# THE BEGINNINGS OF NUMERICAL WEATHER PREDICTION AND GENERAL CIRCULATION MODELING: EARLY RECOLLECTIONS

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## 1. INTRODUCTORY REMARKS

I should say at the outset that my intention here is not to deliver a comprehensive history of the important formative circumstances of the late 1940s and the following decade. The invitation was to make a personal presentation of events in which I had been involved and of which had first-hand knowledge. I have tried to assemble, from memory and from personal documents, my impressions of the time. My account will therefore be quite selective, but I hope it will be viewed as a useful, if no insightful, contribution to the history by a witness and participant. I therefore apologize in advance for the many lapses in completeness which, no doubt, will be detected by the many others who had been involved in that fascinating period.

The appropriateness of including the era of numerical weather prediction in a symposium on "Advances in the Theory of Climate" is, in retrospect, quite obvious. It was the development of the scientific bas and technical methodology needed for the modeling for prediction tha paved the way later on for modeling the processes responsible for th general circulation and thereafter for the simulation of climate. In turn, i was the general circulation models that provided the vehicle in the 1960 and 1970s for extending numerical weather prediction beyond a few days

Hardly any of the events and circumstances touched upon in this account could have happened without Jule Charney. They might have occurred eventually and probably in some other form, but Charney's geniu




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and driving force were singularly responsible for their happening whe and how they did. Charney passed from our midst in June 1981. It is m honor to dedicate this work to his memory.

## 2. Some Personal Antecedents

My interest in meteorology began in my midteens (in the late 1930s when I thought that weather prediction was somehow accomplished de terministically by the application of physical principles. Quite consis tently, I also thought this was true for the design of ship hulls, and in fac at that time my first interest was in naval architecture. But financial an family considerations dominated my career decision, and I entered a un versity course of study in meteorology. I, of course, quickly learned tha my basic assumption was quite incorrect. Weather forecasting was quit subjective, but based on powerful conceptual procedures—the construc tion of the isobaric weather map and an identification of the air mass an frontal systems. The predicted time–space evolution of the synoptic ma was based on the experience of having observed and classified many suc evolutions. The forecast of wind, temperature, and precipitation wa based on empirical models of how these meteorological parameters woul be associated with the predicted pressure field and its attendant air mas and frontal systems.

World War II interrupted my formal university education and I entere a military meteorology training course at the Massachusetts Institute ( Technology (MIT) where, in 1943, I came into contact with the eminer dynamic meteorologist, Professor Bernhard Haurwitz. When I asked hi why physical principles had not been applied to the practical problem ( weather prediction, he quickly pointed out the futility of using the ter dency equation to predict surface pressure changes. The actual winc were not sufficiently accurate and the geostrophic approximation woul give nonsensical measures of the horizontal divergence. When querie further, Haurwitz did recall the work of L. F. Richardson during and ju after World War I, but, as I remember, did not attach great importance t its implications.

I was resigned to frustration and disappointment which remained do mant until the end of the decade. I returned to civilian life, resumed m university education, and went on to complete a master's degree wit emphasis on dynamic meteorology. My first position was as a researc meteorologist at the U.S. Weather Bureau under Dr. Harry Wexler. I 1949, I heard a lecture by Jule Charney which changed my life. H systematic analysis of the scale properties of large-scale atmospheric mo tions and his presentation of a rational approach to deriving a geostroph cally consistent set of prediction equations, reawakened my hopes for hydrodynamic framework for prediction. I did not, of course, know ho far Charney's ideas would carry in shaping, and indeed revolutionizin the physical and dynamical basis for weather prediction. In fact, we no know that the basic methodology would eventually find its way into tl study of a much broader part of the spectrum of phenomena than midtr pospheric Rossby waves. With the modern high-speed electronic con puter, then under development by von Neumann and his colleagues at tl Institute for Advanced Study in Princeton, it would eventually be possib to study synoptic-scale baroclinic processes, the dynamics of convectic and mesoscale phenomena, the general circulation, climate, and even tl ocean circulation.

In one day, my visions were completely transformed. Little did I kno that I would be privileged to participate in a scientific revolution tha when I first made my career choice, I had mistakenly thought had alreac happened at the time.

## 3. The Institute for Advanced Study 1949–1953

The formation of the Meteorology Group at the Institute for Advance Study (IAS) in Princeton and its first numerical forecasts on the Ele tronic Numerical Integrator and Computer (ENIAC) were key events the early history of numerical weather prediction. These events we eloquently described in authoritative detail in a lecture[1] in memory Professor Victor P. Starr at MIT in 1979 by Professor George W. Plat man, who himself was instrumental during that period. Here, I will on try to supplement his account with additional documented contempora impressions, keeping duplication at a minimum. At this point one shou note that remarkably parallel developments were taking place in the S viet Union during the 1940s and 1950s. But because scientific communic tions with the West did not begin to fully develop until the late 1950 much of the Soviet work was largely unknown until an excellent compar tive survey of research through 1959 was published by Phillips, Blume and Coté in 1960.[2]

Based on John von Neumann's radically new logical ideas for a store program computer using Williams's cathode ray tube technology as

[1] Platzman, G. W. The ENIAC computations of 1950—gateway to numerical weath prediction. *Bull. Am. Meteorol. Soc.* 60(4), 302–312 (1970).

[2] Phillips, N. A., Blumen, W., and Coté, O. Numerical weather prediction in the Sov Union. *Bull. Am. Meteorol. Soc.* 41(11), 599–617 (1960).

storage device, an Electronic Computer Project was established in 1946 at the IAS. Only von Neumann's great reputation and persuasive power were able to overcome the opposition of the faculty to so mundane an enterprise. The circumstances surrounding this event are well documented in a book by H. H. Goldstine,[3] who was one of the prime movers on the project. As he points out, a threefold thrust was intended: engineering, numerical mathematics, and some important and large-scale applications. For the latter, von Neumann selected numerical meteorology. This was based on his knowledge of Richardson's earlier work and also on encouragement by Carl-Gustav Rossby of the University of Chicago and Harry Wexler of the U.S. Weather Bureau. It was recorded at the time:[4]

> A project whose ultimate effects on weather forecasting may be revolutionary has been quietly under way during the past year in the academic surroundings of the Institute for Advanced Study, Princeton, New Jersey. . . . In August 1946, a conference of meteorologists met in Princeton to discuss the project. . . . Since last summer, work has gone forward in promising fashion, though it is still far too early to expect immediate, tangible results. . . . The immediate aims of this group are the selection and mathematical formulation of meteorological problems to be solved by the electronic computer . . . the most interesting feature of the project is the effort being made to link the theory behind atmospheric processes with future weather.

After failing to persuade Rossby to come to the Institute to lead the effort, von Neumann invited one of Rossby's young proteges from the University of Chicago, Albert Cahn, Jr., who was then succeeded by Philip D. Thompson.

Charney, who had been a graduate student of J. Holmboe's at the University of California at Los Angeles, came to Rossby's attention when he briefly served as a research associate at the University of Chicago in 1946–1947 on his way to a postdoctoral appointment at the University of Oslo. During that academic year, Rossby, with a distinguished group of collaborators, produced a famous synoptic, theoretical, and experimental paper on the interaction of long waves with the zonal circulation.[5] Although Charney was at Chicago for only part of the duration of that project, he impressed Rossby to the point where Charney was invited to lead the IAS Meteorology Group upon his return from Oslo in 1948. It was in Oslo that he wrote his scale paper.[6]

Charney immediately invited Arnt Eliassen to join him. Eliassen had by

[3] Goldstine, H. H. "The Computer from Pascal to von Neumann." Princeton Univ. Press, Princeton, New Jersey, 1972.

[4] "Electronic Computer Project," Weather Bureau Topics and Personnel, July 1947.

[5] Staff Members of the Department of Meteorology of the University of Chicago (J. G. Charney, G. P. Cressman, D. Fultz, L. Hess, A. D. Nyberg, E. V. Palmen, H. Riehl, C. G. Rossby, Z. Sekera, V. P. Starr, and T.-C. Yeh). On the general circulation of the atmosphere in middle latitudes. *Bull. Am. Meteorol. Soc.* **28**, 255–280 (1947).

[6] Charney, J. G. On the scale of atmospheric motions. *Geofys. Publ.* **17**(2), 1–17 (1948).

that time completed his definitive paper on a consistent formulation of th hydrostatically conditioned equations in pressure coordinates.[7] That wa the beginning of the famous Meteorology Group. Charney was also joine by a young mathematician, Gilbert A. Hunt. It was this triumvirate that in January 1949, reported on a "Program for Numerical Weather Predic tion" in New York that had captivated me. Hunt, soon after, returned t his first love and is now a distinguished Professor of Mathematics a Princeton University.

The beginning of the collaboration of Charney and Eliassen in Osl produced two key papers after they reunited in Princeton. The first, b Charney himself,[8] was a comprehensive rationale which laid the founda tion for dynamical prediction. It justified the use of the geostrophic ap proximation to filter small-scale high-frequency noise from the vorticit equation, discussed the propagation of signal and its implications on dat requirements, introduced the notion of the equivalent-barotropic atmo sphere to reduce the forecast problem to a two-dimensional one, an finally, showed how Green's functions could be used to make a linear one dimensional prediction for an arbitrary initial geopotential distribution a midtroposphere. A companion paper, submitted a few days later by Char ney together with Eliassen,[9] gave the results of one-dimensional predic tions (along a latitude band) and also applied these techniques to the stud of topographically produced quasi-stationary perturbations.

In those early days, Charney's group for the most part consisted of tw to four meteorologists on visits for about one year. The main exceptio was Norman A. Phillips, who arrived in 1951 after completing his Ph.D. a the University of Chicago and moved to MIT with Charney in 1956.

In 1949, I was invited as an occasional visitor, from my base in Wash ington, D.C., to assist the group in extending its one-dimensional linea barotropic calculations. On behalf of the Weather Bureau, I also wa asked to become familiar with the theoretical aspects of a more realisti model. As a result of a month-long visit in the spring of 1949, I recorded i a report:[10]

> Essentially, the new method is a much refined form of the vorticity theorem enunciated by Rossby in the late 1930's. Although this model is, as Rossby's, a barotropic fluid in one-dimensional motion which only considers small perturbations, it can take into account [equivalent-barotropic] divergence, the mean finite lateral width of a disturbance, friction, topography, an arbitrary initial pressure disturbance, and the

[7] Eliassen, A. The quasi-static equations of motion with pressure as independent variabl *Geofys. Publ.* **17**(3), 1–44 (1949).

[8] Charney, J. G. On a physical basis for numerical prediction of large-scale motions in th atmosphere. *J. Meteorol.* **6**, 371–385 (1949).

[9] Charney, J. G., and Eliassen, A. A numerical method for predicting the perturbations the middle latitude westerlies. *Tellus* **1**(2), 38–54 (1949).

[10] Memorandum, Smagorinsky to Chief of Bureau [F. W. Reichelderfer], June 30, 194

boundary conditions which arise from considering circular latitude lines. To constru this model, it was necessary to introduce a number of arbitrary parameters in order t describe more fully actual atmospheric motions. The parameters involve (1) a measu of the finite lateral extent of the disturbances and (2) a second approximation on th assumption of a constant basic zonal current. These can best be evaluated by repeate application of the forecast formula to many varied situations for different season performing, more or less, a controlled experiment. It should be remarked that the parameters have some physical meaning, and this is utilized in testing.

