explore an advective geostrophic model after the barotropic model. But this was quickly recognized as leading to great instabilities at short wave-lengths, and was never programmed at Princeton.

The two-level computations used information levels at 250- and 750-millibars in a vertical finite-difference scheme introduced by Charney. The test case was a severe storm that developed quickly over the Eastern Seaboard on November 25 1950. Results were not as satisfying as we had hoped. Nonetheless in August 1952, von Neumann called a meeting at Princeton of representatives of the Weather Bureau and the weather services of the Air Force and Navy, to inform them of the progress made by Charney’s Meteorology Group. He recommended to them the establishment of an educational program and a mechanism to investigate technical questions related to beginning implementation of operational weather forecasting. The *Joint Meteorological*

Committee

(JMC) from the three governmental meteorological organizations took responsibility for this. Each organization assigned technically qualified meteorologists to work with Charney's Meteorology Group in Princeton. Another group participated in the first university course in numerical weather prediction, given by George Platzman at the University of Chicago. (Smagorinsky, 1983)

The following spring I left Princeton for Stockholm, to work with meteorologists at Rossby's International Institute of Meteorology in making barotropic forecasts with the new Swedish computer, BESK. This computer was one of 17 computers built in the period 1952-1957 that was patterned closely after the Princeton computer (Aspray, 1990, p. 91). The first forecast was made around Christmas time in 1953, and a telegram with the pleasant news was sent to Rossby, who was vacationing in Capri.

Back in Princeton, Jule suspected that the two-level model failed to do a good job of predicting the cyclogenesis of November 24 1950 because it could not represent the large horizontal temperature gradients in the lower troposphere. The size of the Princeton computer did not allow many levels of data, so Jule formulated a special 3-level model, with information levels at 400, 700, and 900 millibars. This model did predict a strong surface cyclone for 24 hours after the initial time of 1500Z on the 24th of November. In a paper published in the *Proceedings of the National Academy of Science* (Charney, 1954), Charney described this as the first numerical prediction of cyclogenesis.

A series of forecast tests were then arranged to guide the JMC. The Princeton 3-level model was run on 14 cases and a 2-level model developed by the Air Force Cambridge Research Center was run on 60 cases (Shuman, 1989) . The JMC chose the former

model. A committee to advise the JMC on computers had also been established, and an IBM 701 was delivered to the fledgling Joint Numerical Weather Prediction Unit (JNWP) in early 1955. Dr. George Cressman, a graduate of Rossby's Department of Meteorology at the University of Chicago, who was then working as a consultant for the Air Weather Service, was named Director (Figure 1) and the first forecast was made on April 15 1955. [This is reproduced as Figure 5 in Smagorinsky, 1983.]

4. The Joint Numerical Weather Prediction Unit.

The first forecast was a reasonably good forecast, but later results were unsatisfactory. Fred Shuman, who succeeded George Cressman as Director of the National Meteorological Center, has written as follows.

> " *When the model was programmed for the IBM 701 in 1955, run on an operational schedule, and subjected to the critical eye of the practicing synoptician in real time, it was discovered that it not only was unable to predict reliably and accurately, but it also provided little or no useful information to the forecaster." (Shuman, 1989, page 287.)*

Shuman remarks further that useful results from the JNWP operational system did not come until 1958, when an analysis program was implemented in April (Cressman, 1959) and automatic handling of data from the international WMO network was developed. The forecast system, furthermore, had been simplified to a barotropic model. However, the predictions were not sent out to weather forecast offices until 1960; until then the computer output was used only for guidance of forecasters at the National Meteorological Center.

The conditions under which personnel operated at JNWP and at centers in other

countries were quite different from those of today. FORTRAN had become available, but computer time was much scarcer than today, and less reliable. Secondly, forecast output was only available from a line printer; there were no graphics programs such as we have today. Finally, any statistical working up of results from a group of forecasts had to be done by hand; there was no mechanism by which forecasts from different days could be stored in one electronic data base for convenient manipulation.

But in addition to routine technical problems there were several meteorological difficulties encountered at JNWP, difficulties that were unexpected. The first was the operational failure of Charney's 3-level model. The second appeared later in the barotropic model; when that model was extended to cover a hemisphere, the very long waves moved westward. A third problem was recognized gradually, that both the continuous and finite-difference form of the prediction equations had to be formulated carefully so as not to introduce physically inconsistent results. (e. g. Shuman, 1957; Hollman, 1956).

5. A revisit to the Appalachian storm of November 1950 .

Charts A and D in Figure 6 show observed 900-mb heights for 15Z on November 24 and 25. In this 24-hour period the cyclone at this level deepened by the equivalent of 15 millibars, with a simultaneous intensification of the neighboring anticyclones. Very strong winds resulted, with intense snowfall. Chart B presents the results of Charney's 24-hour forecast. (Charney, 1955). His 3-level model predicted a deepening of the right amount, but considerably north of the verifying location. It also falsely predicted an unrealistically large pressure rise over the coast of the Gulf of Mexico. (A less extreme form of the latter was a common occurrence with multi-level geostrophic models in the first years at JNWP.)

