problems and promises of deterministic extended range forecasting[1]

Joseph Smagorinsky
Geophysical Fluid Dynamics Laboratory
ESSA
Princeton, N. J.

Abstract

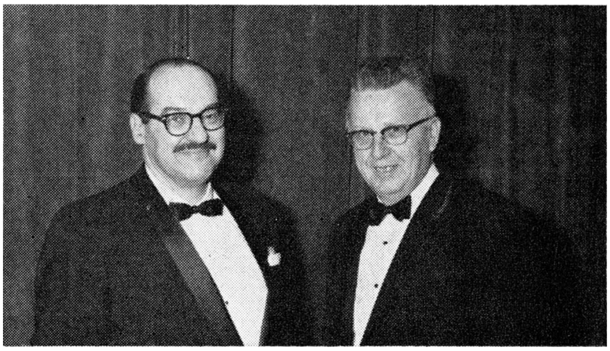
The past 20 years have encompassed remarkable scientific and technical advances in the atmospheric and oceanic sciences which herald a new era for deterministically predicting atmospheric behavior. Many of the key innovations were directly influenced, if not originated by Harry Wexler during his very productive career. This paper will deal with a critique of recent progress in modelling the atmosphere-ocean system, some of the newly exposed problems, and the needs and expectations for the future.

1. Antecedant foundations

An account of the history of long range forecasting was definitively documented by Namias (1968) in last year's Wexler Memorial Lecture. I can think of no one better qualified to have done the job. Namias is, after all, the world's foremost practitioner of the art, and no dilettante, such as I, can possibly challenge the key insights he has derived from many years of experience on the firing line. However, in his lecture, he did touch upon some of those areas which will form the main focus of my discourse this evening. In this overlap, I may from time to time explicitly or otherwise take exception to Namias' expectations for the future, if not in kind, at least in degree. I know that Harry Wexler would have been pleased that this lecture series in his honor provided a platform for a dialogue.

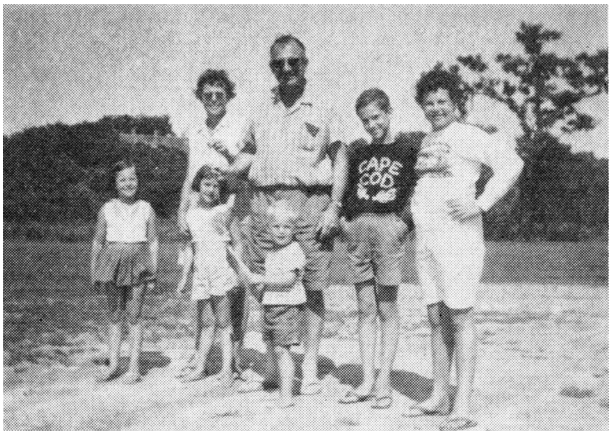
If it is not clear already, I think I should explain why the subject of my talk is appropriate to Wexler Lecture Series. Perhaps this is best done by briefly recounting some of Harry Wexler's initiatives and the chain of events that contributed to the developments I will speak of today.

In 1946, he, von Neumann and Rossby decided to establish a meteorology group at the Institute for Advanced Studies' Electronic Computer Project. The success of this venture is now legend. One of the immediate consequences was a decision in 1953 to form an operational forecasting group in Suitland called the Joint Weather Numerical Prediction Unit. At the beginning it resided in Wexler's research organization at the Weather Bureau.



Dr. Joseph Smagorinsky, Wexler Memorial Lecturer, and AMS President Verner E. Suomi.

[1] Wexler Memorial Lecture, presented at the American Meteorological Society 49th Annual Meeting, 20 January 1969, New York, N. Y.



Inscribed on the back in Harry Wexler's hand "HW & Smog family, 20 July 62, Woods Hole."

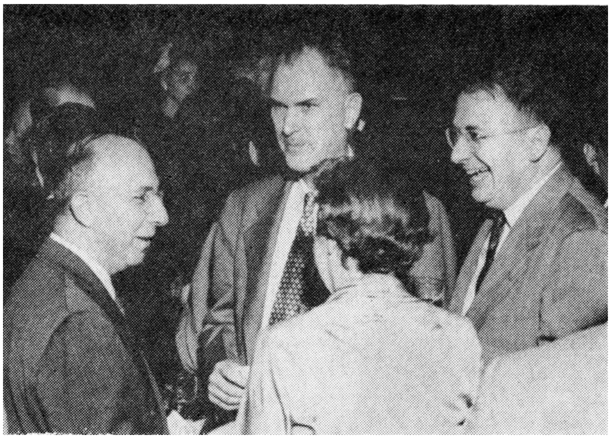


FIG. 15. Doctors H. E. Landsberg, C. W. Thornthwaite and Harry Wexler on the occasion of Dr. Wexler's receiving the Career Service Award from the National Civil Service League in Washington, D. C., 21 March 1961. (The woman in the forefront is unknown.)

A further outgrowth of these successful activities at the Institute was the recognition of the timeliness to bear down on the problem of understanding the atmospheric general circulation. Here again, Harry Wexler was the prime mover. Under his direction the General Circulation Research Section was established in the Weather Bureau in 1955. It was the forerunner of the Geophysical Fluid Dynamics Laboratory.

Harry Wexler's broad ranging awaredness of necessity and action does not only lie with numerical prediction. The early barotropic forecasts at Princeton already demonstrated the crucial importance of adequate aerological data, particularly over the oceans. It was through his perserverance that the Weather Bureau established in the early 1950's a merchant vessel program for the routine aerological sounding of the atmosphere over the oceans. This still remains one of the most important segments of our woefully inadequate aerological network.

In the early 1960's, as a result of the United Nations resolutions on the peaceful exploitation of outer space, Wexler and V. Bugaev of the Soviet Union laid the ground work in a fundamental document for the use of satellite platforms for the surveillance of the atmosphere.

In these events lie the foundations for the World Weather Watch and the Global Atmospheric Research Program.

Now I wish to return to 1955. In October of that year, a conference was held in Princeton on the Dynamics of Climate (Pfeffer, 1960). This conference, I would say, was largely precipitated by Phillips' pioneering numerical experiment in the simulation of the general circulation. Harry Wexler, of course, was there. With characteristic perception, von Neumann remarked "In considering the prediction problem, it is convenient to divide the motions of the atmosphere into three different categories depending on the time scale of the prediction. In the first category, we have motions which are, in the main, determined by the initial condition—in which we may extrapolate the initial tendencies over a short period of time. In the second category, we have the opposite extreme, namely, motions which are practically independent of the initial condition. In attempting to forecast such motions, we concern ourselves with traits of the circulation, which on the average, will always be present." . . . "Now, between the two extreme cases, there is another category of flows. In this category, we are sufficiently far from the initial state so that the details of the initial conditions do not express themselves very clearly in what has developed. There can be no question, therefore, of calculating backwards from the ultimate conditions to the initial conditions. Nevertheless, certain traits of the initial conditions bear a considerable influence on the form which the circulation takes." . . . . "It seems quite plausible from general experience that in any mathematical problem it is easiest to determine the solution for shorter periods, over which the extrapolation parameter is comparatively small. The next most difficult problem to solve is that of determining the asymptotic conditions

—that is, the conditions that exist over periods for which the extrapolation parameter is very large, say near infinity. Finally, the most difficult is the intermediate range problem, for which the extrapolation parameter is neither very small nor very large. In this case the neglect of either extreme is forbidden. On the basis of these considerations, it follows that there is a perfectly logical approach to any computational treatment of the problem of weather prediction. The approach is to try first short-range forecasts, then long-range forecasts of those properties of the circulation that can perpetuate themselves over arbitrarily long periods of time (other things being equal), and only finally to attempt to forecast for medium-long time periods which are too long to treat by simple hydrodynamical theory and too short to treat by the general principles of equilibrium theory."

In making these remarks von Neumann associated time spans of 30 to 180 days with the medium range forecasting problem. Actually much of what he had to say is applicable to even shorter periods. In fact I would prefer to extend his lower limit from 30 days down to 2 or 3. His suggested approach as to the order of doing things in tackling the problem has remained essentially valid and intact. This is just another tribute to his remarkable intellect. Indeed, it was the application of general circulation models to the extended range problem that made the difference.

It is of interest that some detractors of large scale modelling research claim that it is not really relevant to the practical mid-latitude forecast problem. They argue that there is sufficient energy in the meso-scale to obscure the practical significance of the large-scale. I think such pessimism is overstated and unwarranted. It is quite likely that the forced components of the meso-scale will be physically predictable for extended periods. For example there are those excited by small scale orography and coastal thermal contrasts. On the other hand the deterministic predictability of the free meso-scale components are probably of the order of a few hours so that we would need parameterizations to statistically relate their variability to that of the large scale. As we know from attempts to devise such parameterizations to model subgrid scale effects on the large scale, this is not yet a completely solved problem.

This naturally carries me to the entire notion of a deterministic threshold for the atmosphere.

2. On the theoretical limit of predictability

Over the past few years we have heard a great deal about "predictability" and even if the notions involved seem a bit fuzzy, it has become customary to hear speak of "2 week prediction."

My concern is that many of the people doing the speaking may not be entirely clear in their own minds as to what it all means. What is worse is that the listeners get an even fuzzier idea of what it is all about, and may be hearing only what they want to hear.

Some of the first clear expressions of an inherent limit of predictability for the atmosphere came in a paper by P. D. Thompson in 1957. In a rather simple analysis, he demonstrated that a limit should exist, and concluded that "a week-long prediction is no better than a sheer guess." Lorenz's papers of 1963 and 1965 sharpened the insights, attributing the deterministic limit to the atmosphere's admissible physical instabilities and also its inherent non-linear and dissipative character. He showed very simply and convincingly that such physical systems can depart from slightly different initial states to flows which are ultimately randomly related. Remarkably, Lorenz accomplished this with models possessing only 28 degrees of freedom. Nevertheless he was able to make an order of magnitude estimate of the limit of synoptic scale predictability to be "a few days to a few weeks." The next step in this chain of inquiry occurred when Charney (National Academy of Sciences, 1966a) suggested the application of the then general circulation models to study the departure of the two different initial states. Based on such experiments and on an estimate of the linear growth rate of baroclinically unstable disturbances in mid-latitudes, he concluded that limit of predictability imposed by typical observational errors to be about two weeks. One must also cite the estimates published recently by Robinson (1967). Assuming certain empirical dissipation characteristics of the atmospheric energy spectrum, his conclusion was far more pessimistic than any of the others. But the tools he used were more crude.