The Meteorology Group had only made a few test forecasts. With the aid of Mr Margaret Smagorinsky,[11] well over one hundred 24-hour winter forecasts at one lat tude were made and analyzed. The forecasts verified fairly well, and may be consi ered competitively with subjective forecasts. This is very encouraging since the latt type of forecast has only limited physical basis, while the Charney–Eliassen method based wholly on dynamic considerations. However, because of the simplicity of th model, it is recommended that this method be used only as a supplementary too recognizing where its shortcomings lie and how they will affect the forecast. It is four that one can usually state *a priori* how well the objective method will verify, but it thought that improvement in verification will come from additional detailed analyses the forecasts.

A small number of trial 5-day forecasts at 500 mb were made. These verified ve poorly. However, theoretical examination showed that the proper choice of the arb trary parameters mentioned above becomes extremely critical in the forecast and th they are a major source of discrepancy. The breakdown can best be observed l successive forecasts from the same profile for 1, 2, . . . , 6, 7 days. One of the mo important and successful tools of the Extended Forecast Group [of the Weather B reau] is an empirically corrected form of Rossby's original formula. It is hoped th proper employment of the more refined technique will enhance the usefulness of th vorticity concept.

Dr. Charney and Mr. Eliassen are not considering extending this model to tw dimensions, since hand computations would become formidable. Instead, their pla are to construct a two-dimensional barotropic model which also permits non-line motions. The solution, of course, can only be found by the electronic compute Studies of how the present simple model fails will aid Charney and Eliassen in the attempt on the non-linear problem by giving them a clue as to which simplifications a most detrimental.

A short while later, I attended a conference at the University of ( cago in which Charney, Eliassen, and the Staff of the Departmen Meteorology participated. The purpose was to assess the basic theory the numerical forecasting technique, and to examine the results of s trial forecasts. I reported:[12]

The role of forced stationary perturbations was reviewed, and it was agreed that th effects of topography [and] friction . . . were taken into account as well as possib with this simple atmospheric model.

[11] It was not unusual for wives to be professionally involved. Adele Goldstine, Klara Neumann and, for a short period, Margaret Smagorinsky all programmed for the IAS c puter—in absolute octal, of course.

[12] Memorandum, Smagorinsky to Chief of Bureau, July 18, 1949.

The fact that the entire energy balance cannot be described by a single-layer barotr
pic model led to the tentative conclusion that this method would fail for long perio
forecasts even with the two-dimensional non-linear model. . . .
Professor Rossby gave high praise to the work of Charney and Eliassen and e
pressed the view that this represented one of the most significant turning points in th
history of theoretical and synoptic meteorology.

Subsequently, Eliassen[13] repeated his misgivings: "Personally, I m
confess that I don't expect too much from the application to 5-day m
maps but experience will, of course, be the best judge."

I was also applying the influence functions to the 36-hr, 700-mbar fo
cast problem, at the three latitudes, 35° N, 48° N, and 55° N for
longitudes 50° W to 120° W. Upon comparing the results with the Weat
Bureau Analysis Center (WBAN) operational forecasts, Wexler co
mented:[14] "It is seen that the two techniques give quite similar verifi
tion which should be considered quite a victory for the numerical, obj
tive procedure, based upon the results of dynamic meteorolog
Actually, the dynamical forecasts were slightly inferior.

Meanwhile, the Princeton group was already moving onward rapidly
deal with the barotropic finite amplitude problem.

In the early stages, Charney wrote:[15] "We had so many difficulties w
the hand computations for the 2-dim. finite amplitude motion that I
cided to abandon the project, especially in consideration of the fact t
we are planning to do the same thing on the ENIAC beginning Decem
1."

The ENIAC at the U.S. Army Aberdeen Proving Ground in Maryl
had been enlisted for the nonlinear barotropic forecast integrations. Ac
ally, the Aberdeen operation was delayed until March 1950. There
some uncertainty in the choice of the particular case to be studied. I
suggested a blocking situation in December 1949. However, Charney u
mately settled on January 5, 30, and 31 and February 13, 1949. Some
the considerations were:[16]

How will this model explain the subsequent motions on a map

(a) that is essentially barotropic;
(b) which is predominantly baroclinic;
(c) which displays pronounced blocking of the jet stream.

These tests are designed so that one may discover which properties of baroclini
motions are most essential in devising a more realistic atmospheric model. In additio

[13] Letter, Eliassen to Smagorinsky, July 31, 1949.

[14] Memorandum, Wexler to Chief of Bureau, January 12, 1950.

[15] Letter, Charney to Smagorinsky, November 3, 1949.

[16] Memorandum, Smagorinsky to Chief, Weather Bureau, February 7, 1950.

it is planned to test some hypothetical situations which exhibit such "pure" disturbances as:

(a) an isolated vortex in a field of no relative vorticity,
(b) an isolated vortex in a field of zonal motion which possesses vorticity,
(c) periodically distributed vortices in a field of zonal motion which is dynamically unstable.

Because of the limited capacity of the ENIAC it is also necessary to decide upon the most suitable map projection, and the appropriate time and space scales.

In reporting on the outcome of the ENIAC expedition, I noted:[17]

Unfortunately, the lack of time made it impossible to make as many tests as would have been desirable. Before one could actively test the hypothesis, a number of fundamental questions had to be answered:

1. What are the upper and lower limits of the grid spacing from which observed contour heights are selected?
2. What is the most feasible time interval to be used in going into the future?

Tentatively, the results of a number of test forecasts indicate a grid size of about 5° longitude at 45° latitude and a time interval of two or three hours. On this basis, two forecasts were made from synoptic situations that were characteristically barotropic, and a third was partially completed. The first was a 24-hour forecast from the 0300 map of January 5, 1949. The computed forecast was not particularly good. Some of the failure could be attributed to the small size of a closed low over the United States which fell in a blank spot of the grid used and so the data picked off did not describe sufficiently the flow about the low. However, the computation did predict the westward motion of the western cell of the Bermuda high. Another 24 hour forecast, from the 0300 map of January 31, 1949, gave excellent results. It called for such changes of the atmospheric motion as the intensification of a blocking high, the filling of trough and the pivoting of an elongated low pressure area.

This relatively crude theoretical model of the atmosphere succeeded in serving as the basis for the prediction of some very pronounced modifications of the planetary flow pattern and demonstrated that for the cases and forecast period chosen the atmospheric processes were essentially barotropic. However, this is not meant to minimize the role played by baroclinic phenomena, which, through the transformation of potential to kinetic energy, are able to generate new disturbances. The small amount of experience thus gained indicates that the barotropic model is adequate for forecasting up to 36 or 48 hours. It is planned that succeeding models will incorporate baroclinic mechanisms which take into account the effects of variations in the vertical structure of the atmosphere.

The fact that the memory capacity of the ENIAC was being overtaxed was already evident. This together with the relative slowness of the machine (36 hours for a 24 hour forecast) deems it impractical to use the ENIAC on a baroclinic model, since it would require many times the memory capacity and correspondingly greater speed. For this purpose the Princeton machine will probably be used. The date of availability of the [IAS computer] is still a moot question, with optimistic estimates ranging from June to October of this year.

[17] Memorandum, Smagorinsky to Chief, Weather Bureau, April 14, 1950.

Some of the results of this month's work are immediately applicable to subjective forecasting techniques. The experiments showed that a knowledge of the field of maximum absolute vorticity transport gave an excellent indication of the regions of extreme instantaneous pressure tendency. The absolute vorticity transport corresponding to the latest available synoptic map can be calculated by desk computer. Making use of the relatively conservative property of these instantaneous tendencies, one can use them as a supplementary tool to the one-dimensional numerical forecasts. This tool will be experimented with in the Short Range Forecast Development Section [of the U.S. Weather Bureau].

In conclusion the writer wishes to express his appreciation for being given the opportunity of being associated with this historic development in the science of meteorology. We are at the beginning of a new era in weather forecasting—an era that will be based on the use of high speed automatic computers. For best use of the computers, it is essential that our aerological data procurement over the oceans be greatly increased.

The formal scientific report of this first ENIAC expedition was publishe by the principals in *Tellus*.[18] John C. Freeman of the Meteorology Grou and George Platzman were the other participants (Fig. 1). In following ur I wrote about future plans:[19]

The Weather Bureau will participate in a number of experiments for some preliminary tests in anticipation of the use of the [IAS computer] for the solution of this [baroclinic] problem. Experience with the earlier tests at Aberdeen indicated that many questions of fundamental nature can be investigated before use is made of a high speed computer.

In the tests at Aberdeen, it is thought that one of the failures of the barotropic model to explain a pronounced development will be remedied by taking into account the baroclinicity which obviously was present. This situation, a low which first deepened at higher levels in eastern Canada on January 30, 1949, will be used for the first test.

Examination of the data for that date once again brought to focus the great lack of sufficient and reliable data to high levels. It is hoped that the future will show this unfortunate situation remedied.

In the summer of 1950, I began an extended stay at the Institute; it wa to last until the spring of 1953. Norman Phillips was already working o his two-layer model for his doctoral dissertation at the University c Chicago. In his 1951 paper[20] he showed that one can construct a model c two superimposed barotropic fluids which indirectly can be related to th real baroclinic continuum. With this model he calculated sea level tender cies and midtropospheric vertical motions for the famous 1950 Thanksgi ing Day storm over eastern North America. The results were quite e couraging. This was to be a test case of baroclinic development again

[18] Charney, J. G., Fjørtoft, R., and von Neumann, J. Numerical integration of the barotr pic vorticity equation. *Tellus* **2,** 237–254 (1950).

[19] Memorandum, Smagorinsky to Chief, Weather Bureau, May 31, 1950.

[20] Phillips, N. A. A simple three dimensional model for the study of large-scale extratroj cal flow patterns. *J. Atmos. Meteorol.* **8,** 381–394 (1951).

FIG. 1. Some of the participants in the first ENIAC expedition, March 1950. Left
standing: R. Fjørtoft, J. G. Charney, J. C. Freeman, J. Smagorinsky (J. von Neum
G. W. Platzman, absent); front: programmer assistants of the Aberdeen Proving
The ENIAC is in the background.

years later. Phillips was invited to join the IAS group in 1951. Mean both Eady[21] in the United Kingdom and Eliassen[22] in Norway h signed their "2½-dimensional" models. It was pointed out by El that the three models were mathematically equivalent to each othe the proper interpretation of the dependent variables and constant pa

[21] Eady, E. T. Note on weather computing and the so-called 2½-dimensional model **4,** 157–167 (1952).

[22] Eliassen, A. Simplified dynamic models of the atmosphere, designed for the pu numerical prediction. *Tellus* **4,** 145–156 (1952).

ters. Of course, the greater problem remained, that is, establishing equivalence of these models to the real atmosphere. The generalizat was undertaken by Charney and Phillips.[23] The limit for large $n$ of tl $n + \frac{1}{2}$ level formulation was "not the most general three-dimensic model, but is one that can be obtained from the next general mc through ignoring certain effects due to the spatial variations of st stability and absolute vorticity." They then used the $2\frac{1}{2}$-dimensional v sion to make the first finite interval baroclinic forecasts on the IAS co puter which was completed in early 1952.[24] A series of six 12- and 24 forecasts were produced; the case was the Thanksgiving Day Storm 1950. Based on the ENIAC experience with sparse oceanic data, a limi area covering eastern United States and southern Canada was selected the integration.