A possible explanation is demonstrated on Figure 7, showing the 400-mb streamfunction and geopotential fields at the initial time, 15Z on November 24. The streamfunction was determined from the vorticity, which was analysed using wind measurements supplemented by gradient wind calculations. For convenience, the geopotential field is presented in the same units, obtained by dividing the 400-mb geopotential by f_O , where f_O is a representative value of the Coriolis parameter at 37.5 deg latitude. (A constant was then added to make comparison easier.) The overestimation of the wind by the geostrophic value is apparent in the larger gradient on the geopotential chart in the southern U. S. The cyclonic relative vorticity , ζ , is also overestimated.

If we consider the vorticity equation,

$$\frac{\partial \zeta}{\partial t} = -v \bullet \nabla(f + \zeta) + f \frac{\partial \omega}{\partial p} \quad , \qquad (2)$$

the overestimate of ζ on both sides to some extent will cancel out, since it is proportional to the Laplacian of the geopotential in both places. But the overestimate of the advecting velocity, v , will remain on the right side, leading to an overestimate of the speed with which the vorticity maximum in the sharp trough is advected to the east and north. Underneath this large increase in vorticity at upper levels of the model, conventional quasi-geostrophic reasoning predicts strong upward motion so as to create the large temperature decrease required for hydrostatic and geostrophic balance. Thus, excessive surface cyclogenesis will be predicted under the falsely large positive value of

$\frac{\partial \zeta}{\partial t}$. Simultaneously, the overly- rapid advection of the positive vorticity center to the east and north will leave behind an area of strong anticyclogenesis at low levels in the wake of the vorticity maximum. This agrees with the situation pictured on Chart B of Figure 6.

Charney's 3-level model is conventional in that it is based on an analysis of the geopotential field. This means that the temperature field at $t = 0$ is "exact", while the accuracy of the wind field at $t = 0$ is limited by the geostrophic approximation. One can instead consider the opposite assumption: that is, to use a streamfunction ψ for the wind, and then use ψ, multiplied by an average value of the Coriolis parameter, in place of geopotential as an approximation in the hydrostatic equation. Which approximation scheme is best depends on the structure of the atmospheric flow pattern, and the relative importance of vorticity and temperature advection. It cannot be answered by ordinary scaling theory. But the success of the barotropic model suggests that atmospheric flow patterns are such that it may be more important to have a good representation of the wind than of the temperature.

In 1958 I published the results of such a calculation, using a 2-level model at 400 and 900 mbs, in the 60th year *Festschrift* for Erik Palmén (Phillips, 1958) . The 24-hour forecast for 900-mb height, as deduced from the forecast streamfunction via the balance equation, is reproduced as Chart C in Figure 7. It is a major improvement over the 3-level results, especially if allowance is made for the neglect of latent heat. Unfortunately this possibility for improvement in the quasi-geostrophic model was realized too late; interest was shifting to use of the so-called

"primitive equations", undoubedly sparked by Hinkelmann's paper in the *Rossby Memorial Volume.* (Hinkelmann, 1959). I believe that baroclinic quasi-geostrophic models might have been more productive in the United States if they had used a streamfunction in place of the geopotential.

The good news about the November 1950 storm is that, almost 50 years later, an exceptionally good forecast can be made with current methods of analysis and prediction. Figure 8 shows the results from reruns made last year by the National Center for Environmental Prediction using their current data assimilation, analysis, and forecast system (Kistler, et al, 1999). A nearly perfect forecast of the sea-level storm for 1500Z on the 25th is made as much as 3.5 days in advance. The longer forecasts of 4.5 and 5.5 days, although useful as indicators of storm development, are not as accurate. A likely explanation for this is that the 500-mb vorticity center associated with the storm did not enter the North American radiosonde network until 03Z on November 22, 3.5 days before the storm. This is shown by the two 500-mb charts at the bottom of Figure 8. (In modern days, there would have been much better data over the oceans, from aircraft winds and satellite temperature retrievals, and a useable forecast one week in advance might have been possible.)

My principal conclusions are as follows.

(1) A modified quasi-geostrophic model could have given an excellent 24-hour forecast.

(2) The Appalachian storm of November 1950 was not a case of sudden unpredictable instability; with modern forecasting methods it could have been forecast days in advance.

(3) This is strong evidence for how much numerical weather prediction has advanced, and justifies the confident optimism of the five scientists shown in Figure 1.

6. The retrogression of the long waves.

The advent of a larger computer, the IBM 704, enabled the JNWP group to expand the barotropic integration area to near hemispheric dimensions. It was noticed immediately that large height errors appeared from the rapid westward movement ("retrogression") of the very long waves, numbers 1, 2 and 3. This was not anticipated in the early estimates by Charney. (Charney, 1949)

Initially the error was reduced by artificially holding those wave numbers fixed during the integration. Later, Cressman (1958) obtained a slightly better error reduction by introducing an empirical correction for the divergence that can be present in a barotropic model with a free surface. (The correction was large enough to require assumption of an inert layer above the barotropic layer.) These errors had not appeared in earlier barotropic integrations because the smallness of the forecast area simply eliminated these large wavelengths. In the atmosphere these long wavelengths are quasi-stationary, being associated principally with large mountain ranges.