None-the-less, all of these studies by somewhat different means, demonstrated the same principle and the same qualitative characteristics of the atmosphere's deterministic limit. Quantitatively, however, the numerical values of this limit were tentative, but useful in their time. In meteorology, we are often content with order of magnitude estimates. However, in the case of predictability limits, it is of practical significance whether the limit is one week or four weeks. In fact it does not take too much convincing that even a factor of two can alter one's perspectives.

To pursue this idea further, let us pose the following question: If we had

1) a physically faithful model of the real atmosphere, and
2) an ability to fully specify the initial conditions for all spectral components, and
3) committed no truncation error in numerically integrating the system of non-linear differential equations,

then could we predict the atmospheric evolutions from the initial time with infinite precision infinitely distant into the future? Or, would the flutter of a butterfly's wings ultimately amplify to the point where the numerical simulation departs from reality, so that there will come the time when they must be randomly related to each other? Would all spectral modes become uncorrelated at the same rate? If not the flutter of the butterfly's wings, the disturbance might be the result of instrumental errors in the initial conditions or of round-off errors in the numerical integrations.

Such questions are not merely of academic interest. Our answer would inform us as to the *ultimate limit* of predictability to which we can aspire. Beyond that point, no further effort to specify initial conditions, nor to construct more realistic models, nor to build faster computing machines could yield an increase of predictability of a *given scale* of atmosphere variability.

These questions are not easy to answer, but in principle can be approached if one is in possession of models that have displayed an ability to reproduce the essential characteristics of the real atmospheric energy cycle; i.e.:

1) the transformation of the zonal available potential energy (which is produced by the latitudinal radiation gradient) into kinetic energy,
2) the non-linear spectral exchange of energy, and
3) the dissipation of this energy by friction.

We shall follow Charney's proposed approach. The principle is simple: a given simulation with such a model for an extended period, is taken to be characteristic of a real atmospheric evolution. This simulation is interrupted at some point and some of the variables slightly disturbed in some way (the flap of the butterfly's wings). One then observes how the two experiments depart from each other. Such an experiment simulates the growth of errors in the initial conditions. The errors could have been due to faults in the observations used, or due to how they were interpolated spatially (i.e., the "objective analysis" scheme), or due to how other variables were deduced from them to complete the necessary data set (i.e., the "initialization" scheme). In contrast to such instantaneous error sources, deficiencies in the physical or mathematical formulation of the model impose continuous error sources.

The details of the model used for such an experiment are described in the papers by Smagorinsky *et al.* (1965), and Manabe, Smagorinsky and Strickler (1965). The particular version of the model employed in the present study is exactly the same as that used in Experiment 3 of Miyakoda, Smagorinsky, Strickler and Hembree (1969).

Briefly, the model is governed by the primitive equations and has 9 vertical levels. The domain is hemispheric and the computational grid is mapped stereographically. The model is "moist" with a condensation criterion of 80% relative humidity; the parameterized convection scheme is a moist adiabatic temperature adjustment. Long and short wave radiation are accounted for, with water vapor, carbon dioxide and ozone as absorbers of radiant energy which at most are functions of height and latitude and are constant with time. Large scale mountains and the thermal consequences of land-sea contrast are accounted for. The surface drag coefficient is constant irrespective of land and sea; the availability of water for evaporation is 0.5 over land and 1.0 over sea;

the temperature specification is different over land-ice and sea-ice; the snow-line is constant with time. The effective Karman constant for the internal non-linear lateral viscosity is 0.4.

The experiments were carried out with two different horizontal resolutions, i.e., $N = 20$ and $N = 40$. $N = 20$ denotes 20 gridpoints between the pole and equator, and corresponds to a grid size of 640 km at the pole and 320 km at the equator. $N = 40$ is double this resolution. The unperturbed initial data used is for 12Z, 4 January 1966. This is the same case reported in Miyakoda *et al.* (1969) as the additional example. Two integrations were made: one is a *control* run and the other is a *perturbed* run in which the perturbation was added to the initial temperature field, and I shall refer to their difference at any time as the error. These calculations were done for $N = 20$ and $N = 40$ resolution. The integration period was three weeks in each case.

The standard deviation of amplitude of the random temperature disturbance at initial time was 0.5C and was the same for all vertical levels. The perturbations were in practice produced by random numbers generated in the computer. Obviously there is far more energy introduced in this disturbance than in "the flap of a butterfly's wings," also its spectral qualities are somewhat different.

The standard deviation of temperature error at each level $\sqrt{a}$ is defined through

$$a = \left[\Sigma \frac{(T_a - T_b)^2}{m^2} \Big/ \Sigma \frac{1}{m^2}\right] - \left[\Sigma \frac{T_a - T_b}{m^2} \Big/ \Sigma \frac{1}{m^2}\right]^2 \tag{1}$$

where T_a and T_b are the temperatures in the control and the perturbed runs, respectively, $m = 2/1 + \sin\phi$ is the map scale factor for the stereographic projection and ϕ is the latitude. The summation Σ is taken for all horizontal gridpoints in the Northern Hemisphere at one vertical level.

The vertical average of the standard deviation is computed by

$$A = \sum_k a_k \Delta Q_k$$

where k is the index for the vertical level, and

$$\Delta Q_k \equiv Q_{k+\frac{1}{2}} - Q_{k-\frac{1}{2}}, \quad p = p_* Q$$

p and p_* are the pressures at an arbitrary coordinate level and the surface, respectively, and Σ_k is taken over all 9 levels.

The growth of temperature error is shown in Fig. 1a in terms of the vertical average of the standard deviation A. The ordinate is a logarithmic scale in units of centigrade. The curves marked "persistence" are the standard deviation for a hypothetical prediction in which the initial values were used as the forecast. In practice, persistence is computed from formula (1) by substituting the initial T_a for T_b. Persistence increases at first and then levels off. One may take the asymptotic level of the persistence curve to be a measure of the natural variability of temperature in the domain for this period.

When the standard deviation of temperature error reaches the level of the natural variability, we may assume that there is no skill left in the prediction beyond that time, and this defines our limit of predictability.

Actually the standard deviation decreases the first day from 0.5C to 0.2. This reflects geostrophic adjustment between the initially disturbed thermal wind field and the undisturbed actual wind field. Thereafter the error growth rate is exponential until about 7 days, with a doubling time of about 2-1/2 days, and then decreases as the error becomes large and therefore non-linear. Both high and low resolutions growths are characteristically the same.

The asymptotic levels of persistence for both $N = 40$ and $N = 20$ are about 5.5C. At 3 weeks that standard deviation of temperature error is little more than half of the natural variability. Fig. 1b shows the standard deviation temperature error at each level as a function of time. After about a week, that is when the temperature error becomes non-linear, the degeneracy distinctly is more rapid at lower levels.

It is of interest to examine the synoptic charts for the high resolution experiments ($N = 40$) at 1000 and 500 mb. Figs. 2 show the control and perturbed integrations at

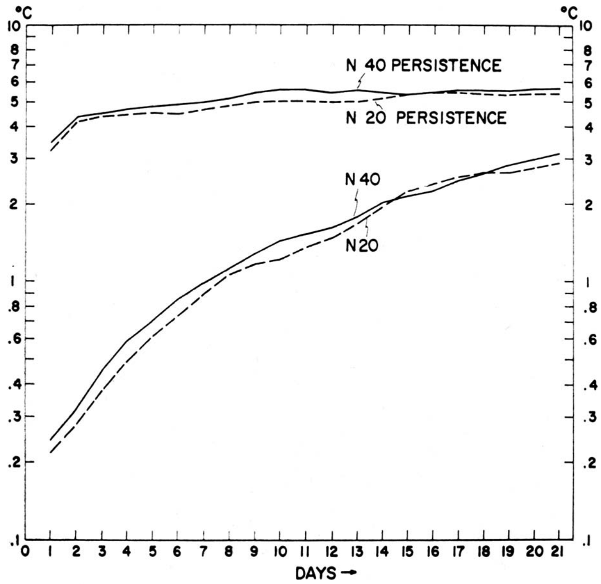


FIG. 1a. Vertically integrated standard deviation of temperature error (°C) as a function of forecast time interval for two different computational resolutions. The upper two curves are for a persistence forecast and the lower two for the difference between the control and the initially perturbed temperature field.

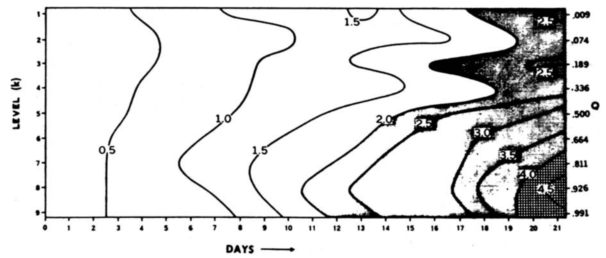


FIG. 1b. Vertical structure of the standard deviation of temperature error (°C) as a function of forecast time interval for N = 40.