In the Meteorology Group's (Fig. 2) report for 1952,[25] which, of cou covered these new results, Charney exposed his contextual philosophy well as described some of the details:

> The problem of primary interest at present is the prediction of changes of atmo spheric flow over a period of 24 to 48 hours. The prediction of the field of motion is necessary, though not a sufficient, pre-requisite for predicting cloudiness and precip tation. The philosophy guiding the approach to this problem has been to construct hierarchy of atmospheric models of increasing complexity, the features of each succe sive model being determined by an analysis of the shortcomings of the previous mode
>
> The primitive equations of motion reflect the fact that the atmosphere is capable c sustaining a wide spectrum of disturbances. For the purpose of short-range weathe prediction, only those disturbances of planetary dimensions with periods of 3 to 7 day are of importance. These motions may be characterized as quasi-hydrostatic an quasi-geostrophic. . . .
>
> The work during the calendar year 1952 was geared to the use of the Institut computer which was completed early in the year. It was decided that the most logic procedure was to test the various models on a single sequence of weather events. Fo this purpose the storm of November 25, 1950 over the eastern United States wa admirably suited. This storm was one of the most rapid and intense developmen ever to have been recorded by a modern observational network. Since its develop ment involved large conversions of potential to kinetic energy and since it was we documented it appears to be an excellent laboratory in which to apply the variou models. . . .

[23] Charney, J. G., and Phillips, N. A. Numerical integration of the quasi-geostro equations for barotropic and simple baroclinic flows. *J. Meteorol.* **10,** 71–99 (1953).

[24] Versions of this computer were constructed at several locations in the United S and carried such names as MANIAC (Mathematical Analyzer, Numerical Integrator Computer) at Los Alamos, New Mexico and JOHNNIAC (named for John von Neuman Rand Corporation in Santa Monica, California. MANIAC is sometimes erroneously use the IAS computer.

[25] The Institute for Advanced Study, the Meteorology Project, Summary of work u Contract N-6-ori-139(1), NR 082-008 during the Calendar Year 1952.

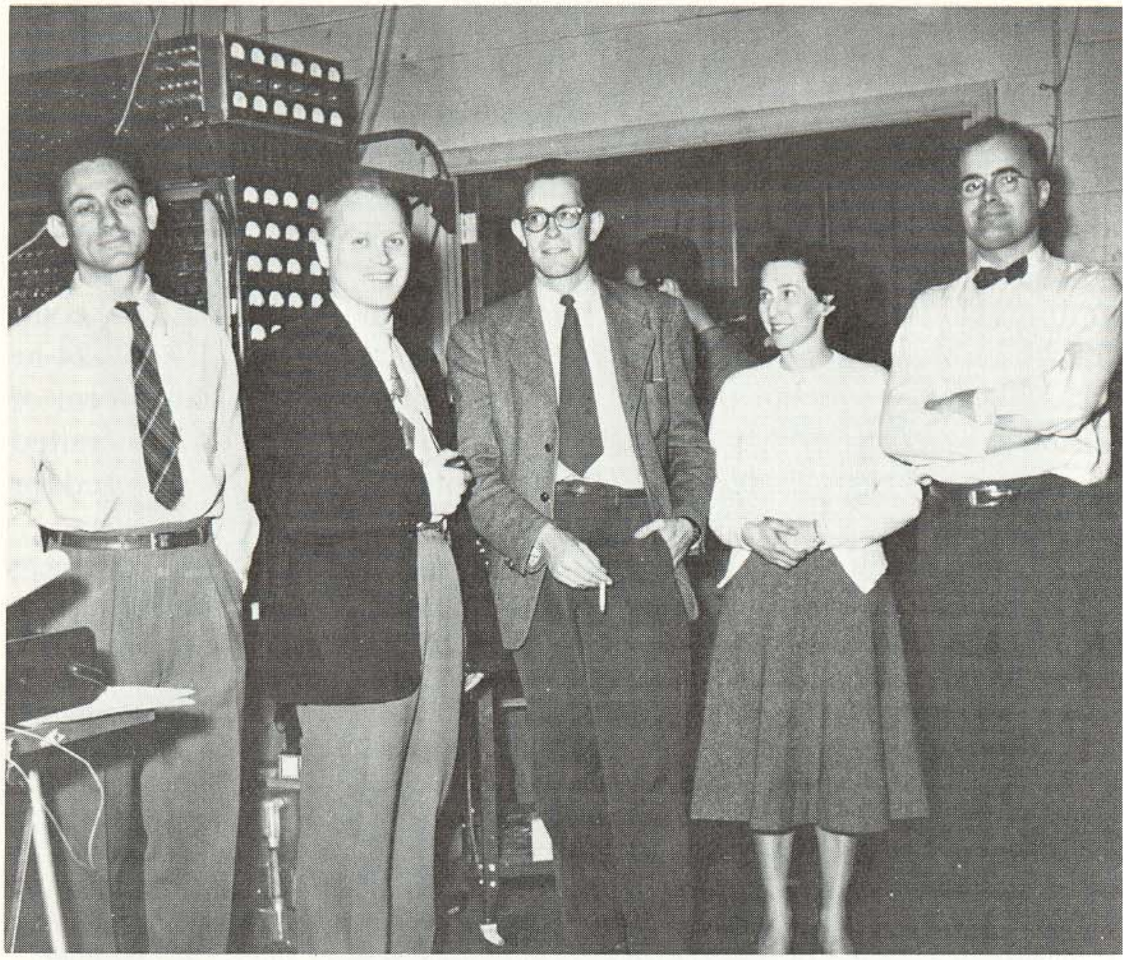

FIG. 2. Some of the members of the IAS Meteorology Group in 1952. Left to right: J. ( Charney, N. A. Phillips, G. Lewis, N. Gilbarg, G. W. Platzman (behind the camera: Smagorinsky). The IAS Computer is in the background.

The [baroclinic] model consists essentially of two barotropic layers and requires initial data at the 700 mb and 300 mb levels. It was necessary in order to avoid computational instability to proceed in half-hour time steps. The total computation time for a 24-hour forecast was approximately $2\frac{1}{2}$ hours at full speed. However, the machine usually operated at half speed.

No account can be taken of the horizontal variations of the static-stability in the two-layer model. A diagnosis of the nature of the two-layer model's shortcomings indicates that this artificial constraint may be of importance. In order to remove it, a model with information at a minimum of three levels is required. Such a model is now in the process of preparation for the machine. [Lorenz showed in 1960[26] that an energetically consistent two-level model with variable static stability can be constructed.]

Much thought has been given to the construction of a full three-dimensional model which will adequately describe the vertical variability of the atmospheric motion. The theoretical and programmatical problems are manifold. Since non-adiabatic effects must at present be neglected for lack of adequate knowledge, the motion is regarded as adiabatic. The potential temperature is then a conservative quantity and may be used as the vertical coordinate in a semi-Lagrangian coordinate system. In this system the equation of motion has a beautifully simple form and is well-adapted to numerical

[26] Lorenz, E. N. Energy and numerical weather prediction. *Tellus* **12**(4), 361–373 (196(

integration. The complete integration of this equation has now been programmed for the I.A.S. machine. The actual coding and computation now awaits the completion of a magnetic drum auxiliary memory, which is needed in a computation of so large a magnitude as this.

We plan eventually to consider the influence of non-adiabatic effects, large-scale orography, and friction at the earth's surface. Also, since the geostrophic approximation does not always appear to be valid, investigations are being made of higher order geostrophic approximations.

Even with the present crude models, cursory comparison with subjective prognoses made by experienced forecasters indicates at least comparable accuracy. Moreover, whereas subjective methods have not shown significant improvements in the past 20 years, the present approach may be refined in a logical manner. It is therefore expected that more realistic atmospheric models will yield predictions becoming progressively and significantly better.

The activities of the Meteorology Group at Princeton have created much interest in numerical weather prediction throughout the world. Research in this direction is concurrently being conducted in England, Norway, Sweden, Denmark, Germany and Japan. In this country, the Weather Bureau and the weather services of the Navy and Air Force have expressed their desire to investigate the possibility of preparing numerical forecasts on an operational basis.

In a subsequent article,[27] Charney commented that the predictions o the November 1950 storm with the $2\frac{1}{2}$-dimensional model "were moder ately accurate during the period immediately preceding the storm bu deteriorated markedly in accuracy after its onset." He did not feel tha the cyclogenesis had been predicted. He felt that the constraint of hori zontal invariance of the static-stability precluded important effects of th low-level thermal asymmetries associated with a front, reflecting the ear lier conclusions in the 1952 Project report. Indeed, the paper goes on t show the results of an integration for this case with a three-level quasi geostrophic model. The full intensity of cyclogenesis was predicted (Figs 3 and 4), as it was in still another, less intense, but more typical case. H showed that by changing the three levels from 200, 500, and 850 mbar t 400, 700, and 900 mbar, the forecast was improved because of a bette representation of the baroclinic structure in the lower troposphere. Fror this, Charney concluded that nongeostrophic and nonadiabatic effect were not essential for the cyclogenetic instability process.

During much of this interval of baroclinic modeling and testing at IAS, was only peripherally involved. My major occupation was to explore th nature of the quasi-stationary components of the atmosphere. This was i keeping with Charney's broad perspective of looking into forced as we as transient modes of the general circulation. Charney and Eliassen's[2

[27] Charney, J. G. Numerical prediction of cyclogenesis. *Proc. Natl. Acad. Sci. U.S.* **40**(2), 99–110 (1954).

[28] Charney, J. G., and Eliassen, A. A numerical method for predicting the perturbatio of the middle latitude westerlies. *Tellus* **1**(2), 38–54 (1949).