The effect of orography was included in the barotropic model. But this representation was necessarily crude, since the surface wind that determines upslope motion could only be taken as a fixed fraction of the 500-mb wind. In this respect, the Charney-Eliassen computations

(Charney and Eliassen, 1949) seem to have been over-optimistic; they had chosen a half-width only 32 degrees of latitude wide for their zonal channel, *as having given the best results for their orographic modelling.*

The Haurwitz Rossby waves, for a non-divergent atmosphere on a globe, for zonal wave number one, and meridional wave number two, have a period of about 2.5 days, i.e. a westward phase speed of about 144 degrees per day. Charney's assumptions of a limited

width for his zonal channel lead, however, to a much smaller westward propagation of about 20 degrees per day for zonal wave number one.

In retrospect, it is therefore not surprising that

(a) The hemispheric barotropic model did not represent well the physics of the quasi-stationary long waves, and,

(b) when the existing long waves were included in the initial flow pattern at 500 mbs, they moved westward much faster than suggested by Charney's zonal barotropic model.

It is very doubtful that the orographic effect can be modelled adequately in a barotropic model, and it is not easy to think of an improvement to the empirical corrections introduced by Cressman. Perhaps the only conclusion that can be drawn is the one suggested by Shuman (1989, page 289):

" It is fair to describe the situation not as a case of faulty theory, but rather as a case of incomplete theory. ---- the operational environment must be considered as a necessary milieu for completion of the theory. Through repetition during more than 30 yr, this has become a well-established principle. "

Shuman wrote this in 1989. A welcome change to practice in centers of numerical weather prediction is the provision of enough computer time to test new features in detail before operational implementation. This is especially necessary when the machine output is made available to outside forecasters and the public.

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Figure Legends

Figure 1. Principal contributors in the U. S. initiation of numerical weather prediction. Upper left: Carl-Gustaf Rossby. Center: Jule Charney. Upper right: John von Neumann. Lower left: Francis Reichelderfer. Lower Right: George Cressman. Picture credits are to

the *American Meteorological Society* for Charney, Rossby, and von Neumann, the *National Academy of Science* for Reichelderfer, and *C. Bates* for Cressman.

Figure 2. Cartoon in the UCLA student newspaper of 1946 showing Charney conversing with a fellow student. (Wurtele, 1990.)

Figure 3. The ENIAC computer in 1948. The operators are changing the plug-in wiring. (Platzman, 1979.)

Figure 4. Participants in the ENIAC forecasts. Standing, from the left: R. Fjørtoft, J. Charney, J. Freeman, and J. Smagorinsky. (G. Platzman is absent.) Kneeling: Two programmer assistants at the Aberdeen Proving Ground. The ENIAC is in the background. (From Smagorinsky, 1983.)

Alternate Figure 4. Participants and supporters at the end of the ENIAC computations in April 1950. From left to right: H. Wexler, (Director of Scientific Research for the Weather Bureau), J. von Neumann, M. Frankel (mathematician at Aberdeen) , J. Namias (Head of the Long-Range Forecasting Group of the Weather Bureau), John Freeman, R. Fjørtoft, F. Reichelderfer, and J. Charney. G. Platzman and J. Smagorinsky were absent.(*Courtesy of C. Bates*.)

Figure 5. Some personnel of the Meteorology Group in Princeton, 1952. From the left: J. Charney, N. Phillips, G. Lewis, N. Gilbarg, and G. Platzman. (From Smagorinsky,

1983.)

Figure 6. Observed and forecast fields of 900-mb height. Units are decafeet (3.048 m) deviation from the standard atmosphere value. Upper left (A): Initial observed field at 15Z 24 November. Upper right (B): 24-hour forecast for 15Z 25 November calculated with three-level “geopotential” model. Lower left (C): 24-hour forecast for 15Z 25 November calculated with two-level “streamfunction” model.
Lower right (D): Verification field at 15Z 25 November.

Figure 7. Initial streamfunction and geopotential at 400 mbs, 15Z 24 November. Units are 10^{6} m^{2} sec^{-1} . Geopotential values in m^{2} sec^{-2} were converted to 10^{6} m^{2} sec^{-1} through division by the value of the Coriolis parameter at 37.5 degrees north.

Figure 8. Seven sea-level pressure forecasts of length from one day to 5.5 days, all verifying at 15Z 25 November 1950, as computed by the National Center for Environmental Prediction in a rerun of historical data using the NCEP current analysis and forecast system. (Green areas signify precipitation.) The verification figure is shown twice, as the upper left and lower right of the 9 smaller maps. The two bottom charts are
500-mb analyses for 03Z on November 21 and 22, green areas signifying centers of positive relative vorticity. The letter C on these charts denotes the location of the vorticity center that ended up as the storm center on November 25. (The black arrows connect the initial 500-mb chart with the corresponding sea-level forecast.)