14 days, and for reference the observed maps at that time. Figs. 3 are for 21 days, but without the observed maps. Note the systematic large scale departure between perturbed and control in the North Atlantic which is already evident at 14 days. In general even at 21 days, the perturbed map is still far from randomly related to the

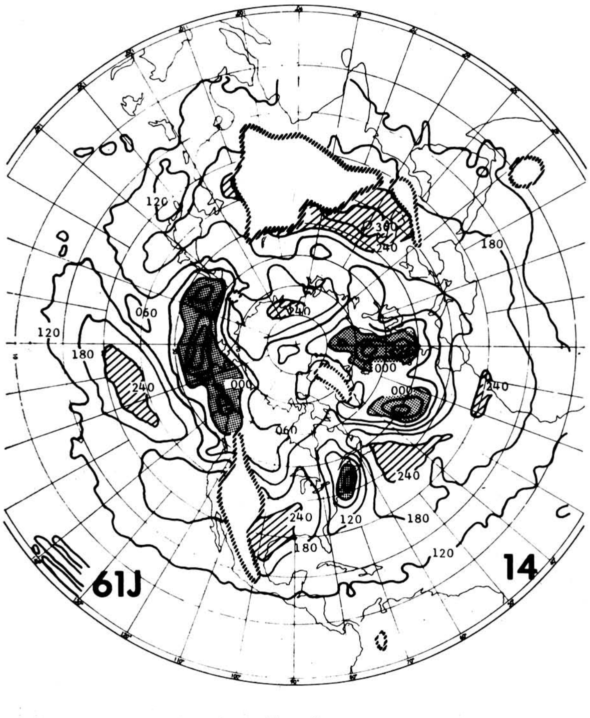


FIG. 2a

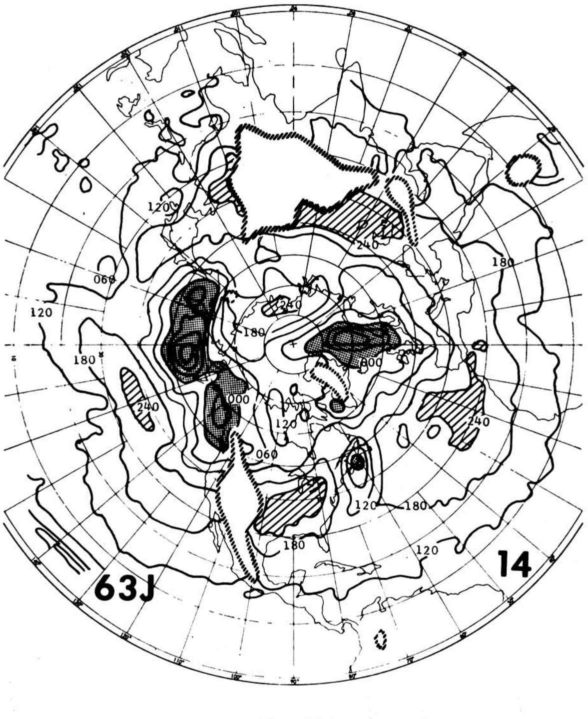


FIG. 2b

(See Fig. 2 legend on page 292)

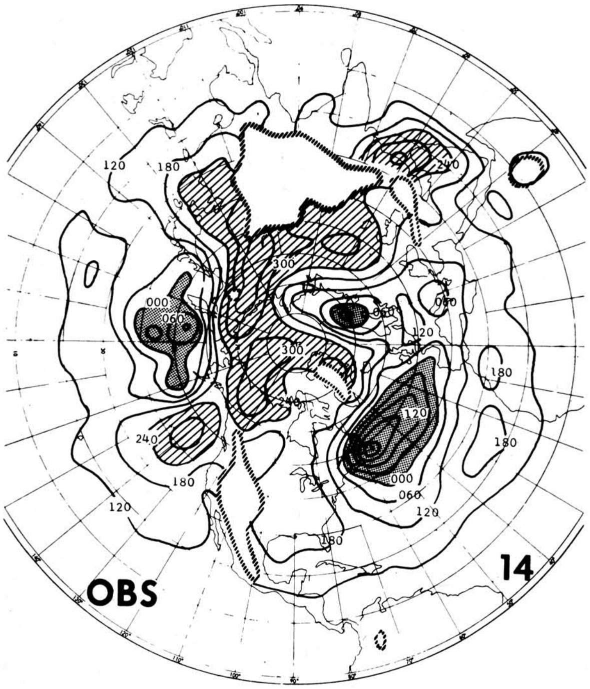


Fig. 2c

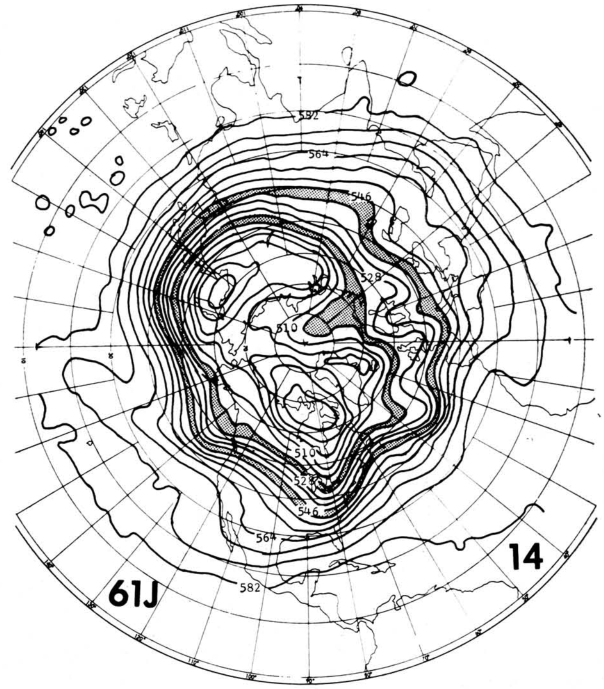


Fig. 2d

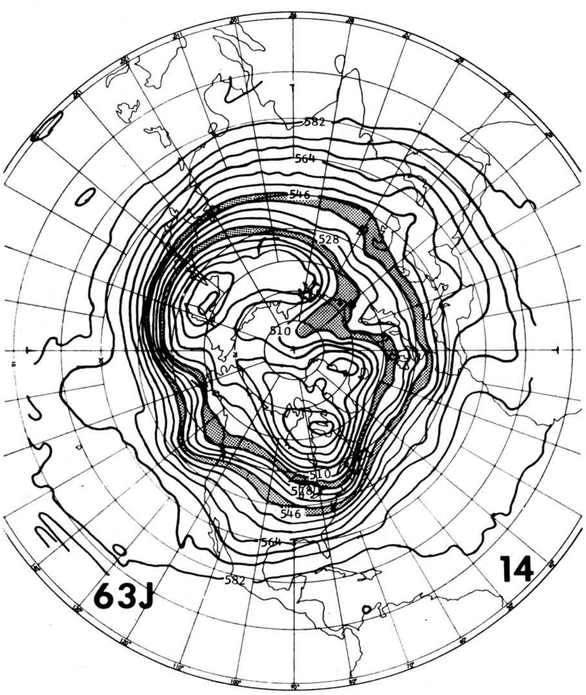


Fig. 2e

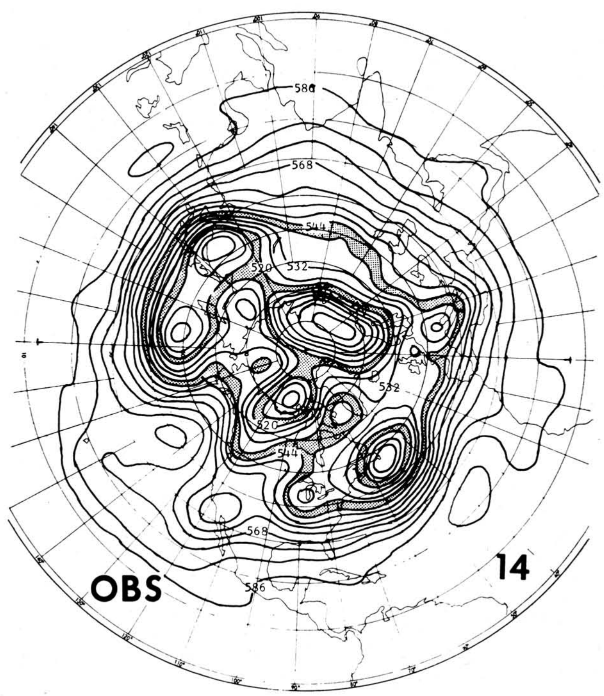


Fig. 2f

Fig. 2. Hemispheric synoptic charts of geopotential 14 days after the initial time. a, b and c are at 1000 mb and d, e, f are at 500 mb. The forecasts are with N = 40: 61J denotes the control experiment and 63J the perturbed. "OBS" denotes the observed map at the 14th day.