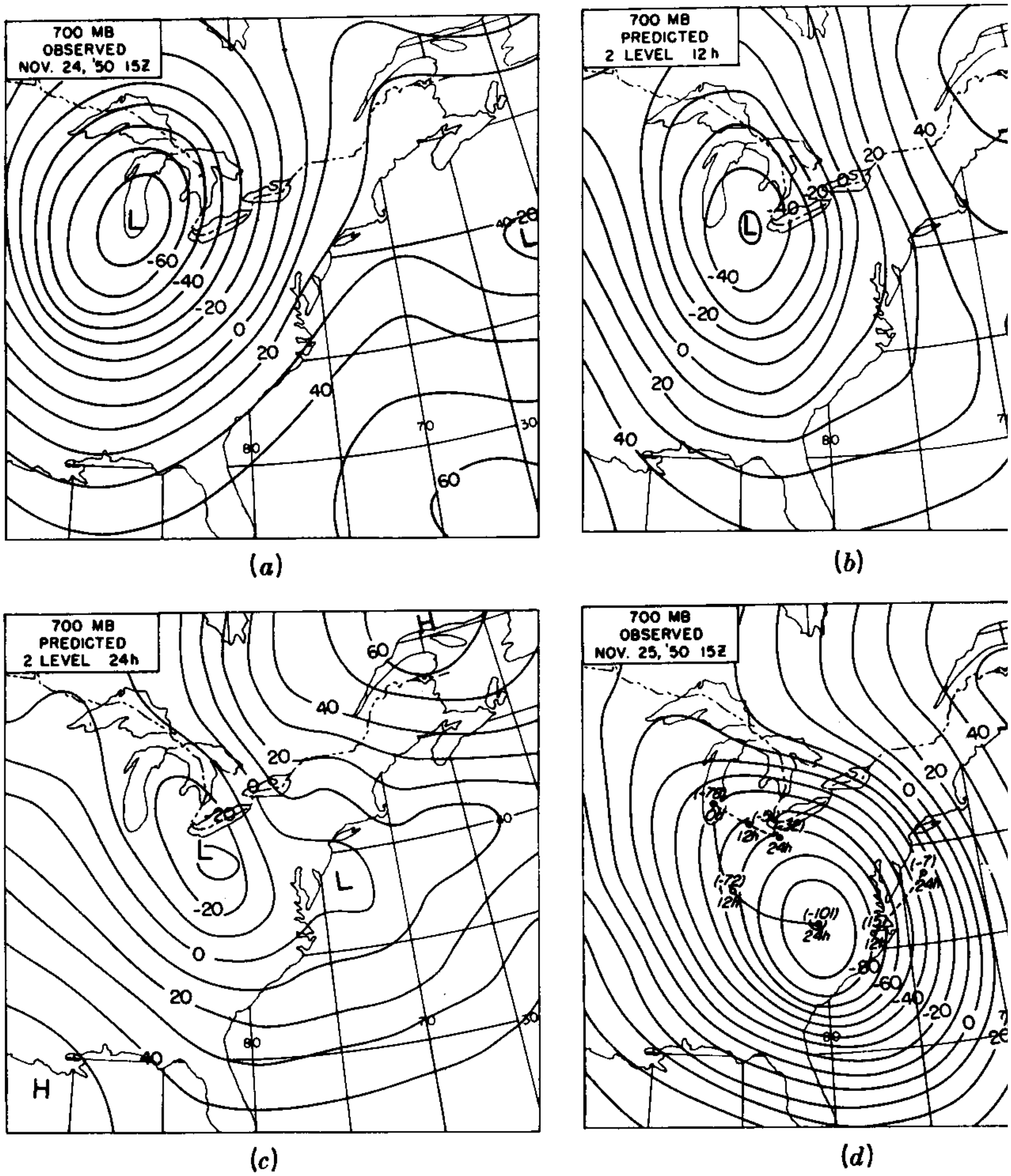


FIG. 3. (a) Observed 700-mbar height contours at 15 Z November 24, 1950. The c
are labeled as deviations from the standard height of 9879 ft in units of 10 ft. (b) Tw
700-mbar prediction for 03 Z November 25. (c) Two-level 700-mbar prediction f
November 25. (d) Observed 700-mbar chart for 15 Z November 25. The small circl
nected by solid lines indicate the successive positions of the observed low center, an
connected by dashed lines, the predicted positions. The height difference at the c
printed above, and the time below each circle. From J. G. Charney, *Proc. Natl. Ac
U.S.A.* **40**(2), 99–110 (1954).

linear barotropic results of orographically produced perturbations a
subsequent barotropic calculations by Bolin[29] had left unexplaine
relative role of the ocean–continent distribution in forcing quasi-st

[29] Bolin, B. On the influence of the earth's orography on the general character
westerlies. *Tellus* **2**, 184–195 (1950).

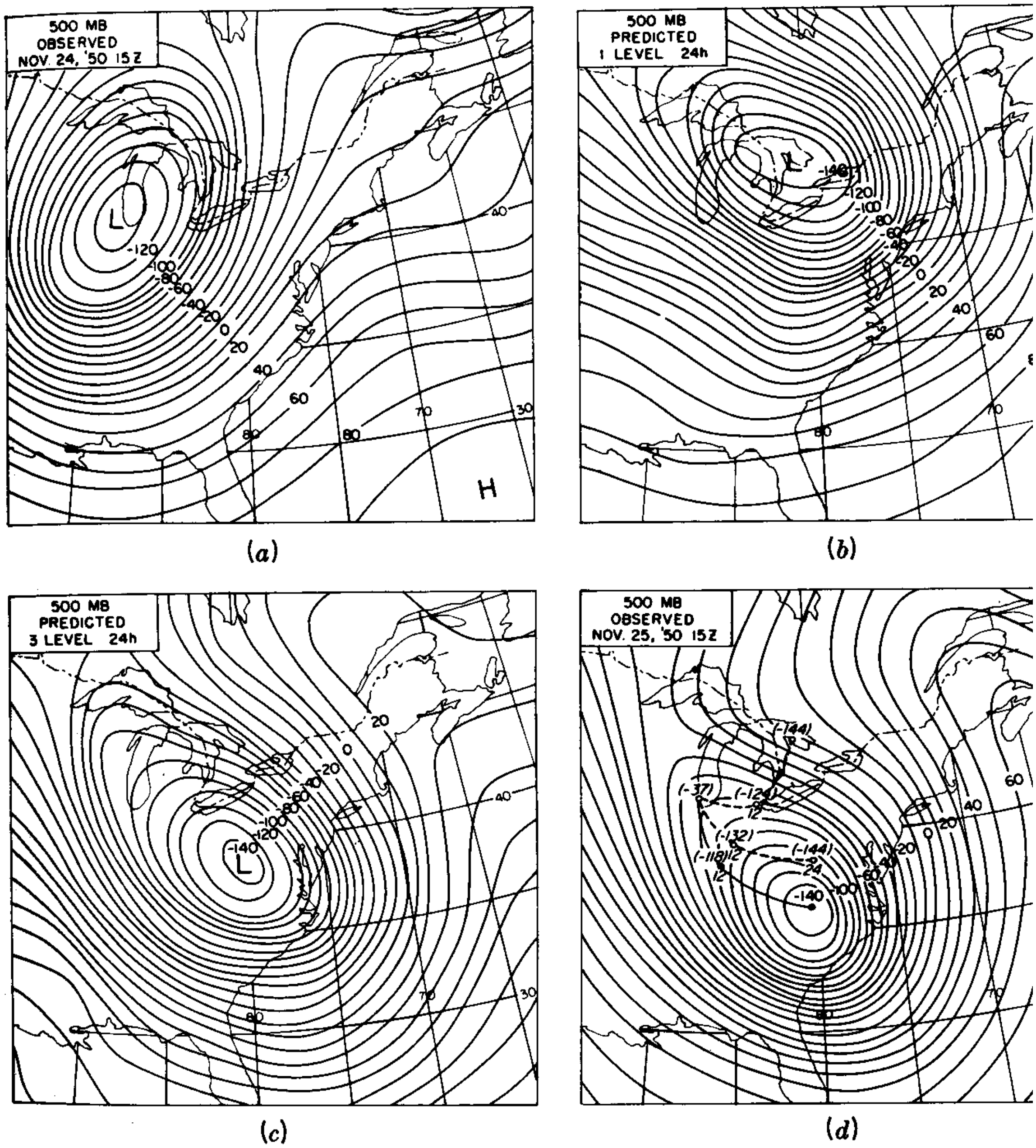


FIG. 4. (a) Observed 500-mbar height contours at 15 Z November 24, 1950. The cont
are labeled as deviations from the standard height of 18,281 ft in units of 10 ft. (b) One-l
(autobarotropic) 500-mbar prediction for 15 Z November 25. (c) Three-level 500-mbar
diction for 15 Z November 25. (d) Observed 500-mbar chart for 15 Z November 25. See
3 legend for explanation of circles and connecting lines. The upper dashed line is the pat
the low center in the one-level model, the lower dashed line is the path in the three-l
model. From J. G. Charney, *Proc. Natl. Acad. Sci. U.S.A.* **40**(2), 99–110 (1954).

ary modes. This would be essentially a baroclinic process and should critically season dependent. I was able to make use of the model t Charney devised for his baroclinic instability study.[30] Independently unknown to me, Bruce Gilchrist, a student of Eady's at Imperial Colle

[30] Charney, J. G. Dynamics of long waves in a baroclinic westerly current. *J. Meteoro* 135–162 (1947).

undertook an almost identical investigation. By still more remark; coincidence, Gilchrist made use of Eady's baroclinic instability mod and Charney's and Eady's work were coincidences in themselves. paper[32] ultimately became my doctoral dissertation and was transmi to the *Quarterly Journal of the Royal Meteorological Society* by E himself. Gilchrist's results[33] and mine complemented each other v well. In essence, they concluded that both orography and heat asym tries are equally important in explaining the perturbations in midtro sphere, but that heating plays the dominant role in the lower levels. l thermore, the large-scale eddy transports of momentum and energy not crucial in explaining the observed perturbations of the normals, are essential for the maintenance of the westerlies.

## 4. The Road to Operational Adaptation of Numerical Weather Prediction

In order to provide some perspective on this phase of developmen will backtrack. On August 5, 1952, von Neumann held a meeting at IAS of representatives of the Weather Bureau, Air Force, and Na which included H. Wexler, S. Petterssen, and D. Rex. Some of thoughts expressed here are extracted from the minutes of the meetin Von Neumann started by saying: "The meeting has been called beca the work that has been done here at the Institute is at the stage wl some practical information is available concerning operational wea forecasting by numerical methods. The object of the meeting is to de mine whether the stage is ripe to prepare for operational forecastin After giving a short summary of the achievements that had accrued u that time (which did not yet include the three-level model integrations) went on to say:

> I would now like to make some inferences concerning practical forecasting based c the above experience we have gained. I assume that practical forecasting should beg with a general baroclinic model, which should provide valid forecasts for a period of least 36 hours . . .

[31] Eady, E. T. Long waves and cyclone waves. *Tellus* **1**(3), 33–52 (1949).

[32] Smagorinsky, J. The dynamical influence of large scale heat sources and sinks o quasi-stationary mean motions of the atmosphere. *Q. J. R. Meteorol. Soc.* **79**, 342 (1953).

[33] Gilchrist, B. The seasonal phase changes of thermally produced perturbations i westerlies. *Proc. Toronto Meteorol. Conf., 1953* pp. 129–130 (1954).

[34] Minutes of the meeting held at the IAS on August 5, 1952 on the subject of *prac numerical weather forecasting*.

The problem of initiating such a program can be divided into two parts, an educational problem and a technological problem. There is an educational problem because there are practically no people available at the present time capable of supervising and operating such a program. Synoptic meteorologists who are capable of understanding the physical reasoning behind the numerical forecast are needed to evaluate the forecasts, for example. Mathematicians are needed to formulate the numerical aspects of the computations. During the first several years of the program the meteorological and mathematical aspects probably cannot be separated and personnel familiar with both aspects are needed. An intense educational program could conceivably produce enough people in about three years.

In response to an inquiry by Petterssen on the size of the forecast are: von Neumann offered:

We considered the United States the largest area at the present time with adequate data. This is probably not sufficient for the best possible 36-hour forecasts. I believe that my request for more extensive data had best follow practical demonstration of the usefulness of numerical forecasting. The memory limitations of the machine are also of importance, placing an upper limit to the data which can be handled by the machine.

The area from Japan east to eastern Europe is about four times the area we used here at Princeton. Therefore, an appreciably larger machine would be needed to forecast for such an area, especially when more complicated atmospheric models are used. This technical problem might be solved, let's say, within about five years after the program under consideration is started.

Wexler reported:

In talking to various forecasters they have given me the impression that they expect numerical forecasting methods to yield improvement in forecasts beyond 36 hours, since they believe that present methods yield sufficiently accurate 24- or 36-hour forecasts. I don't necessarily share this opinion but I think this group should be informed of its existence.