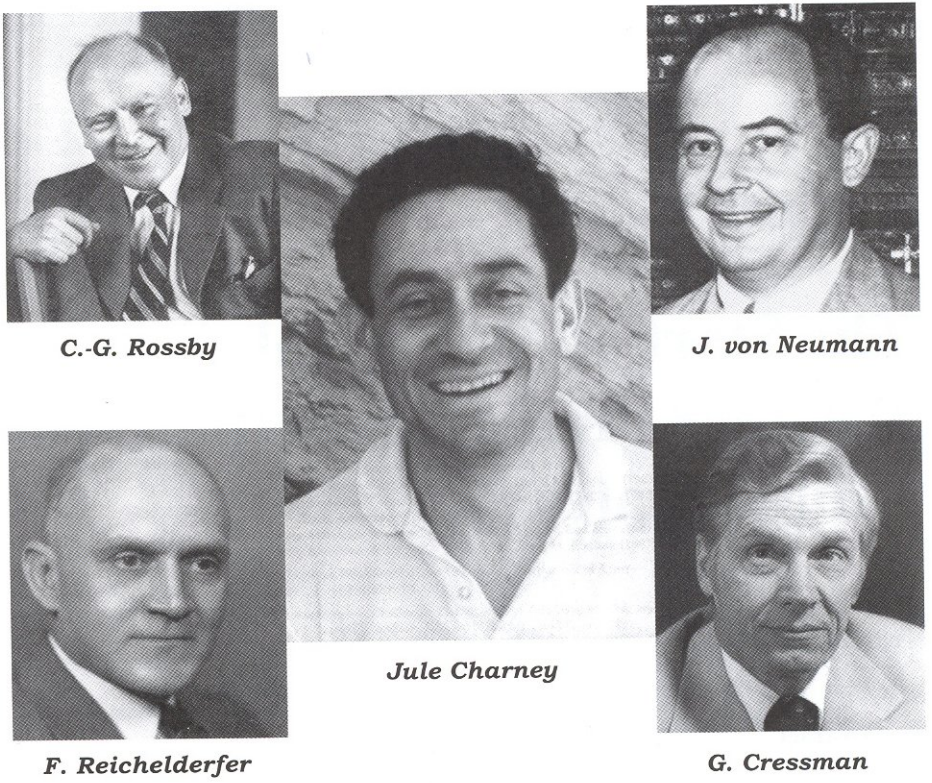
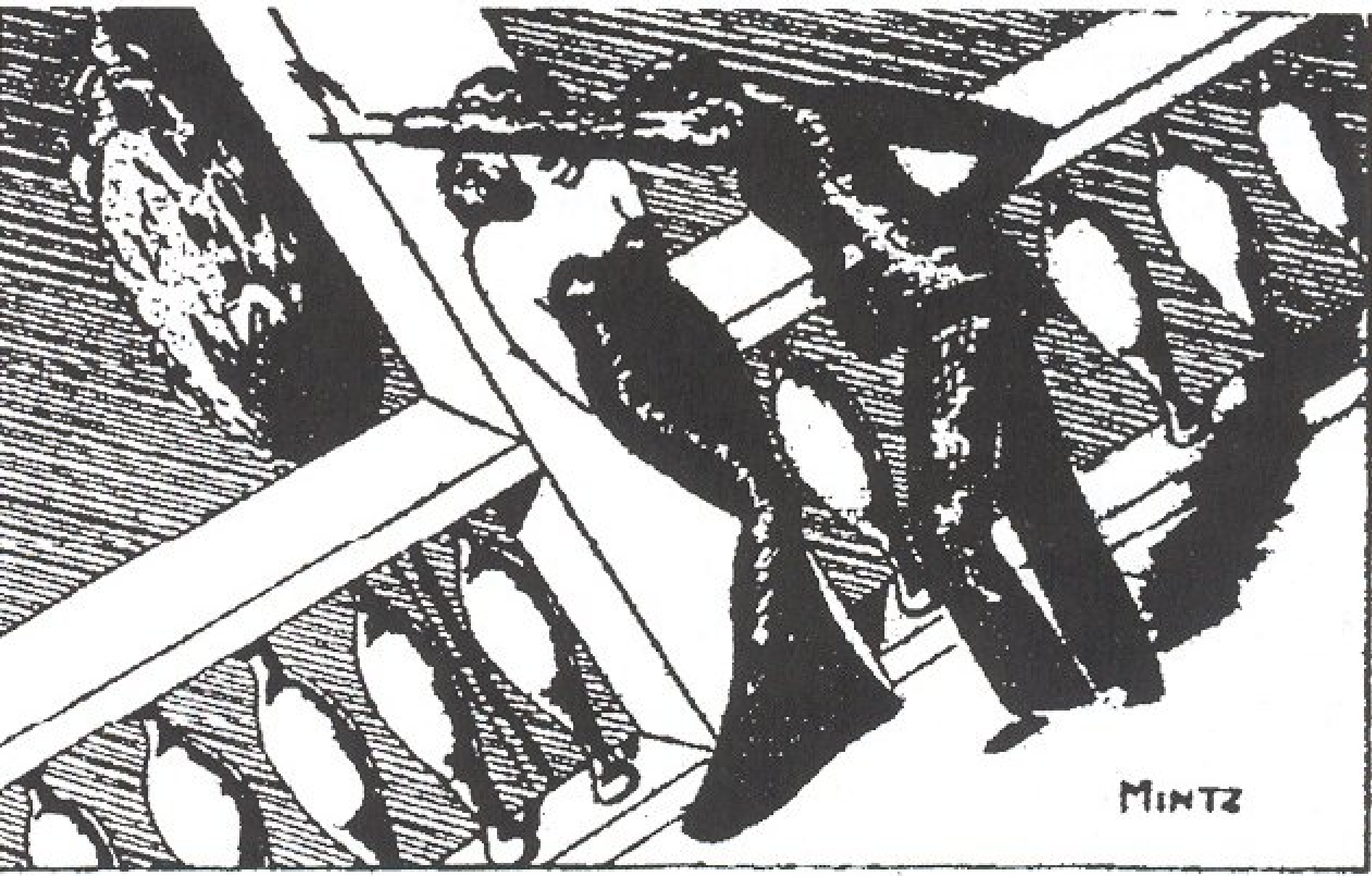