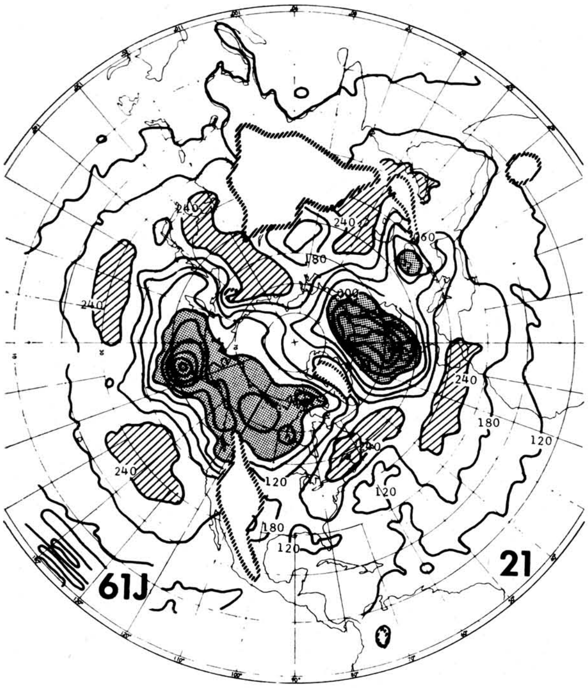


FIG. 3a

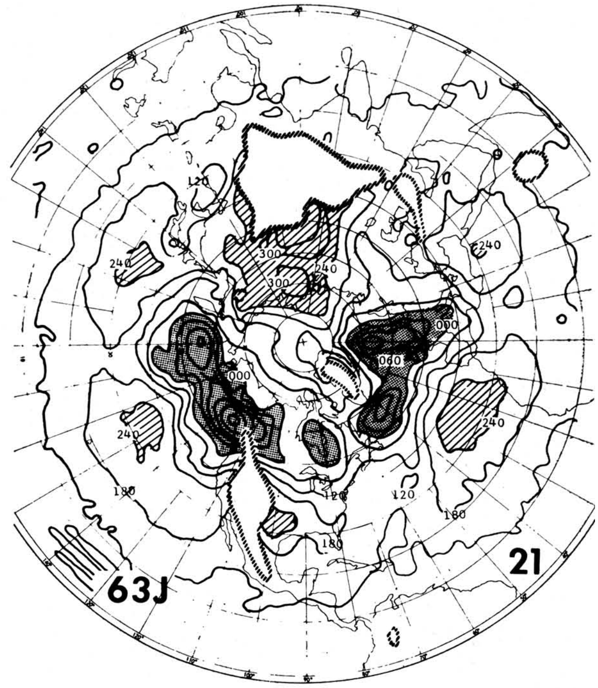


FIG. 3b

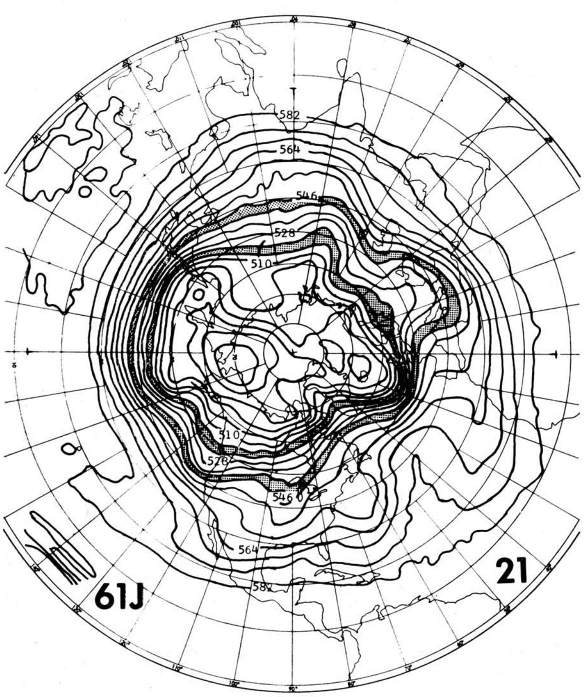


FIG. 3c

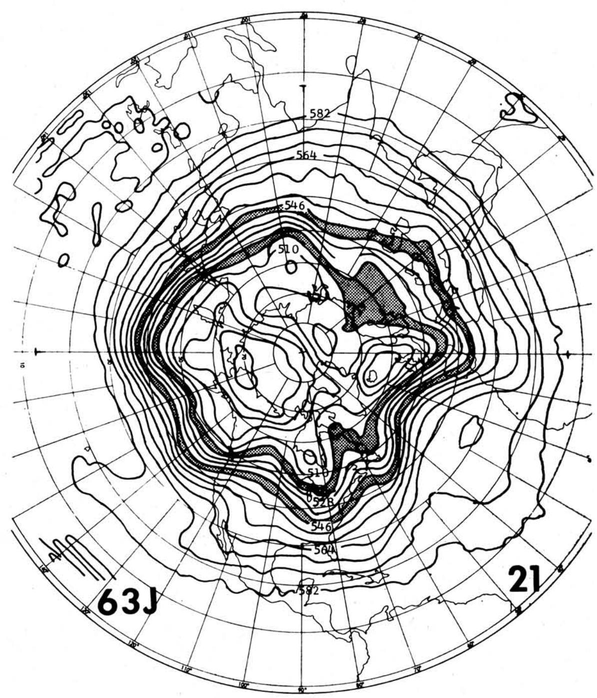


FIG. 3d

FIG. 3. Hemispheric synoptic charts of geopotential 21 days after the initial time. a and b are at 1000 mb and c and d are at 500 mb. The forecasts are with N = 40: 61J denotes the control experiment and 63J the perturbed.

control—another way of saying that the *deterministic limit* has not yet been reached. On the other hand a comparison of either with the observed map at 14 days indicates that the *practical limit* of predictability has already been reached at that time.

The standard deviations of the geopotential at 1000, 500 and 50 mb for $N = 20$ and $N = 40$ are shown in Fig. 4. Note that these are not on a logarithmic scale. In distinction to the vertically integrated temperature we note here a definite difference between the persistence levels of $N = 20$ and $N = 40$. In part this is a result of the ordinate scale used here and in part it is due to the vertical averaging of the temperature error. On the other hand the error curves for the two resolutions coincide, departing significantly at 1000 and 500 mb only after two weeks. But for each computational resolution the difference between persistence and error is about the same at 3 weeks.

In Fig. 5 we show the correlation coefficient of the time change of the geopotential height difference between perturbed and control high resolution experiments at 1000, 500 and 50 mb. These reflect, as did Fig. 1b, the fact that the loss of predictability is most rapid at the lower levels, but only after a week.

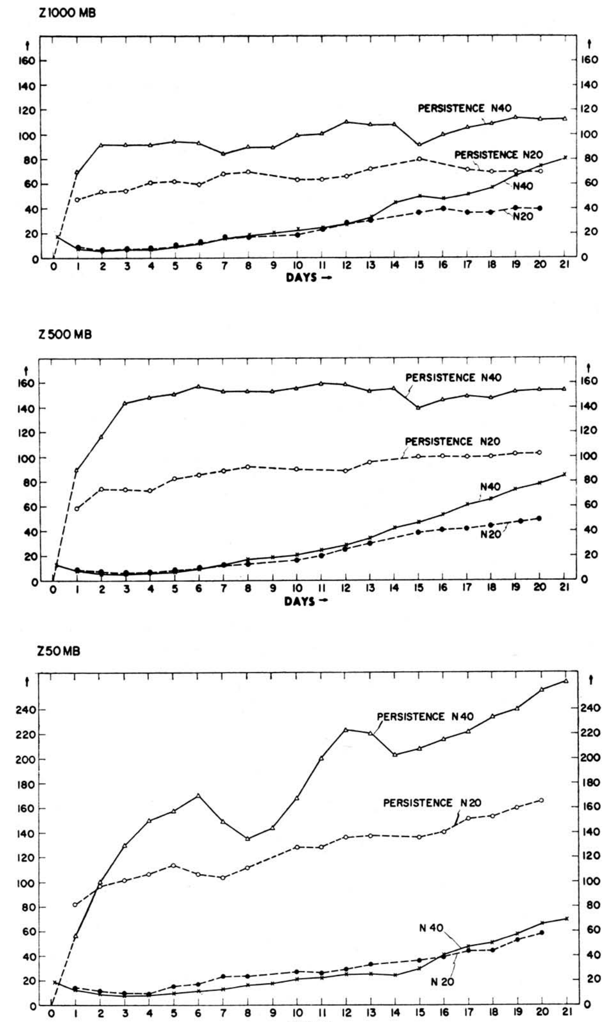


FIG. 4. Standard deviation of geopotential error (linear scale in meters) at 1000, 500 and 50 mb as a function of forecast time interval for two different computational resolutions. The upper two curves are for a persistence forecast and the lower two for the difference between the control and the initially perturbed experiment.

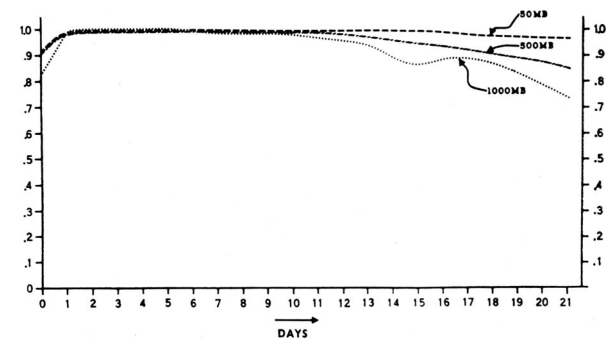


FIG. 5. Correlation coefficient of the time change of the geopotential height difference between perturbed and control high resolution experiments at 1000, 500 and 50 mb as a function of forecast time interval.

One can also quickly assess the relative decay of the time spectral modes. In addition to correlations of corresponding instantaneous states of the control and perturbed experiments, one can do the same for 1 and 2 day averages. This is shown for 1000 mb in Fig. 6 with an expanded ordinate scale. It is quite clear that as the higher frequencies are excluded the level of predictability increases, thereby reflecting Namias' basic tenet. This suggests that the position of storm tracks would have a larger span of determinism than that of individual extra-tropical cyclones. It must still remain as speculative whether there is hope to deterministically distinguish one year's January from another.

We now turn to an examination of the zonal spectral dependence of predictability. Fig. 7b shows a Fourier decomposition of the standard deviation of 500-mb geopotential error normalized by the Fourier decomposition of asymptotic persistence level for the latitude belt 35–45N both for $N = 20$ and $N = 40$. This normalization was determined for each wave number by a zonal spectral analysis of the 15–21 average of the persistence forecast, which is shown in Fig. 7a. Returning to Fig. 7b we note that in these normalized units, 1.0 is the threshold of randomness with respect to the natural variability. The high noise level results from this being only one case. Statistics from many cases would be required for a smooth picture of the spectral dependence. Nevertheless, for both computational resolutions, we find short wave predictability decays most rapidly. The distortion is markedly greater for $N = 20$ at wave numbers 14–18, which is near the limit of computational resolution. For $N = 40$, the predictability is a maximum at wave number 14 There is a secondary minimum at 6–12, the scales of maximum baroclinic instability. This is reflected in the $N = 20$ integration as well. Both also show a maximum at the very long waves—those that are geographically forced. This maximum has, however, been de-emphasized by normalizing with a geographically fixed persistence forecast. Note that the $N = 20$ calculation shows another minimum of predictability at wave number one, which is due to the large difference of the normalization

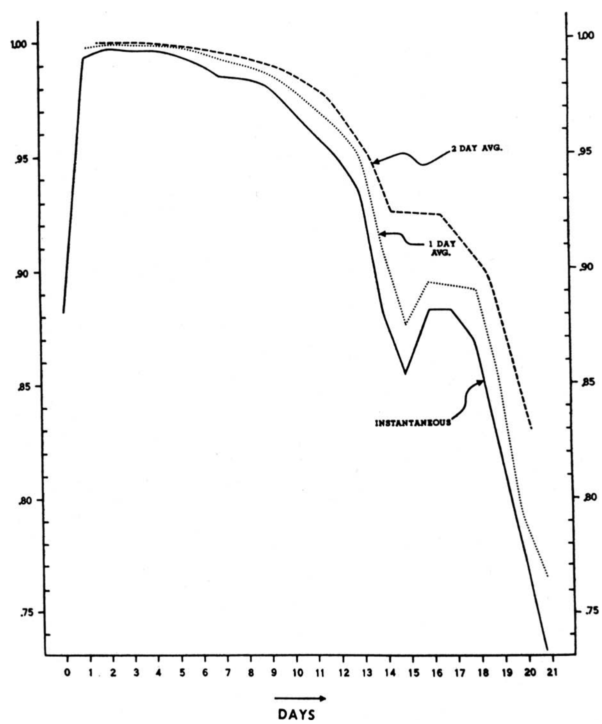


FIG. 6. Correlation coefficient of the time change of the 1000 mb geopotential height difference between perturbed and control high resolution experiments which have been time averaged. For comparison, the instantaneous correlation curve from Fig. 5 is also given.