There was a generally expressed opinion that it was too early to encou age any expectations of improvement in forecasts for longer period: Charney added:

From our experience I would say that the barotropic forecasts are perhaps not as good as the best conventional forecasts, but the indications are that baroclinic forecasts will be much better.

Some discussion followed on a proposal by Philip Thompson (who wa not present) that it would be more economical to use a two-dimension: "equivalent baroclinic" model (sometimes referred to as a "therm( tropic" model) which might produce predictions just as accurate as thos from a three-dimensional model. Thompson had just completed som work on such a model, and a paper was to be published shortly.[35] At thi

[35] Thompson, P. D. On the theory of large-scale disturbances in a two-dimensional bar( clinic equivalent of the atmosphere. *Q. J. R. Meteorol. Soc.* **79**, 3–38 (1953).

point, it was a moot question because Charney's three-level results w not yet available and Thompson had only a theoretical framework. Cl ney's opinion was, "I do not believe that a very simple model will sufficient for operational forecasts." To which von Neumann added, " problem of deciding at what level to begin forecasts can be decided c by experience which we hope to have within another year."

The general consensus of this historic meeting was to gain moment rapidly. It reflected a deep commitment by the operational forecas agencies of the United States, both individually and collectively, to 1 idly build their internal competence and to accelerate their activitie numerical weather prediction.

In early 1953, I returned to the U.S. Weather Bureau to head a mo effort to begin to introduce the fruits of numerical methods into the search environment. Mr. Louis P. Cartensen was assigned to spend ] of his time with me. He already had been involved earlier, in 1949, in linear barotropic forecast calculations. Contacts with the IAS group w being maintained. In 1952 and 1953 the IAS visitors increased subs tially: Ernst Hovmöller and Roy Berggren of Sweden; Eric T. Eady Bruce Gilchrist of the United Kingdom; Kanzaburo Gambo of Ja Frederick G. Shuman, George P. Cressman, and Jacob F. Blackbur the United States; and Ragnar Fjørtoft returned.

One of the earlier concerns was the potential utility of the barocl models for forecasting precipitation. I had noted:[36]

> It is possible in principle to predict the 3 dimensional large-scale field of motio with the existing U.S. data density. To date, vertical motions of 500 mb have be predicted by means of high speed computing devices. More vertical detail in t vertical motion will be forthcoming as physical models of greater complexity a devised. However application to present operational needs without the aid of a co puter are not entirely hopeless. It seems that certain approximative schemes, in parti ular the one suggested by Fjørtoft for barotropic models, may lend themselves 1 application to a $2\frac{1}{2}$ dimensional model. If this can be done successfully then it would possible to predict the horizontal flow at 700 and 300 mb and the vertical velocity at 5 mb for 24 and possibly 36 hours by means of graphical techniques. The techni aspects of this problem are being explored presently.

In the absence of a computer, the Fjørtoft graphical method was only hope available to gain experience with the new theoretical fra works. I was to return to this problem of precipitation prediction in a years.

The research agenda for the small Weather Bureau research gr which also would include Charles L. Bristor, was intended to inc studies in objective analysis "to determine the most suitable method

[36] Memorandum, Smagorinsky to Wexler, March 4, 1953.

respect to dynamical, numerical and operational requirements." The pre-vious work by Hans A. Panofsky was to be the starting point.

The early frustration with inadequate data, especially over the oce[ans] for establishing initial conditions, seemed to be a critical weak lin[k in] what we needed to exploit the numerical models. My desperation is [ex]pressed in a memorandum[37] which grasped for a possible solution.

> One basic method of cutting costs is to automatize present sounding methods so [as] to sharply reduce the number of operating personnel. Some possible ways of doing th[is] suggest themselves, but each will take a great deal of development. The virtue of su[ch] an approach is that an automatic sounding technique would be quite adaptable f[or] oceanic observations. Anything would be less expensive than weather ships.
>
> One can even go a step further and suggest looking for an entirely new method f[or] taking soundings in addition to the process being automatic. Presently, the instrume[nt] must pass *through* the atmosphere. What about methods for indirect measuremen[t?] Isn't the density stratification on the ocean determined indirectly by sonic device[s?] Can one take advantage of the fact that radar is somewhat sensitive to variation [in] atmospheric density?
>
> It seems that development work in meteorological observations should be a contin[u]ing process not only for small modifications on existing methods but also further in[to] the future on Buck Rogerish innovations. Had this been so in the past, we no[w] probably would not be working with instruments which are basically 20 and 30 yea[rs] behind the times. It must be said that rawins are an outstanding exception.

In retrospect, a solution did not begin to emerge until the wea[ther] satellite was proposed. Actually in the 1950s the number of weather sh[ips] decreased somewhat.

In an earlier memorandum,[38] I had the opportunity to comment [on] future World Meteorological Organization (WMO) data requirements

> The recommendation that the minimum aerological density over the oceans be 10[00] km between stations would not satisfy the needs for numerical prediction. Experien[ce] indicates that an absolute minimum for any region to be treated numerically is approx[i]mately 500 km, roughly the density of Canada. Of course it is more desirable to have [a] density corresponding to that of the U.S.—a separation of about 300 km betwe[en] soundings. The above recommended minimum of 500 km assumes that there are [at] least as many winds as soundings.
>
> It appears that if upper air predictions, subjective or numerical, are worth their sa[lt] they should be reliable for at least 12 hours. Based on this assumption, it would see[m] that 6-hourly observations would be superfluous. 8 hourlies or even 12 would appear [to] be adequate. An exception would be the needs for research purposes, and perhaps o[ne] can provide for 6 hourlies for periods of a few days.
>
> Now that surface and upper air data are used together in formulating a forecast, [it] seems paradoxical that the two observation times do not coincide. Although there a[re] no doubt practical reasons for trying to avoid the crowding of the observer's schedul[e,] it would be extremely desirable that *synoptic* surface and aerological data be availabl[e]

[37] Memorandum, Smagorinsky to Wexler, May 26, 1953.

[38] Memorandum, Smagorinsky to Tannehill, March 5, 1953.

This is especially true since all upper air height calculations have their origin based c the surface pressure, so that any objective analysis procedure would need the surfac pressure in making vertical and horizontal consistency checks of the data.

These density specifications are not too different from those settled for the Global Weather Experiment of 1979. However, I was still think in terms of simultaneous observations.

In December 1953, I had the opportunity to speak at a national c( puter conference.[39] I commented that "preliminary experience indica that a general objective analysis prepared on a four-dimensional distri tion of data with widely varying density will require as many logical arithmetic operations. This problem is thus ideally suited for high sp digital computers." This implied asynoptic assimilation but withou specific proposal. I do recall an early realization that objective analy could in principle deal with data arbitrarily distributed in three-dim sional space. As a consequence, significant level data could be u instead of mandatory level data, with the attendant communicati economy.

The Weather Bureau was also thinking ahead in terms of training people needed. George Platzman was offering a concentrated 10-w course at the University of Chicago during the summer of 1953. ] Bureau sent Shuman, Carstensen, Bristor, and two others. Corresp dence from the Chicago "students" contained such messages as: "] [Weather Bureau] library is getting unhappy about some of these publi tions we brought along. . . . These publications are and have been constant use by all five of us guys" and "living expenses are low enou so that $5 per day pretty well covers them."

The possibility of operational utilization of numerical methods v much discussed. As a result of a visit to the IAS in late May 1953 reported:[40]

At the time of my visit to the IAS, Colonel George F. Taylor, Air Weather Service was also there to discuss operational numerical weather prediction. Although he wa not voicing an official opinion, Colonel Tayler said that some of the responsibl weather people at ARDC [Air Research and Development Command] in Baltimore fee that a joint operational group should be formed immediately. He also indicated tha many of the important technical problems connected with such a venture are fully within the Weather Bureau's domain. However, he was cognizant of the fiscal situa tion and thought the only reasonable arrangement was to have the military bear th bulk of the financial burden. In a discussion with Professor von Neumann and Dr Charney it was agreed that because a machine could not be obtained before 6 to 1 months, even with high priorities, a joint meeting at least progress to the point where

[39] Smagorinsky, J. Data processing requirements for numerical weather prediction. *Pr East. Comput. Conf., Washington, D.C., Dec. 1953* pp. 22–30.

an order can be placed for a machine. Colonel Taylor agreed that it is extremel desirable that any working committee which is formed by the JMC [the Joint Meteor logical Committee of the Joint Chiefs of Staff] or ACC/MET [Air Coordinating Con mittee/Meteorology] be given the authority to act rather than merely to recommend

My recommendation was that the Weather Bureau pursue its efforts ward the formation of a joint Weather Bureau–Air Force–Navy ope tional numerical forecasting group so that a commitment could be mad the earliest possible date for a high-speed calculator of the IBM 701 ty This was a somewhat faster commercial version of the IAS machine with IBM card equipment for input–output.

Developments were occurring swiftly in the dialogue on the establi ment of a Joint Operational Numerical Weather Prediction U (JNWPU). In a memorandum, Wexler wrote:[41]

On June 4 Dr. Joseph Smagorinsky, Dr. George Cressman (AWS [Air Weathe Service]) and Major Thomas Lewis (AWS) met at Andrews Air Force Base wit representatives of IBM in order to ascertain information regarding the availability c and costs of IBM Computer 701. Present indications are that because of order cance lations a machine could be available between January and June 1954. However, IB would like a letter of intent as soon as possible. The annual rental fee for the 701 i between $175,000 and $300,000 depending on the auxiliary equipment and the numbe of operating hours. . . .

On June 9 [1953] Dr. Smagorinsky presented a proposed agenda of problems to b considered by the *ad hoc* committee[42] and with minor modification the following wa adopted:

1. Functions and Organizational Structure
2. Personnel Problems (Stability, Training, Selection)
3. a. Machine Availability—letter of intent to IBM
   b. Physical Location of Unit
4. a. Initial Cost Estimate
   b. Continuing Cost Estimates
   c. Joint-Financing Arrangement

As far as Weather Bureau representation on the *ad hoc* committee is concerned, it i recommended that Dr. Smagorinsky be appointed a member.

Within 2 months, plans solidified to the point where the Bureau w making specific budgetary provisions.[43]

RECOMMENDATION

The Weather Bureau make available $32,000 in FY 1954 and $39,000 per annur thereafter to give one-third support for the Joint NWP Unit as proposed by JMC.

[41] Memorandum, Wexler to Chief of Bureau, June 11, 1953.

[42] The initial membership of the JMC ad hoc Committee on Numerical Weather Predic was Commander D. F. Rex (Chairman), Majors W. H. Best and T. H. Lewis, H. Wexler, with R. A. Allen, P. D. Thompson, and J. Smagorinsky as participants.