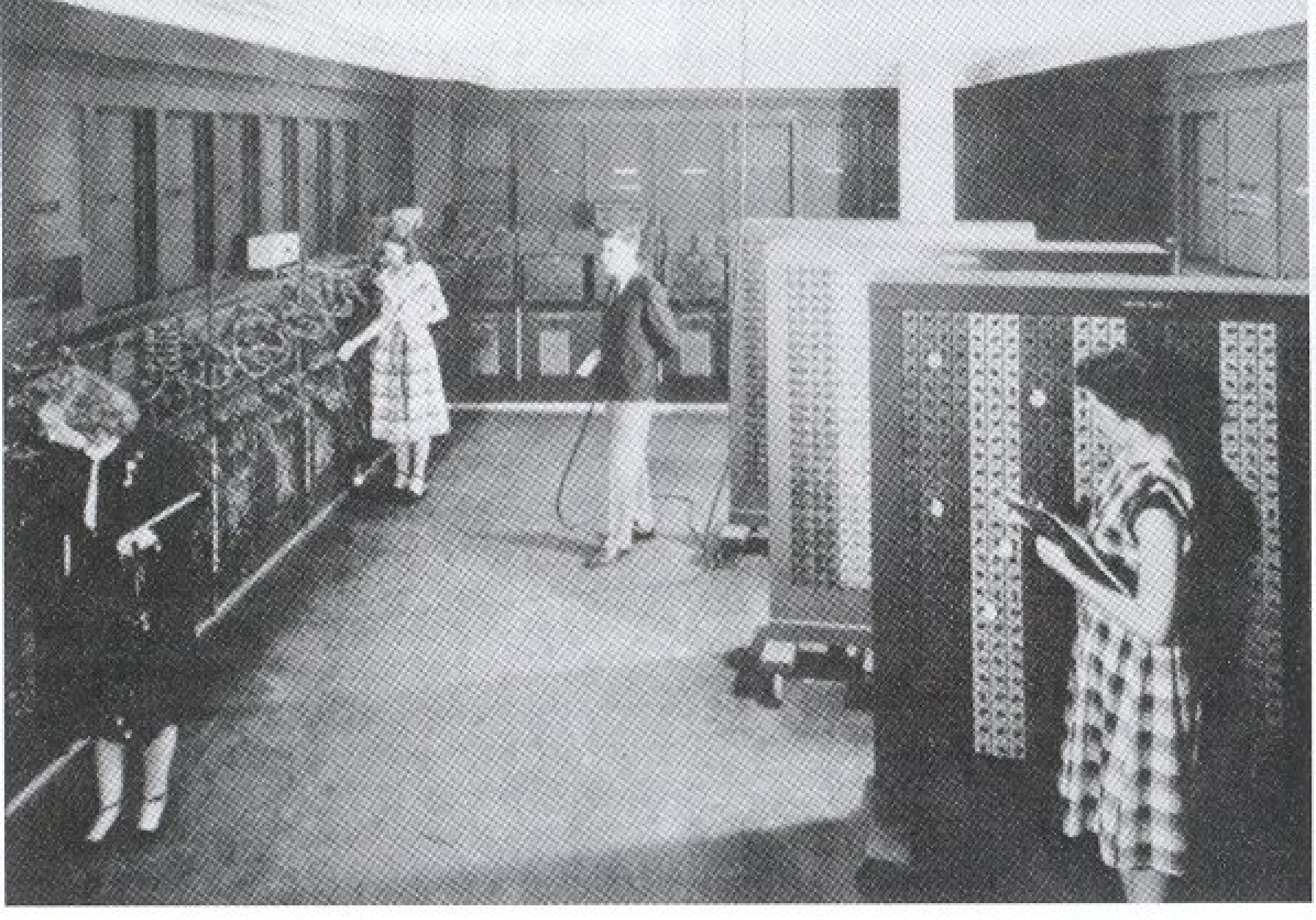