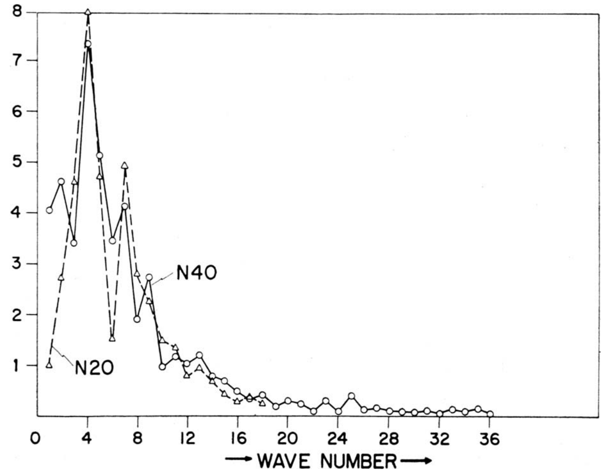


Fig. 7a. Zonal spectrum for the 35–45° latitude belt of the 15–21 day average persistence forecast error at 500 mb (in 10's of meters) for the two different resolutions.

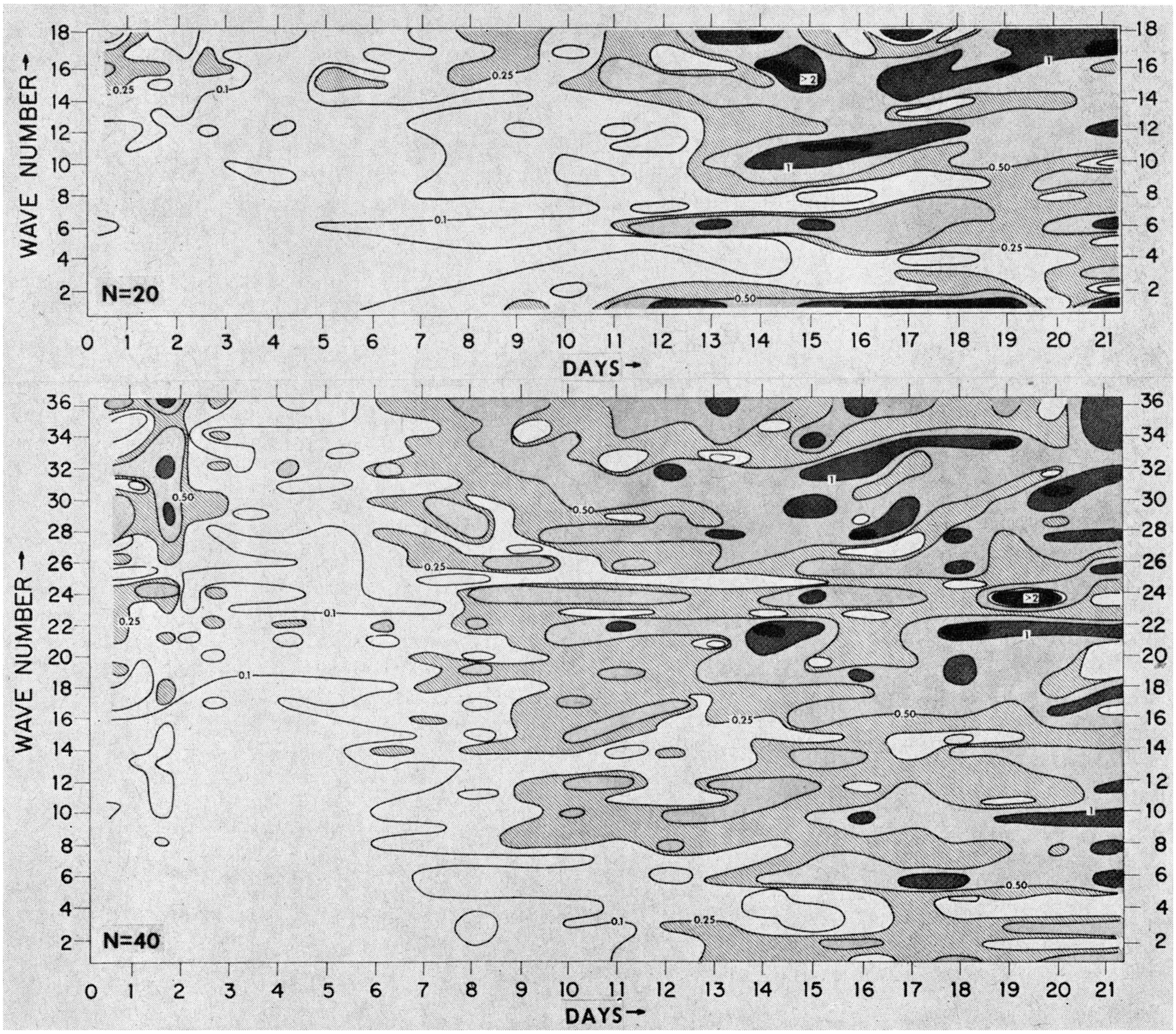


Fig. 7b. Zonal spectrum for the 35–45° latitude belt of the standard deviation of the 500-mb geopotential error as a function of forecast time interval. The spectrum has been normalized at each wave number by the corresponding asymptotic persistence level taken from Fig. 7a. Results are shown for both N = 20 and N = 40.

factor for the two resolutions, as is evident from Fig. 7a. It is not clear why the persistence forecasts for the two resolutions differ so much.

Of course this experiment also lacks complete generality. Obviously the limit of determinism from such experiments will depend in part on the nature of the disturbance: its four-dimensional spectrum, its amplitude and what variable or variables are disturbed, and on the nature of the undisturbed state.

With this reservation in mind we conclude from these experiments that the deterministic limit of synoptic scale predictability is at least 3 weeks.* In contradistinction, the

* Our discussion would be deficient without a comment on some very recent findings by Lorenz which he exposed at the AMS meetings this week entitled "Three approaches to atmospheric predictability." (See pp. 345 of this issue of the BULLETIN AMS.) He reports on the application of a "dynamic-empirical method" which uses derived equations for the errors, with observed spectral properties of the atmosphere appearing as coefficients. This is somewhat similar to Robinson's approach, but more sophisticated. Lorenz points out that this method is the only one which treats smaller scale features explicitly. His study covers a spectral span of 40 m to 40,000 km to demonstrate the scale dependence of the deterministic limit. The dynamical vehicle is a 2-dimensional incompressible model. Hence although he treats the non-linear spectral exchange in detail, the fundamental baroclinic instability responsible for the characteristic growth rates in the linear phase is lacking. Nevertheless, Lorenz concludes that synoptic scale disturbances have an error doubling rate of 2 or 3 days.

Lorenz then goes on to describe another approach which is virtually independent of the deficiencies of both the dynamic-empirical method and the dynamic method. It is an attempt to study statistically the departure of observed near-analogues. He points out, however, that the historical record is too short to be able to find sufficiently close analogues which differ only by a measure of small error. But, rather courageously, he does the best he can with the existing aerological record. This yields only a measure of large initial error, but he extrapolates this result to estimate the growth rate for small errors. Here again he arrives at a doubling time of less than 3 days.

The third element of Lorenz's paper is a reinterpretation of the Mintz-Arakawa results published in Charney's National Academy of Sciences report. He points out that the spectral properties of those results are incompatible with those determined from the atmosphere and therefore the Mintz-Arakawa doubling time of about 5 days should be wrong. Lorenz estimates that with more appropriate coefficients, small amplitude errors are indicated as doubling in about 2 1/2 days. The reason is quite clear. The Mintz-Arakawa grid was so coarse that it was not really able to resolve the most unstable baroclinic modes. We know from our own experience with models of resolution straddling theirs, that with lower resolution, approximately 1100 km ($N = 10$) in mid-latitudes, baroclinic instability is not possible at all. Twice this resolution ($N = 20$ or 550 km) is capable of resolving dry baroclinic instability, but is only marginally adequate in the presence of water-substance phase changes. The Mintz-Arakawa calculations were dry. One must conclude therefore that if the Mintz-Arakawa resolution were increased and if water vapor were permitted to be thermodynamically active (thus producing a smaller effective static stability), then the most unstable modes would be smaller and their growth rates would be much larger than those which yielded their 5 day doubling time. However, we have shown the results of predictability experiments with 2 different resolutions ($N = 20$: 550 kms and $N = 40$: 275 kms), both more or less adequate to resolve the most unstable modes. We thus did the test which Lorenz tried to estimate. As we saw, there is no substantial difference between the two for synoptic scales.