FACTS BEARING ON CASE

The *ad hoc* Committee on NWP appointed by JMC on June 23, 1953 at the request ( the Weather Bureau (see attached supporting paper) has formulated a plan for th establishment of a Joint NWP Unit to begin operations July 1, 1954. Some findings an recommendations which will soon be submitted to JMC are summarized below:

1. Initially, daily prognosis of the 3-dimensional atmospheric flow field will be mad 7 days per week.
2. A Staff of 34 will be required to carry out the functions planned for the first ye: of which 13 will be professional meteorologists. By July 1, 1954, the Weather Burea expects to have trained in NWP methods six of its meteorologists, the Air Force si and the Navy two.
3. For incorporation of NWP results in current analyses and prognoses it wa agreed that the NWP Unit should be located adjacent to WBAN [Weather Bureau–A Force–Navy] Analysis Center. With increased experience it is expected many of th functions now performed by WBAN Analysis Center will be absorbed by the NW Unit.
4. The Committee has agreed to recommend that administration of the Unit b assigned to the Weather Bureau.
5. Initial expenditures of $94,500 will be required four to six months prior to Jul 1954; continuing operating costs for FY 1955 will be $415,019 of which $199,559 will b for machine rental.

After submitting its final report, which formed the basis for an organ tional plan, the ad hoc Committee on NWP was dissolved on Septem 11, 1953 and on September 17, the JMC created a new ad hoc Group the Establishment of a Joint Numerical Weather Prediction Unit cons ing of Rex (Chairman), Lewis, and Wexler. This new group was emp ered to select a director, whom it would assist in implementing the org zational plan. On September 22, 1953, Dr. George P. Cressman nominated for the directorship. On October 9, 1954, Herman Golds and I were commissioned to conduct comparative tests on the two la scale computers available at the time, the IBM Type 701 and the E (Engineering Research Associates) Model 1103.[44] The 701 was rec mended, and then accepted in January 1954 by a Technical Advis Group chaired by von Neumann.

By June 30, 1954, the three participating agencies had identified t personnel contributions to JNWPU: seven each from the Weather Bur and Air Force and three from the Navy. The IBM 701 had been orde for delivery by March 1, 1955 and a site was selected at Suitland, M land. At its 15th meeting on July 1, 1954, the ad hoc Group for Establi ment of a JNWPU unanimously recommended its own dissolution also the formation of a standing steering committee, which neverthel

[44] Goldstine, H. H. "The Computer from Pascal to von Neumann," p. 329. Princ Univ. Press, Princeton, New Jersey, 1972.

was named the ad hoc Committee on Numerical Weather Predict chaired by Wexler.

The latter half of 1954 was a busy period of assembling the new gr preparing the computer program for the IAS three-level (900, 700, and mbar) quasi-geostrophic model, and setting up an operational system receiving and processing data and for disseminating results. The IBM was delivered early in 1955 and the first 36-hr forecast was produced f 1500Z April 18, 1955 initial conditions (Fig. 5).

A report of the first year's experience was published in 1957.[45]

## 5. The Advent of the General Circulation Modeling Er

Meanwhile, Norman Phillips had completed, in mid-1955, his m mental general circulation experiment.[46] As he pointed out in his pape was a natural extension of the work of Charney on numerical predict but Phillips's modesty could not obscure his own important contribut to NWP. The enabling innovation by Phillips was to construct an ener cally complete and self-sufficient two-level quasi-geostrophic m which could sustain a stable integration for the order of a month of si lated time. Despite the simplicity of the formulation of energy sources sinks, the results were remarkable in their ability to reproduce the sal features of the general circulation. A new era had been opened.

Von Neumann quickly recognized the great significance of Philli paper and immediately moved along two simultaneous lines.

One was to call a conference on "The Application of Numerical I gration Techniques to the Problem of General Circulation" in Prince during October 26–28, 1955. Of course, the centerpiece was Philli results, but many others presented papers on related research. Of part lar interest were von Neumann's published remarks on climate forec ing.[47]

The discussion centered on many questions raised in the paper mention only a few that have special historical significance. There extended discussion on the streakiness developed in the flow during latter stages of Phillips's integration. This "noodling" was the resul convolutions of the vortex lines, presumably the nonlinear result of t cation error. It already was a property of quasi-geostrophic flows.

[45] Staff Members JNWPU. One year of operational numerical weather prediction. *Am. Meteorol. Soc.* **38,** Part I, 263–268; Part II, 315–328 (1957).

[46] Phillips, N. A. The general circulation of the atmosphere: A numerical experin *Q. J. R. Meteorol. Soc.* **82,** 123–164 (1956).

[47] Pfeffer, R. L., ed. "Dynamics of Climate." Pergamon, Oxford, 1960.

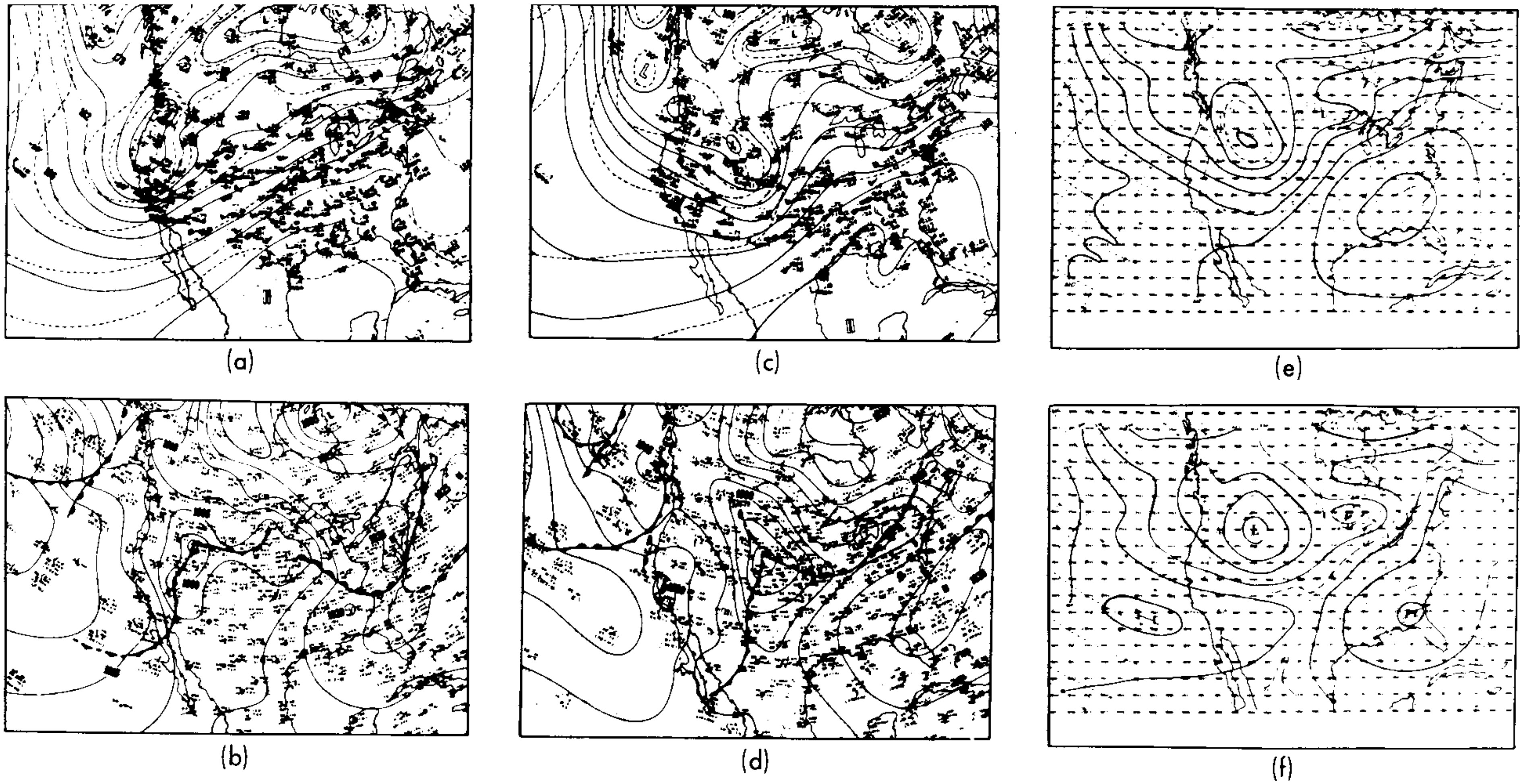


FIG. 5. The first operational three-level numerical forecast by JNWPU from 15 Z April 18, 1955. Shown are observed maps from April 18 at (a) 500 mbar (15 Z) and (b) sea level (1230 Z) and also for April 19 (c) and (d). Shown also are the 24-hr forecast results at (e) 700 mbar and (f) 900 mbar. Actually a 36-hr forecast was made for 900, 700, and 400 mbar and vertical motions produced at 800 and 550 mbar.

small-scale eddy viscosity should properly represent the energy (and strophy) exchange between the explicitly represented modes and the tr cated smaller (subgrid) scales so as to preserve the spectrum in the vic ity of the limit of computational resolution. Charney recalled \ Neumann and Richtmyer's experience with shock-wave flows[48] in wh they found that a nonlinear viscosity served to preserve the scale of shock wave during the course of its propagation. In their paper they s "the dissipation is introduced for purely mathematical reasons." The used terminology that the viscosity is "artificial," in my view, give misleading connotation. Surely, an eddy viscosity in any form is an a fice to replace a reality missing from the finitistic representation, that the real spectral communication between the explicity resolved flow the molecular range where a physical viscosity does its work. Charn and Phillips recommended that I apply such a nonlinear viscosity to primitive equation general circulation model later on. I am often credi with the original idea, but it belongs to others; I only used it and ratior ized it.

Another area of discussion centered around the prediction of precip tion. I had, at that time, been in the midst of diagnostic calculations precipitation from a vertical velocity field[49] and also was trying to sh the effect of released latent heat in positively feeding back to amplify vertical motion and reduce the horizontal scale.[50] I was also working the formulation of a water vapor predictive framework to be incorpora in a numerical model.[51] Other researchers in Japan and the United Sta were also working on the problem at the time, making important contri tions.[52-55]

[48] von Neumann, J., and Richtmyer, R. D. A method for the numerical calculatio hydrodynamic shocks. *J. Appl. Phys.* **21,** 232–237 (1950).

[49] Smagorinsky, J., and Collins, G. O. On the numerical prediction of precipitation. *M Weather Rev.* **83**(3), 53–68 (1955).

[50] Smagorinsky, J. On the inclusion of moist adiabatic processes in numerical predic models. *Ber. Dtsch. Wetterdienstes* **38,** 82–90 (1957).

[51] Smagorinsky, J. On the dynamical prediction of large-scale condensation by nume methods. *Am. Geophys. Union, Mongr.* No. 5, pp. 71–78 (1960).

[52] Kombayasi, M., Miyakoda, K., Aihara, M., Manabe, S., and Katow, K. The quan tive forecast of precipitation with a numerical prediction method. *J. Meteorol. Soc.* **33**(5), 205–216 (1955).

[53] Miyakoda, K. Forecasting formula for precipitation and the problem of conveyanc water vapor. *J. Meteorol. Soc. Jpn.* **34**(4), 212–225 (1956).

[54] Estoque, M. A. "An Approach to Quantitative Precipitation Forecasting," Sci. I No. 7, Contract AF19(604)-1293 between University of Chicago and G.R.D., AFCRC, 1

[55] Aubert, E. J. On the release of latent heat as a factor in large scale atmospheric moti *J. Meteorol. Soc.* **14**(6), 527–542 (1957).