Figure 1: Principal contributors in the U. S. initiation of numerical weather prediction. Picture credits are to the *American Meteorological Society* for CHARNEY, ROSSBY, and VON NEUMANN, the *National Academy of Science* for REICHELDERFER, and *C. Bates* for CRESSMAN.



"…and since these are hypergeometric differential equations with logarithmic singularities…"



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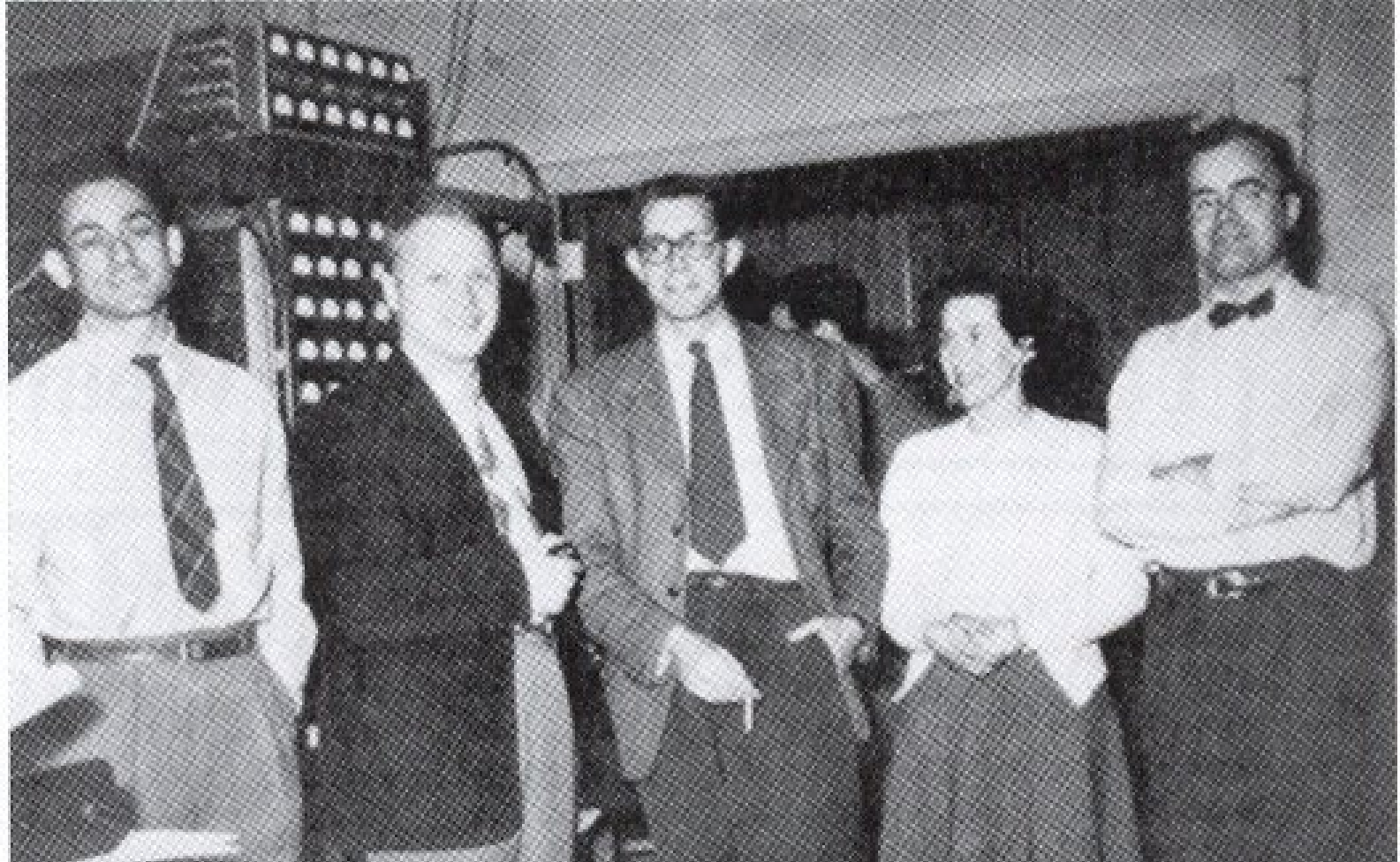