So let me now return to Lorenz's basic contention against the validity of the dynamic method. He argues that because the small scale interactions (i.e., less than 275 kms) are parameterized by means of a pseudo-viscosity, violence is being done in accounting for energy exchanges with the larger scales of interest. The implication is that the resulting distortions emasculate the model's ability to yield meaningful quantitative insight into synoptic scale predictability. I would be the first to admit that this parameterization is a weakness, mainly because we do not understand the real nature of the diffusion and dissipation mechanism. It is also germane, however, that these dynamical models maintain an energy density spectrum which varies approximately as the -3 power of the zonal wave number in the subsynoptic range. As best as data permit, this is a characteristic of the real atmosphere. Lorenz's and Robinson's estimates are based on the assumption of a Kolmogorov initial subrange, that is a $-5/3$ law. Moreover, it must be remembered that actual predictions in real forecast situations with precisely these dynamic models with the very same parameterizations, yield large scale predictions which are not yet randomly related to reality at one week (cf. Section 4b). This is despite the fact that there are known sources of error in these integrations. These errors arise not only from the parameterization of the small scale spectral interactions, but from all of the other parameterizations in the model. Furthermore, there are known truncation errors in the horizontal as well as the vertical, and of course there are serious errors in establishing the initial conditions from our present observational network.

This obviously must be the decisive argument. No heuristic appeal can deny the reality of these results. I submit, therefore, that the quantitative estimates of the dynamic predictability experiments must prevail and are definitive at this time.

current practical limit (see Section 4b) lies perhaps a little beyond one week. This is the gap to be closed if we can reduce mathematical, physical and initial data errors.

We have now raised the question having to do with data—and I would like to pursue it.

3. Observations and data

a) *Why, what and where*

The atmosphere is not much different from any other physical system which comes under scrutiny in science. One needs experimental data to define the characteristic structure and life history of the physical system to form the basis for scientific inquiry. One also needs such information to test theories for their ability to predict the transient behavior of these physical systems. In geophysics, laboratory controlled experiments to provide such data are difficult to come by. We must go directly to the geophysical medium itself. The observational network which now provides data for scientific investigation is one-and-the-same as that which provides the basis for routine weather forecasting. Our world observational system has grown rather sporadically and, to some appearances, randomly. The aerological system currently available is a direct outgrowth of the kite experiments which were performed in the 30's. It is an inescapable fact that a remarkable amount of information has been extracted from our present observational network. However, theoretical developments now appear to have outstripped the ability of this system to define the atmospheric structure and behavior in sufficient detail to support future progress.

For example, if we really had a better parameterization scheme for convection staring us in the face, we might not know it. I know of no set of basic data which can be considered definitive by any measure adequate to positively verify a "reasonable" convective hypothesis. This would also be true for an internal diffusive and dissipative parameterization. From a phenomenological viewpoint, the same can be said about the structure of the tropics and the disturbances which are spawned and reside there day after day.

It is a fact that today, at best, we have at our disposal only very indirect means for judging the validity of some of the theoretical components needed to construct comprehensive large-scale models of the atmosphere. These observations most often provide only necessary, though insufficient, verification constraints.

Yet, despite the crippling absence of definitive data, we must continue to construct hypotheses and to test them as best as possible. The danger is that we may have a worthwhile idea, and reject it because of an inadequate means to judge it by.

What kind of data do we need in order to promote a fruitful conduct of research and to provide the information essential for prediction? On the face of it, as far as the large scale atmosphere is concerned, we need to know the horizontal wind vector, the temperature and the humidity, everywhere, and on all scales.

This overwhelming requirement, it was learned many years ago, can be relaxed. Say one is able to make volumetric measures of these variables, of the order of 100 kilometers on the side and a few kilometers in the vertical, and averaged over a few hours. If so, then much of the detail can be dispensed with, since the higher-frequency, smaller-scale components of the atmosphere lose their identity rather quickly—that is they have brief predictability. It is only their characteristic spectral properties that probably need to be known.

It was not too long ago that the aerological data density requirement for numerical forecasting was taken to be the currently fashionable grid size. This may have placed too stringent a requirement on the observational network. It now appears that the numerical integration mesh size should be finer than that required to define the important physical scales in the initial conditions. This is primarily due to the fact that we are dealing with a characteristically non-linear system, and extra computational resolution is required to minimize the interaction of truncation error with the scales of physical interest.

When I stated that the two horizontal wind components, the temperature and the humidity are the basic large-scale variables, this already made use of certain interdependencies of the atmospheric variables, which actually number 7 rather than 4. The ideal gas law and the hydrostatic law have already been used to reduce the number of variables by 2 at the observational point. The energy of large-scale vertical motion is taken to be small compared to that of the horizontal wind, and is neglected.

Furthermore if it were not for the fact that extra-tropical motions are highly geostrophic, so that the horizontal wind is well approximated by the horizontal pressure gradient, our observational system would be far more inadequate than it is. Hence, the primary 7 variables which define the atmosphere are not really completely independent. To continue, we know that even the large scale vertical motion component of the atmosphere can be deduced diagnostically from a knowledge of the mass distribution. We need only solve a 3-space-dimensional partial differential equation.

It turns out, however, that this apparent redundancy between the variables in the atmosphere does not stop here. There also exists somewhat weaker coupling than those I have just described. They are dynamical in nature. That is, their interdependence requires viewing them in time as well as in space. This is really true for the geostrophic coupling, but the adjustment is so rapid and so small that we more-or-less assume it to be instantaneous. It is this dynamical coupling which may ultimately save the day for us, if we can cleverly employ theoretical models to bring out the relationships.

Let me give you a few examples. The observations of surface pressure is perhaps one of the first field variables ever to have been made in meteorology. It provided some of the earliest forecasting tools last century and, in fact, persisted as the major information source for forecasters right up until the aerological era. It is a variable we have always taken for granted as useful, if not necessary. The fact that there is always a man at the bottom on a rawinsonde ascent means that, in addition to determining temperature, wind, humidity, in a near vertical column, he obtains, with little more effort, a record of the surface pressure. But say we conceive of a direct sounding system where the instruments are carried horizontally at a number of levels. A surface pressure reading over the oceans does not come automatically or even with trivial effort in this case. Something very special has to be done. The question is how much information lies in the surface pressure if you know everything else? How worthwhile is it to make a special effort to acquire it? This type of question is fairly easily answered through a numerical experiment (Smagorinsky and Miyakoda, 1968). Indeed we did one in which the zonal surface pressure structure was completely eliminated from the initial conditions, except for that structure due to the known mean effect of mountains. This simulates a situation far more constraining than any that might occur in practice. Nevertheless, one finds that the two-dimensional surface pressure structure is fairly rapidly restored through dynamical coupling and a knowledge of all the other variables. The relaxation time is of the order of a day or two.

A similar question arises with regard to the humidity distribution. In the earlier attempts to make precipitation forecasts by means of dynamical models in the 1950's, the smaller scale structure of humidity fields, in contrast to that of the wind and temperature fields, particularly concerned us. A determination of highly filamented water vapor fields seemed almost hopelessly inaccessible, except where the existing aerological network was dense, such as in eastern United States. In part, this worry was partially alleviated by the prospect of continuous horizontal field monitoring of cloud distribution by satellites. It was already known from earlier studies that the water vapor field was highly correlated with the liquid water field, which in part could be determined from satellite cloud pictures. Yet, it was of interest to know how critical the water vapor field determination was. To test this we did something similar to the experiment involving surface pressure. The zonal structure at each level and at each latitude was replaced by its zonal mean. Here again the field reconstituted itself within a matter of a day or two (Figs. 8 and 9). The reason in retrospect is fairly obvious. We know that the large scale motion field is largely determinable from the mass field. The closing factor is that the history of the vertical motions is mainly responsible for the water vapor structure. This latter property is reflected in the fact that observational studies in general circulation reveal very high correlations between the vertical motions and the humidity.

Note in Fig. 9 that the "limit of predictability" of the water vapor itself for such a large disturbance in the initial water vapor field is 8–9 days. Other properties of the forecast are much less disturbed because of the weak reaction.