V.N.'s 1st Draft
July 29, 1955

DYNAMICS OF THE GENERAL CIRCULATION

title suggested by HW at lunch table with V.N. + J. Char

1. In 1947, a project was started in Princeton by the U.S. Navy and U.S. Air Force, for theoretical and computational investigations in meteorology, with particular regard to the development of methods of numerical weather forecasting. After a few years of experimenting, the project concentrated on exploring the validity and the use of the differential equation methods developed by Dr. J. Charney for numerical forecasting. For this purpose, the U.S. Army Ordnance Corps ENIAC computing machine was used in 1950 and in 1951, and the Institute for Advanced Study's own computing machine from 1952 onward. Subsequently, use was also made of the IBM 701 machine in New York City. With the help of these computing tools, it was found that forecasts over periods like 24 (and up to 48) hours are possible, and give significant improvements over the normal, subjective method of forecasting. Certain experiments demonstrated that even phenomena of cyclogenesis could be predicted. A considerable number of sample forecasts were made, which permitted the above mentioned evaluation of the validity of the method. A large number of variants were also explored, particularly with respect to eliminating successively the major mathematical approximations that the original method contained. It must be noted, however, that the method, and also all its variants which exist at the present, are still affected with considerable simplifications of a physical nature. Thus, the effects of radiation have only been taken into consideration in exceptional cases, the same is true for the effects of geography and topography, while humidity and precipitation have not been considered at all. That significant results could, nevertheless, be obtained, is due to the relatively short time-span of the forecasts. Indeed, over 24 or 48 hours the above mentioned effects do not yet come into play decisively.

FIG. 6. The first page of a draft proposal by J. von Neumann to establish a project on dynamics of the general circulation—eventually to become the Geophysical Fluid Dynar Laboratory. The written changes in the text are von Neumann's and notes in the upper ri hand corner are by Harry Wexler.

The specific problem of discerning marginal cloudiness which doe yield precipitation was identified as a difficult and as yet unresolved p lem. It still is unsolved! I recall that sometime in the early 1950s, Neumann, I, and several others were standing outside of the Electr Computer Project Building in Princeton, and Johnny looked up at a tially cloudy sky and said, "Do you think we will ever be able to pr that?" In an attempt to answer that question, I had shown in my paper that an empirical correlation can be found between the large- fields of relative humidity and cloud amount for three layers in the tr sphere.

Finally, I mention without further comment that the conference dis sion also touched on the $CO_2$ cycle, but without exciting much conc

This meeting did much to coalesce thinking on problems and oppor ties that lay ahead, from all perspectives: observational, theoretical, experimental.

The other initiative by von Neumann was stimulated by his realiza that the exploitation of Phillips's breakthrough would require a large, separate, and dedicated undertaking. He followed a path simil the one he took 2 years earlier in connection with establishing JNWPU. Von Neumann drafted a proposal to the Weather Bureau, Force, and Navy justifying a joint project on the dynamics of the gen circulation. Because of its historical interest, it is reproduced here i entirety together with a photograph (Fig. 6) of the first page of an ea draft with von Neumann's and Wexler's handwritten changes and c ments.

### Proposal for a Project on the Dynamics of the General Circulation

1. In 1947 a project was started in Princeton by the U.S. Navy and U.S. Air For for theoretical and computational investigations in meteorology, with particular rega to the development of methods of numerical weather forecasting. After a few years experimenting, the project concentrated on exploring the validity and the use of t differential equation methods developed by Dr. J. Charney for numerical forecastir For this purpose, the U.S. Army Ordnance Corps ENIAC computing machine w used in 1950 and 1951, and the Institute for Advanced Study's own computing machi from 1952 onward. Subsequently, use was also made of the IBY [*sic*] 701 machine New York City. With the help of these computing tools, it was found that foreca over periods from 24 to 48 hours are possible, and give significant improvements ov the normal, subjective method of forecasting. Certain experiments demonstrated th even phenomena of cyclogenesis could be predicted. A considerable number of sam forecasts was made, which permitted the above-mentioned evaluation of the validity the method. A large number of variants was also explored, particularly with respect eliminating successively the major mathematical approximations that the origir method contained. It must be noted, however, that the method, and also all its variar

physical nature. Thus, the effects of radiation have only been taken into considerati in exceptional cases, the same is true for the effects of geography and topograph while humidity and precipitation have not been considered at all. That significa results could, nevertheless, be obtained is due to the relatively short-time span of t forecasts. Indeed, over 24 or 48 hours the above-mentioned effects do not yet cor into play decisively.

On the basis of the results cited it was determined by the sponsoring agencies tha routine 24–36 hour numerical forecasting service has become possible, and should set up on a permanent basis. This was done by a joint organization of U.S. Air Forc U.S. Navy, and Weather Bureau (JNWPU—Joint Numerical Weather Predicti Unit) which is being operated by the U.S. Weather Bureau at Suitland, Md. It has nc been making daily forecasts for over 3 months, and with very good success.

2. The logical next step after this is to pass to longer-range forecasts and, mo generally speaking, to a determination of the ordinary general circulation of the terre trial atmosphere. Indeed, determining the ordinary circulation pattern may be view as a forecast over an infinite period of time, since it predicts what atmospheric con tions will generally prevail when they have become, due to the lapse of very long tir intervals, causally and statistically independent of whatever initial conditions m have existed.

There is reason to believe that the above-mentioned "infinite" forecast, i.e., deri ing the general circulation, is less difficult than intermediate length forecasts, say, to or 90 days. This is just a reflection of the fact that extreme cases are usually easier treat than intermediate ones, since in extreme cases only a part of factors plays a rol dominating all others, while in intermediate cases, all factors become of comparat importance. It should be added that both the "infinite" and the "intermediate" foı casts have to be performed for the entire earth, or at least for an entire hemisphei Indeed, the spread of meteorological effects is such that, already after 2 to 3 week every part of the terrestrial atmosphere will have interacted with every other—exce for the relative weakness of the interaction between the Northern and Southern Hen spheres. Thus, in both cases, a hemispheric forecast is the minimum that can envisaged.

In view of the above, it seems logical to investigate now the "infinite" forecast, i. the general circulation. It is hoped that this will subsequently lead to a better unde standing of the factors involved in the "intermediate" forecasts (compare above Thus, the "intermediate" forecasts should enter into the program at a somewhat lat stage.[56]

3. With regard to calculating the general circulation in the Northern Hemispher quite significant progress was made in Princeton. Several calculations were made which the Northern Hemisphere—or rather a quadrant of it—was treated in a high simplified way. The simplifications were as follows: The quadrant of the hemisphe was treated as a "flat" area, thus distorting the geometry, primarily in the Arcti considerably. (It was treated with a "periodic" east–west boundary condition, i. the calculation deals with "planetary waves" of wave number 4 (or 8, 12 and so on Instead of using a coriolis parameter with its proper meridional variability, t "Rossby plane" was used, i.e., the coriolis parameter was given its mean value, a treated as a constant; however, in all places where the exact theory makes reference

[56] And that is how it did happen. Intermediate-range forecasting did not begin unti middle and late 1960s after experience had been accumulated with general circulation librium experiments.

the meridional derivative of the coriolis parameter, the (positive) mean value of that quantity was used.

The solar radiation impinging upon the earth was considered without its seasonal or diurnal variations. Indeed, it was treated as a heat source with a linear meridional variation. This model was treated on a horizontal 16 × 16 lattice, with two vertical strata. Starting with an atmosphere at rest, the integration was carried out over 30 days.

The effects of humidity, and of geography and topography, were disregarded. The calculations on this model were started with an atmosphere at rest, and at a uniform temperature. The developing motions and adjustments were calculated over a period of 30 "real" days. The circulation pattern which developed was first the one that one usually obtains by verbal discussion: Northward flow of heated air aloft, and southward flow of cooled air below, with easterly winds on the lower, and westerly winds on the upper level. This (not real!) flow was observed to pass its turbulent stability limit after 5 "real" days. At this point, its breakdown was induced by adding (computational) "noise" to the motion. Hereupon, in the course of the next 25 days a cyclone and an anticyclone, of familiar type, developed with westerlies in the lower level was about 30 miles per hour, and on the high level the maximum westerly velocity reached 200 miles per hour. The temperature difference between the tropics and the Arctic was, as it should be, about twice what it is in reality. (This doubling should correspond to the fact, that in reality half the heat transported north is latent heat of humidity, hence, when this contribution is neglected, the temperature increment that is needed to take care of all the requirements, will be double of what it is in reality.)

Thus, even this very primitive model disclosed the main features of the general circulation, in a rather detailed way, which no verbal, or less elaborate computational, analyses have ever been able to do. Several calculations of this type were made, that gave concordant results, and also disclosed the limitations of the method used. The above described calculation (repeated for checking) required 30 computing hours on the Princeton machine.

4. It seems clear that these general circulation calculations should now be expanded and improved. Even applying only the obvious mathematical and geometrical improvements will greatly increase the size of each calculation. As a minimum program, the entire Northern Hemisphere should be considered; its curvature and the meridional variation of the coriolis parameter should be properly treated; and the meridional variation of the solar energy input, with or without its seasonal or diurnal variations, should be introduced into the calculation. In addition to this, we know that the optimum grid size is about twice as fine (in linear dimension) than what was used, and that one should properly consider 3 or 4 vertical levels (rather than 2, compare above). All of this, with various secondary complications that it induces, is likely to increase the size of the calculation, allowing for reasonable improvements, by at least a factor of thirty. This would mean a problem time of about 900 hours on the Princeton machine, or if the problem is checked in a less-time-consuming way than by repetition, 450 hours.

Comparing the Princeton machine with the IBM 701, it appears likely that the latter will be about 5 times faster on this problem. (The intrinsic speed of the IBM 701 is only twice that of the Princeton machine, but various memory limitations of that machine probably increase this factor for the problem under consideration, to something like 5.) Thus, on the IBM 701, presumably about 90 hours would be needed per problem, allowing for the above indicated refinements. This means that the time on the IBM 704 would probably be about 45 hours, and on the NORC, perhaps 22 hours.

Since a research program of this type requires large scale experimentation, with computing methods, with variations of parameter, and physical approximations of various kinds, there is no doubt that in any rational program, a large number of such problems will have to be solved. Therefore, even the best time mentioned above (22 hours on the NORC) would not be too fast, i.e., even under these conditions, computing would probably take more time than analyzing and planning. This is increasingly true for the IBM 704 and the IBM 701. Consequently, the use of the IBM 701 or, if feasible, of one of the faster machines would be, in principle, amply justified.

5. It is, therefore, proposed to set up a project which has available to it at least a machine of the IBM 701 type. Since the first improvements and refinements on the problem are sufficiently understood today, to be put immediately into the phase of mathematical planning and coding, it would be important to think in terms of a machine which can be made available soon. The only machine of this speed clâss which is immediately available is the IBM 701. While this machine exists today in about 20 copies, only a few of them have easy access. At this moment, neither Princeton nor New York offer such a possibility, whereas one exists in Washington, at the Suitland establishment of the U.S. Weather Bureau (the JNWPU referred to earlier). It would, therefore, be very profitable to initiate measures immediately which make it possible to use this machine for the calculations mentioned above.

The obvious vehicle for this work would be a project organized around the Suitland machine, and with the advice and collaboration of those who directed the Princeton project, and the above circulation calculations—J. Charney, N. Phillips, and J. von Neumann—readily available.

It is proposed that such a project be set up at the U.S. Weather Bureau to be located at Suitland, with adequate personnel, physical space and facilities, and with about one shift of the Suitland IBM 701 machine available. It is proposed that within the Weather Bureau organization, Dr. H. Wexler, who has considerable familiarity with this work, be made the project officer. It is contemplated that in scientific and policy matters he would be guided by the decisions of a committee to consist of J. Charney, J. von Neumann, and himself.