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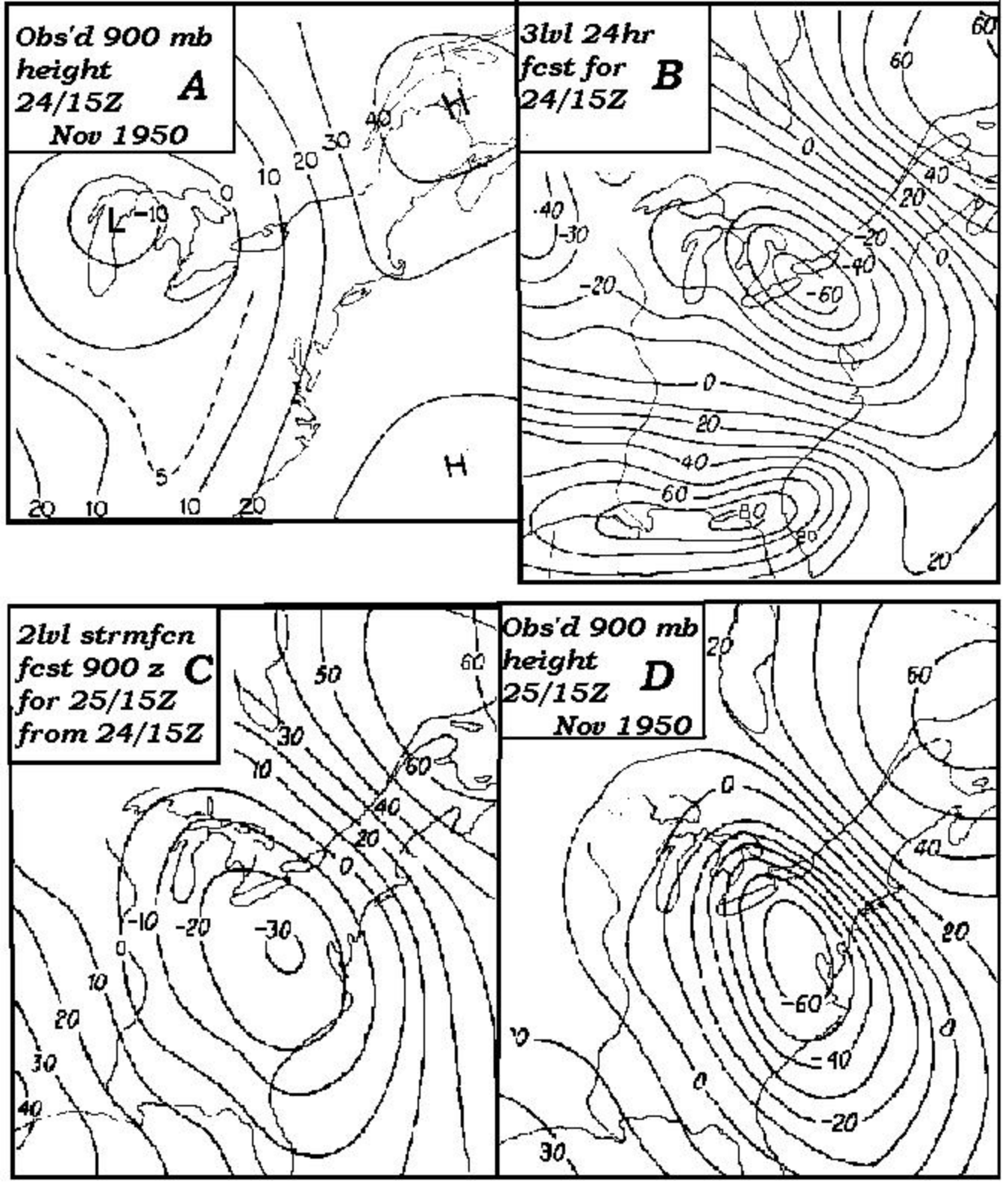
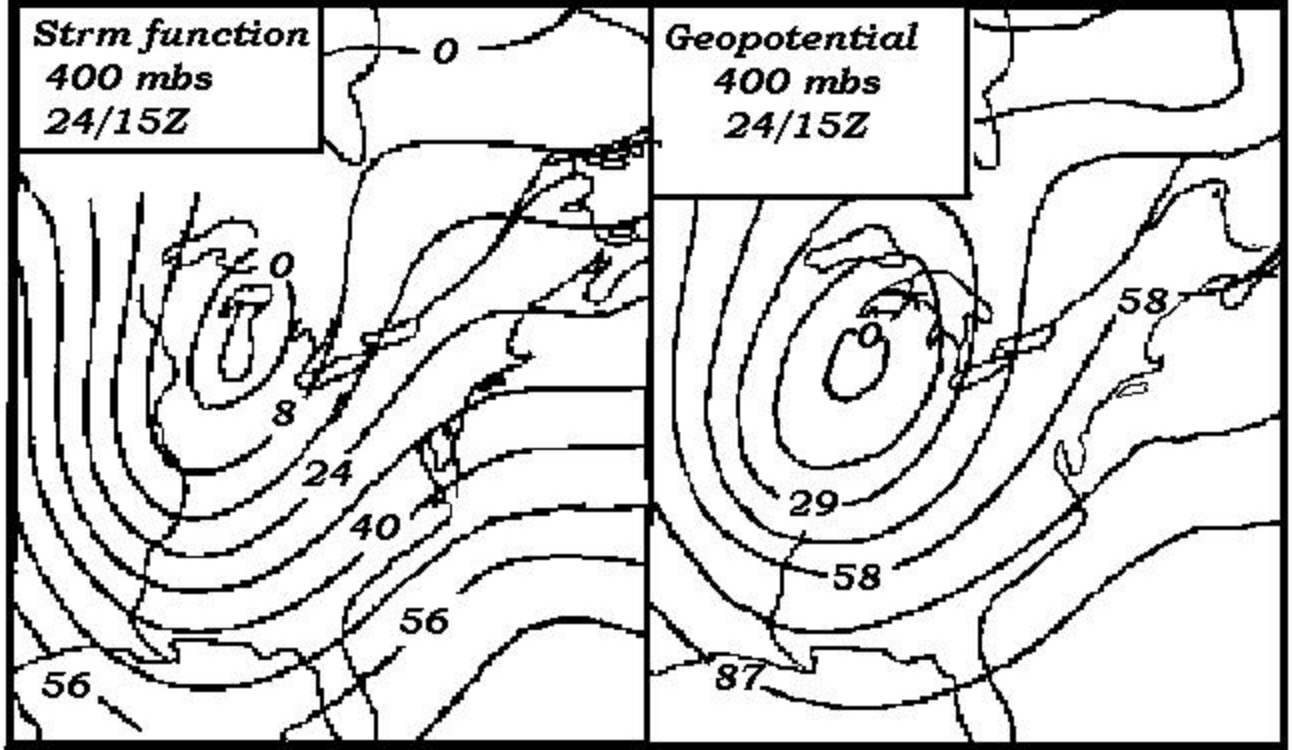