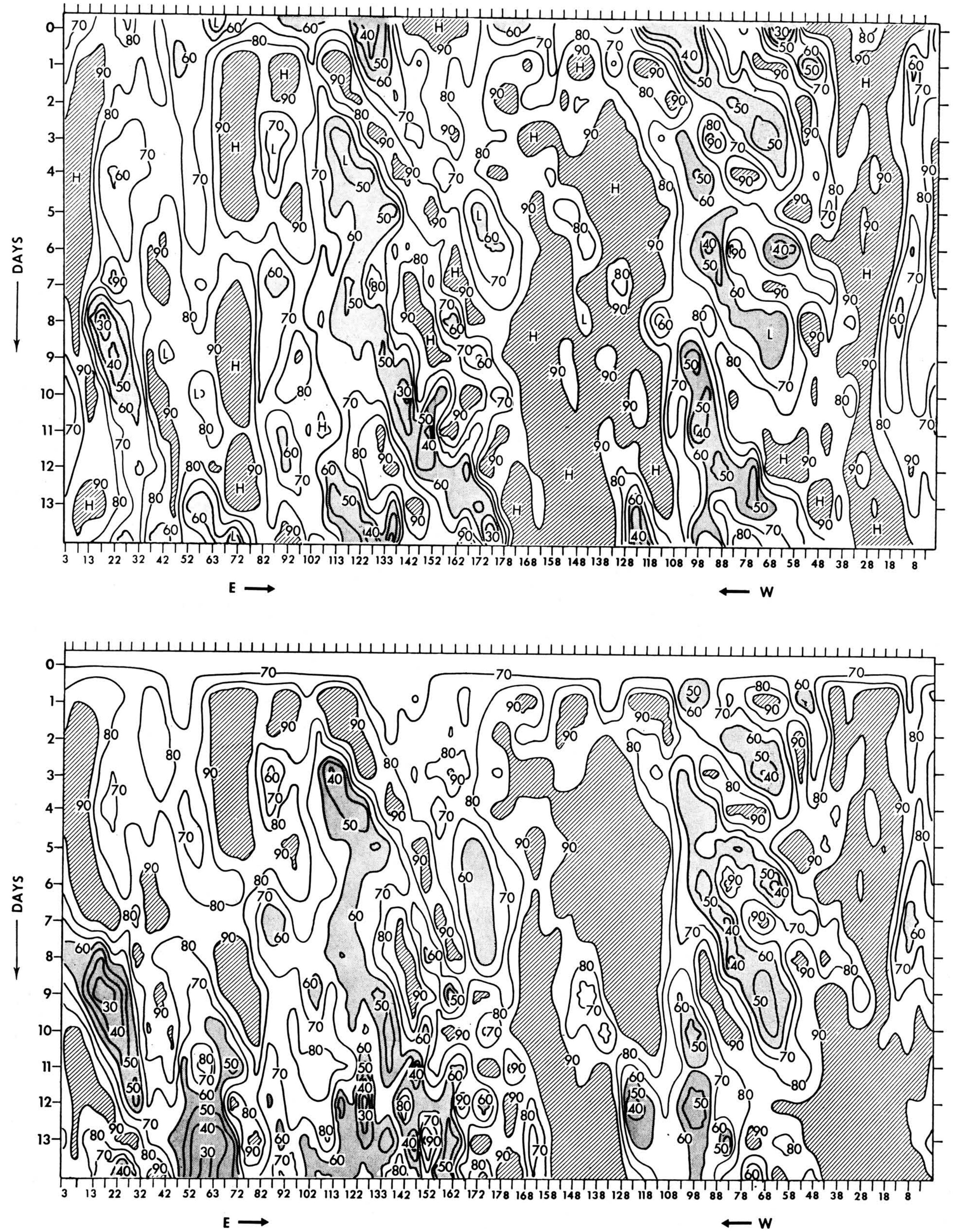


FIG. 8. Relative humidity averaged over the 35–45N latitude belt at $Q = .926$ (approximately 926 mb) as a function of longitude and forecast time interval. The upper figure is from the control experiment and the lower figure from the experiment in which the initial zonal structure was replaced by the zonal mean at every level.

Similar experiments have been performed by removing information of the boundary layer structure of the wind and of the temperature, and similar reconstitutions were experienced.

It is obvious that if one were to carry this reasoning to its natural conclusion then one might decide that no data are required at all. This reminds me of a question-answer exchange I read in a recent newspaper. The question was "Besides the appendix, what other parts of the body can man live without?" And the answer—"Man can afford to lose his thymus, thyroid, three of his parathyroids, gonads and other internal and external reproductive organs, spleen, esophagus, ureters and urinary bladder, most of his liver, his bowel, one lung, one kidney, and part of his brain. *Not all at the same time, of course.*"

The important conclusion to be drawn is that there is a far greater redundancy amongst the large scale variables in the atmosphere if one can view them as variables in a four dimensional field. Comprehensive theoretical models are needed as the vehicle. It is not any longer necessary to think in terms of simultaneously observing the atmosphere everywhere and then to interpolate the point measures 2 or 3 space-dimensionally to establish initial conditions. One can think much more broadly. The general problem now is to assimilate a single observation into a four dimensional array. It may very well be preferable that observations are not simultaneous. From this perspective, the information content in a given observational system is potentially far greater than we now think.

It may very well be that this new-found source of redundancy will not be icing on the cake. The additional information thus made accessible will be needed to compensate for our technological inability to adequately monitor the state of all variables in the atmosphere everywhere.

There is another important conclusion to be extracted from these, as yet, very primitive studies. You may have drawn it already yourself. First of all, not all data are equal in their information-yielding capacity. Some are more equal than others. This tells us that if there is a choice as to what can be measured, then one variable may be preferable over another. I'm sure this will turn out to be the case in the tropics.

I would now like to go on to a related question regarding data requirements. Numerical studies are also beginning to give us some information on the propagation of influence between different parts of the atmosphere. Amongst other things, this tells us *where* in the three-dimensional atmosphere a knowledge of its detailed structure is most critical. For example, the region of major kinetic energy production of the atmosphere is to be found in mid-troposphere at about 600 mb. These baroclinic energy transformation processes depend on very small phase differences between the temperature

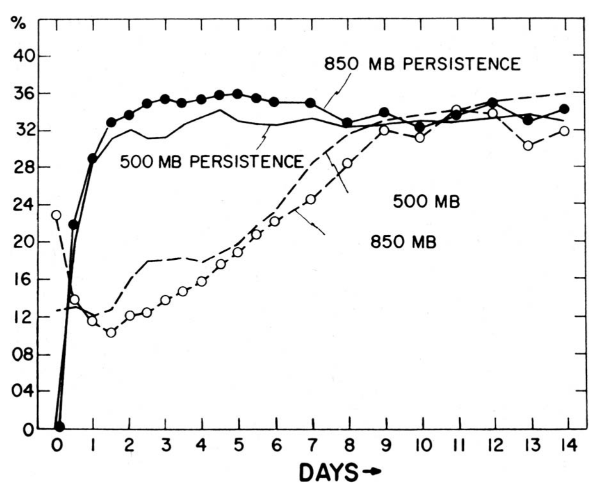


FIG. 9. Standard deviation of the humidity "error" at 850 mb and 500 mb (dashed curves). The corresponding persistence forecast errors (solid lines) are shown as a reference for the natural variability.

and streamline patterns, and therefore are very sensitive to slight errors and inconsistencies in establishing the initial conditions. The energy produced in mid-troposphere is dispersed upward and downward by the pressure interactions as measured by the correlation of the geopotential and vertical motion fields. The major dissipation occurs primarily in the surface boundary layer and there is a secondary maximum at jet stream level where one also finds the maximum perturbation kinetic energy. This gives some clue as to where the greatest sensitivities lie to aberrations in defining the initial conditions. To a large extent this is also reflected by the fact that the maximum growth rate of initial errors in predictability experiment resides in the lower troposphere, as we saw earlier. It is clear to me that an impovement in the obsevational definition of the large scale global tropospheric structure over the large oceanic expanses is probably the most crucial extra-tropical data problem facing us today.

b) A challenge for technological invention

Wexler and Bugaev's grand vision of a satellite viewing the atmosphere as a whole was indeed an exciting one. It set into motion a series of technological innovations unparalleled in meteorological history. Some were directly connected with the use of the earth-orbiting satellite as a platform to remotely view and sense structural properties of the atmosphere, such as the cloud distribution and to a lesser extent the temperature and wind structure. But also the knowledge of the availability of a satellite platform tangentially resulted in the development of horizontal sounding balloons for directly probing the atmosphere. Then, of course, there are the developments not directly attributable nor connected with the satellite development, such as open-ocean buoys.

Only a few years ago when the wave of optimism for completely monitoring the atmosphere was at its peak, it was thought that so many redundant alternatives would be available that our efforts would mainly be concerned with making the best choice. However, the harsh realities of inventing a complete observation system for monitoring the global atmosphere are now more apparent than ever. The number of alternatives that can be made to work is shrinking. We no longer have a candy store full of goodies from which we have but to select the choicest bits. We probably will be faced with the need to form a composite system of every possible workable alternative that is realizable. The challenge for technological innovation is ever present. The problem is far from solved, but the enthusiasm and optimism of the technologists is encouraging. If their performance of the past is any measure of what we can expect in the future, we can all feel confident of their ultimate success in coping with a most formidable problem, that of measuring the atmosphere in all of its important facets on a global scale.

A second key to our expectations for deterministic extended range prediction stems from the systematic growth of model sophistication.

4. Models vs. the atmosphere

a) Are theoretical simulations relevant?

This is not the time to parade before you in detail the succession of modelling advances that have been achieved in the past 15 years. Nevertheless they are germane to the dramatic extensions of the prediction time-span.

In many instances the advances have come from modelling efforts to understand and simulate the large-scale general circulation of the atmosphere. An ability to cope with the primitive equations as the hydrodynamical vehicle of such models is, in part, one such example. Then there are the global mapping schemes and more sophisticated finite differencing techniques. From a mechanistic viewpoint, various parameterization schemes arise from the same motivations: frameworks for the boundary layer, for the moist adiabatic process, for convective adjustment, for small scale diffusion and for ocean-atmosphere interaction, to name a few of the more prominent parameterizations.

As a result it has been possible to understand and to account for considerable detail of the observed general circulation. For example there is the interaction of the troposphere with the stratosphere, and the maintenance of the stratospheric flow, its thermal characteristics and its water vapor structure. To a lesser extent, the dynamical nature of the tropics is somewhat better understood, although there is still considerable doubt whether we even know what the characteristic disturbances are like.

Recently it was reaffirmed by numerical experiment what had been strongly suggested from observational data all along: that one cannot ignore the influence of the sea surface on the atmosphere beyond 3 or 4 days (Miyakoda *et al.*, 1969). This is especially true along western oceans where the warm currents, such as the Gulf Stream, flow. Here significant effects may occur within a day or two. The principle interactive processes are the turbulent transfer of heat in sensible and latent form.

This means that for such time spans, at a minimum, we must know the climatological distribution of sea-surface temperature. However, it is strongly suspected that within a week the upper layers of the ocean (say down to a depth of 1 to 10 m) respond sufficiently to the atmosphere to give rise to significant anomalies in the sea surface temperatures from the climatological norms.

Observational evidence of the large-scale interactions of the atmosphere and the oceans has been called to our attention by both Namias and Bjerknes. In particular, Bjerknes' (1966) speculations stimulated an experiment which was performed by Roundtree (1968) of the British Meteorological Office who was visiting our Laboratory.

He assumed a large scale sea surface temperature anomaly in the eastern equatorial Pacific with a maximum of 3.5C. He compared the evolution in this prediction experiment with a controlled experiment without the anomaly. He then studied the spread of influence as measured by at least a 2C temperature difference at 500 mb from the controlled experiment.

Within 8 days the influence had spread around the tropics and to 45 degrees latitude at the longitude of the anomaly. By 12 days the 2C threshold was reached in Europe and by 16 days in Siberia. This indeed is dramatic support for Bjerknes' hypothesis. This means that for time spans for the order of a week, an ability to predict atmosphere evolutions depends on *observations* of the anomalous sea surface temperature even if one assumes that these anomalies persist over the week long forecast interval.