6. The progress of this project can now be mapped out for about two years. During the first year, the general organization of personnel and facilities should take place, the setting up of computing methods in the sense of the "minimum improved" general circulation problem, as outlined above, and the carrying out of a sufficient sample of calculations on this basis. In the second year, the obvious physical improvements should be gradually introduced into the treatment. As such, one would consider in order of increasing difficulty the introduction of the following factors:

(a) Purely kinematic effects of geography and topography;
(b) Acquisition of humidity in the atmosphere by evaporation. This necessitates the (geographical) consideration of position of the oceans. It also requires the introduction of, presently reasonably well understood, semi-empirical rules regarding the dependence of the rate of evaporation on the local atmospheric and oceanic temperatures, atmospheric stability, and wind velocity.
(c) Some, as yet imperfect semi-empirical rules about the delay-relationships of over-saturation, cloud formation, and precipitation. Also, some semi-empirical rules about the absorption of solar radiation by clouds.
(d) The very difficult problem of the effects of atmospheric humidity on the solar irradiation of the earth and on the long wave radiation from the earth and atmosphere.

It is worth repeating that (d) is an extremely difficult problem, which will probably only be reached at the end of the two year period, and on which progress will only be made at still later stages, and then only in combination with a great deal of theoretical and experimental work, some of which is now under way. (c) is, in principle, even more difficult, but in this case, acceptable practical approximations can probably be made. (a) is quite simple; (b) while not very simple, is nevertheless based on things that we understand reasonably well at present. Personnel and budget for the project are envisaged as follows:

| | | | | |
|---|---|---|---|---|
| 1 | Meteorologist-in-Charge | GS-14 | $10,750 | |
| 2 | Meteorologists | GS-13 | 18,840 | |
| 1 | Senior Programmer | GS-12 | 8,000 | |
| 1 | Synoptic Meteorologist | GS-11 | 7,035 | |
| 1 | Programmer | GS-11 | 7,035 | |
| 2 | Programmers | GS-9 | 11,690 | |
| 2 | Electronic Computer Operators | GS-7 | 9,860 | |
| 2 | Meteorological Aids | GS-5 | 8,150 | |
| 1 | Clerk-Typist | GS-4 | 3,670 | |
| | | Personnel | | $85,030 |
| | Travel | | 4,000 | |
| | Consultants | | 5,000 | |
| | Computing Machine Time | | 162,000 | |
| | Other equipment and office furniture | | 6,000 | |
| | | Other | | $177,000 |
| | | GRAND TOTAL | | $262,030 per annum |

Total for 9 months 1 October 1955 to
30 June 1956 is $196,521
or shared by three 65,507 each

It should be noted that the above figures apply to the first year only. They should be reconsidered at the end of that year, and the budget of the second year determined on the basis of the experiences gained in the first year. It is expected that the latter will not differ very significantly from the budget of the first year on an annual basis, but that it will probably be somewhat higher.

At the end of the first year we may also find that a faster machine than the IBM 701 is becoming available.

In addition to the above, the consultations with J. Charney, N. Phillips, and J. von Neumann (without compensation) will be needed.

The proposal, dated August 1, 1955, was more or less accepted t following month as a joint Weather Bureau–Air Force–Navy venture was asked to lead the new General Circulation Research Section,[57] a

[57] This group subsequently changed its name several times: General Circulation Resea Laboratory (1959) and Geophysical Fluid Dynamics Laboratory (1963).

reported for duty on October 23, 1955. By the end of the year there five of us.

In a recent biography of von Neumann[58] it was asserted:

> One of von Neumann's interests was in weather modification and he participatec a panel on "possible effects of atomic and thermonuclear explosions in modify weather." Von Neumann's most interesting conclusion was that the most likely wa affect the weather and climate is the possible modification of the albedo of the ea Thinking had moved toward the question of how might we change the weather at w Von Neumann thought that the evidence so far was that nuclear explosions had o negligible effects on the weather, but that more theoretical and computer studies needed, like the ones he and Jules [*sic*] Charney had initiated at Princeton.

One wonders whether this was a motivation in his proposal to form new project. If so, it was not readily apparent to me at the time.

In the spring of 1955, von Neumann left the Institute to take up o the posts of Commissioner of Atomic Energy in Washington, D.C. until the time he fell ill in 1956, von Neumann kept in close contact me concerning the work of our group. He died in February 1957 at th of 53. As a result of von Neumann's departure, the Institute's l indifference to the Computer Project ultimately resulted in Charney's Phillips's move to MIT in 1956. A consequence was that the Air F and Navy withdrew further support from our group in Suitland, Mary and the Weather Bureau assumed full responsibility, thanks to Franci Reichelderfer and Harry Wexler.

However, in the brief interval of our close cooperation with the group, they were instrumental in getting us started on a fruitful lin research. We already were busy with the precipitation problem. In case of general circulation modeling, it seemed the next logical step yond Phillips's model was to allow nongeostrophic modes which coul of great significance in how the tropics operated in, and interacted v the general circulation. Some new work by Arnt Eliassen[59] at UC seemed the logical starting point. He did not use the full primitive e tions, but allowed only internal gravity waves by constraining the sur pressure tendency to vanish. The domain was a zonal channel on a sp cal earth, with one boundary at the equator. A nonlinear lateral visc of the von Neumann–Richtmyer type was formulated with the hel Charney and Phillips. Other aspects of the model were quite simila that of Phillips. The two-level model required that the static stabilit

[58] Heims, S. J. "John von Neumann and Norbert Wiener, from Mathematics t Technologies of Life and Death." MIT Press, Cambridge, Massachusetts, 1980.

[59] Eliassen, A. "A Procedure for Numerical Integration of the Primitive Equations o Two-Parameter Model of the Atmosphere," Sci. Rep. No. 4 on Contract AF19(604)-

entered as an externally specified parameter which could be adjusted as to crudely take into account the mean effect of released latent hea

The model and integration scheme were described at an internatio NWP conference in Stockholm in June 1957.[60] Stable integrations for extended period of 60 days were achieved soon after, and our very f results were exhibited by Harry Wexler at the 5th General Assembly CSAGI in Moscow in July–August 1958. By December 1958, at a sym sium of the American Association for the Advancement of Science Washington, D.C., I was able to show the model's ability to sustain index cycle with the attendant fluctuation in the energy and moment fluxes. This property was already suggested by Phillips's results.

The long lapse between this stage and final publication in 1963[61] was result of a personal desire to first perform thorough analyses of the n geostrophic modes and of the energetics. In retrospect, it was a mark immaturity that I decided not to publish the results in several intermedi stages but rather at the end as a comprehensive work.

As an important historical aside, it should be said that Hinkelma published in 1959[62] the results of a numerical experiment with the pri tive equations from which sound waves and external gravity waves a were filtered. Friction, nonadiabatic effects, and orography were glected. This was a five-level model which was stably integrated fo days from idealized initial conditions. Hinkelmann must have started t work about the same time as we did.

In late 1958, encouraged by our success with the two-level model, began to design a nine-level primitive equation hemispheric model. I private discussion, Charney expressed skepticism on the value of many levels. This model would admit external gravity waves and exte high enough into the stratosphere to account for significant energe coupling with the troposphere. The model would have a general radiat algorithm, predict water vapor transport and condensation, and incor rate a convective parameterization and an explicit boundary layer. T model was to be the prototype for much of the laboratory's work in fut years. In 1959 we began to pass our experience on to JNWPU. Tl started their own line of research and although they felt ready to ha

[60] Smagorinsky, J. On the numerical integration of the primitive equations of motion baroclinic flow in a closed region. *Mon. Weather Rev.* **86**(12), 457–466 (1958).

[61] Smagorinsky, J. General circulation experiments with the primitive equations. I. basic experiment. *Mon. Weather Rev.* **91**(3), 99–164 (1963).

[62] Hinkelmann, K. Ein Numerisches Experiment mit den Primitiven Gleichungen. "The Atmosphere and the Sea in Motion—The Rossby Memorial Volume" (B. Bolin, e pp. 486–500. Rockefeller Institute Press, New York, 1959.

launched a primitive equation operation in 1961, inadequate computir power delayed such an operation until 1966.[63]

Arrangements were made for a new computer to be delivered in 1962. was the IBM 7030, or "Stretch," which was to be about 40 times fast than the IBM 701.

In October 1959, Syukuro Manabe joined our group. He was to becon my close collaborator in this massive enterprise, eventually becoming tl leader of our growing general circulation modeling group. In this ne modeling venture, we were to be generously assisted by research scie tists working for IBM. In 1960, Yale Mintz invited me to lecture about o two-level results at UCLA, and I also talked in detail about our ne model and some of our earlier experience with it.

We had decided to test a three-level version of the new model in a octagonal domain with real initial conditions derived from Hinkelmann analysis for January 22, 1959, with the initial divergence set at zero. Tl model was stripped of condensation, mountains, and friction. Twent four forecast results were shown in November 1960 at the Internation Symposium on Numerical Weather Prediction in Tokyo. The errors we comparable in magnitude and distribution to those of a forecast l Hinkelmann. Our results were never published. This is one of sever examples in my career of a paper that should have been published, b was not. But conversely, I can think of some that should not have be published, but were!

It was in 1960 that I decided that we should consider getting involv with the oceans, for two reasons. First, the techniques we were develo ing seemed transferable even though a theoretical framework for tl oceans comparable to that of the atmosphere was lacking. The oth reason was that it was clear that long-term evolutions of the atmosphe and its climatic properties could not be understood without understandi the interaction with the oceans. It was for this reason that Kirk Bry joined our group in March 1961.

We see then that in the late 1950s, the field of climatology was rapid on its way to being transformed from a branch of descriptive geography one of quantitative physical science.

## 6. Epilogue

I am approaching the limit of the scope that I intended for this accou Although it may seem to be a rather arbitrary stopping point, subseque

[63] Shuman, F. The research and development program at the National Meteorologi Center, NOAA (an internal unpublished report), 1972.

developments, from 1960 onward, both in numerical weather predictio and in general circulation and climate modeling, have been exponential i their growth and significance. I doubt whether my own knowledge coul do justice to the breadth of achievements. Many of my views in the earl 1960s were expressed in a Symons Memorial Lecture.[64]

Today, we still encounter problems that we thought had already bee solved in the 1950s. The continuing central problem is that of systemati cally building a framework of understanding. Jule Charney's origina strategy of constructing a hierarchy of models is still quite sound. But a models become more complex, it is difficult, with highly nonlinear an interactive processes, to say why we obtain a given result. There hav been many disturbing examples of a result being apparently correct bu for the wrong reason. Series of well-designed experiments must be em ployed to delineate cause and effect. For this purpose, thorough diagnos tic techniques must continue to be developed and applied. One must als be prepared to go backward, hierarchically speaking, in order to isolat essential processes responsible for results observed from more compre hensive models.

There now are a tremendous number of scientists throughout the worl engaged in modeling research. In contrast, at the International Confer ence in Stockholm in 1957, most of the world's expertise occupied abou 40 seats.

[64] Smagorinsky, J. Some aspects of the general circulation. *Q. J. R. Meteorol. Soc.* **90**, 1–14 (1964).