Obs'd 900 mb height 24/15Z Nov 1950
A
3lvl 24hr fcst for 24/15Z
B
2lvl strmfcn fcst 900 z for 25/15Z from 24/15Z
C
Obs'd 900 mb height 25/15Z Nov 1950
D


Strm function
400 mbs
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Geopotential
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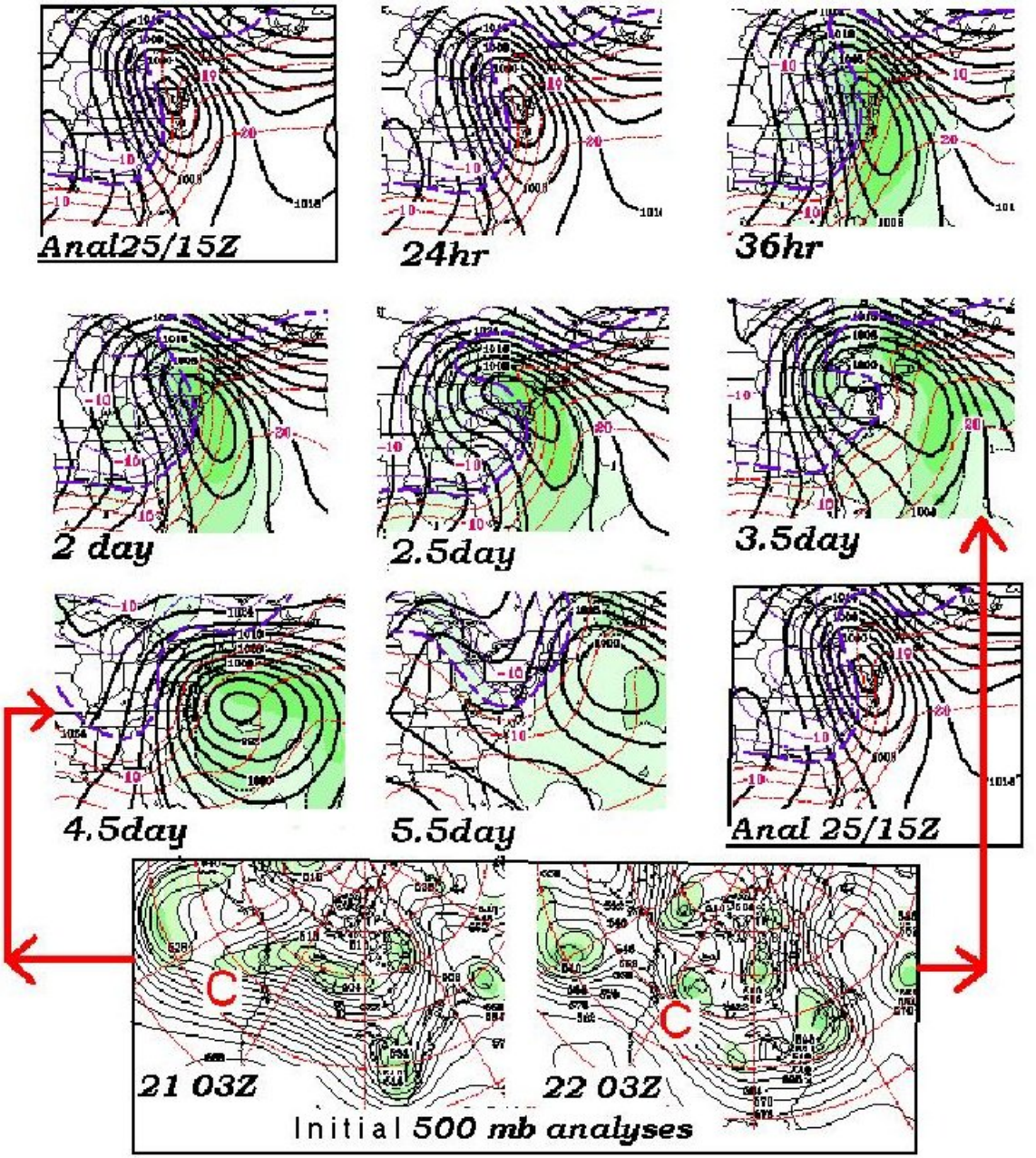
29
58
87



Modern forecasts for 15Z 25 NOV 1950, made by the NCEP analysis/forecast system.

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