For further extended prediction time spans of the atmosphere, the variations in the anomalies appear to be rapid enough to be of significance. Thus for spans beyond 2 weeks it would no longer be adequate to assume that the initially observed sea surface temperature anomalies persist. They must be predicted. It is not for long that the atmosphere and oceans can be regarded as independent physical systems. One must construct joint atmosphere-ocean models which extend deep enough into the oceans that the interaction can be accounted for by the applicable physical laws governing the exchange of heat, momentum and water vapor. Obviously, if one is thinking in terms of seasonal forecasts of very broad scale properties of the atmosphere, then the lower boundary of the ocean-atmosphere model must extend at least down to the seasonal thermocline.

This last requirement is one that we have been very consciously aware of and serious efforts to model the oceans at GFDL have been under way for the past 5 or 6 years under the direction of Bryan. Within the last two years the logical step of joining atmosphere and ocean into a single model was undertaken by Manabe (1969) and Bryan (1969), laying the groundwork for ultimate applicability to the long range forecasting problem. This model gives a considerably detailed accounting of the elements in the hydrologic cycle—not only atmospheric water vapor, precipitation, and evaporation but also variable soil moisture, runoff, snow cover and sea-ice. Manabe and Bryan mainly addressed themselves to the very long term deep atmosphere-ocean interaction suitable for a discussion of their equilibrium structure, that is the joint atmosphere-ocean general circulation. It is for this reason that the ocean models have extended down to the sea bottom. But I expect that joint models, more directly applicable to the seasonal or shorter term forecast problem, will be forthcoming in the natural course of events in the near future.

The systematic advances in general circulation simulation bear not only on the subject of this evening's talk, long range forecasting, but on other possible related applications. For example, there is the already familiar suggestion that when these models become capable of reproducing the natural behavior of the atmosphere, it may be possible to simulate the response of the atmosphere to human tampering. Such a possibility certainly is clearer now than it was a few years ago, when it was first proposed. However I feel we have a considerable distance to go in model development before they are reliable enough to discern the subtle differences between a natural atmospheric evolution and one which can be tampered with by means accessible to man. Nevertheless the potential does appear to be latent, and there is good reason to be optimistic.

Related to the possibility of purposeful attempts to modify climate is assessing the inadvertent influence of human activity on the atmosphere, direct as well as indirect. Hunt and Manabe (1968) recently applied one of the GFDL's earlier models to determine the long term dispersion characteristics of inert tracing material which was assumed to be released in the lower tropical stratosphere. In actuality this corresponds to the release of radio-active tungsten in nuclear experiments. Despite the possible moral issues involved, the monitoring data of the dispersion of this material provided an unusual opportunity to verify the results of the Hunt-Manabe experiment. Remarkably they were able to reproduce the essential characteristics of the observed dispersion. Perhaps most important is that for the first time we know why this happens as well as how. In particular, the very slow dispersion is the result of an almost balanced opposition of the large scale eddy transports and the mean meridional circulation. This result suggests the utility of further experiments to determine the large-scale, long-term dispersion of pollutant material of other types, from other geographical locations, and with other time characteristics, both in the atmosphere and in the oceans.

One can also conceive of experiments that would provide information on more indirect consequences. A number of years ago the National Academy of Sciences (1966 b) Committee on Weather and Climate Modification inquired whether industrialization of the past 100 years and the resulting apparent increase in atmospheric carbon dioxide might not have systematically altered some climatic characteristics. They also asked whether or not a proponderance of SST's exuding vast amounts of water vapor in the stratosphere might not alter the thermal equilibrium of the atmosphere. At the time only tentative answers were possible for these questions. However, it appears that before long the application of viable mathematical models of the atmosphere and oceans will be capable of providing answers to such questions as well.

In a broad sense, the examples I have cited are just as much in the nature of a request for a prediction as the conventional kind.

b) Extended prediction of specific large-scale events

I earlier spoke of the ultimate deterministic limit of the atmosphere. It is an abstract notion but yet, as I indicated, of some practical importance. In contrast, it would be useful for our perspective to ask for a measure of the present practical level of predictability allowed within current limitations. I alluded to this earlier, but would now like to show some illustrative results of an extended prediction experiment (Miyakoda *et al.*, 1969). It is for a Northern Hemisphere domain, bounded by an equatorial wall. The initial conditions are for 9 January 1964 at 12Z. Results and conclusions which I will draw are essentially the same for another such case (4 January 1966, 12Z). The essence of the results is succinctly given in a trough-ridge diagram verification of the 500 and 1000 mb geopotential in the 35–45° latitude belt. This is shown in Figs. 10 and 11. In particular, we found in this experiment that the 1000-mb flow already began to verify rather poorly by 7 days and was virtually randomly related to reality at 10. At 500 mb, the situation is somewhat better. The forecast could be classed as poor at 9 or 10 days, and is more or less random by 13 days. It is significant that an episode of blocking in the eastern Atlantic throughout the 2 week period was predicted.

Historically, precipitation is well known by practicing forecasters to be much more difficult to predict quantitatively than flow or temperature regimes. Up until a few years ago deterministic one day forecasts of precipitation represented a practical limit of skill. In the extended experimental prediction just discussed, the forecast two-day accumulations of precipitation in the United States still carried considerable skill at the fifth and sixth days, but the degeneracy became quite evident from the 7th day on. In another such case we found that 24-hour accumulations were reasonably good out to the 8th day.

The one day numerical forecast problem was to account for the translation of large-scale wave disturbances in mid-tropospheric mid-latitudes. This was accessible through barotropic models. The two or three day forecast problem was, in addition, to determine whether such disturbances would amplify or decay. This is largely accessible through simple thermally inactive baroclinic models. Beyond 3 or 4 days we are faced with the substantially new problem of predicting the formation of new disturbances not in existence at the initial time. By the application of general circulation models we were

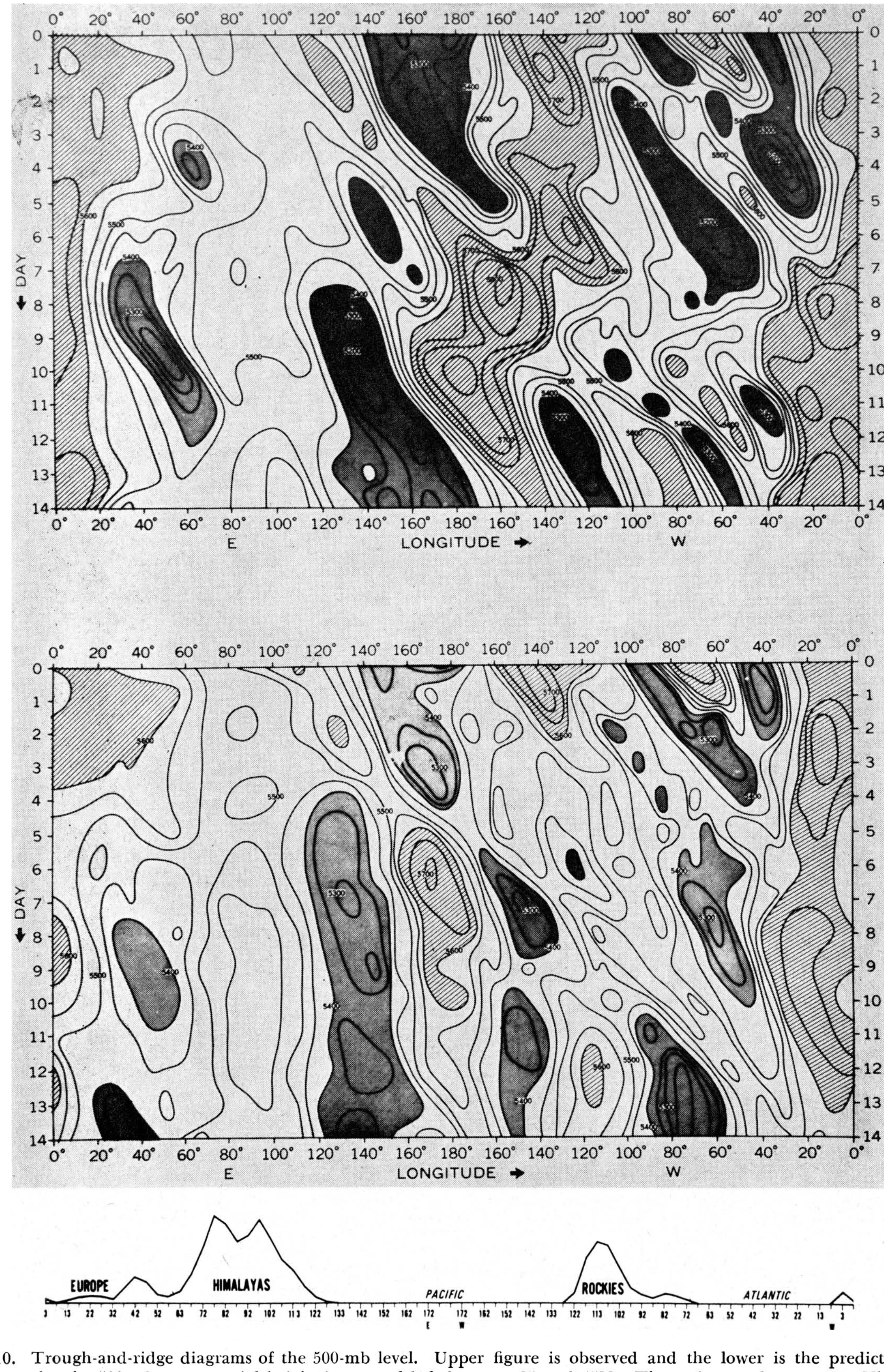


FIG. 10. Trough-and-ridge diagrams of the 500-mb level. Upper figure is observed and the lower is the prediction. The contours are for the 500-mb geopotential height in a zonal belt between 35 and 45N. The units are decameters. The interval is 50 m. The ordinate is time in days, and the abscissa is longitude. The ridge areas with geopotential greater than 5600 m are hatched and the trough areas with values lower than 5400 mare stippled.

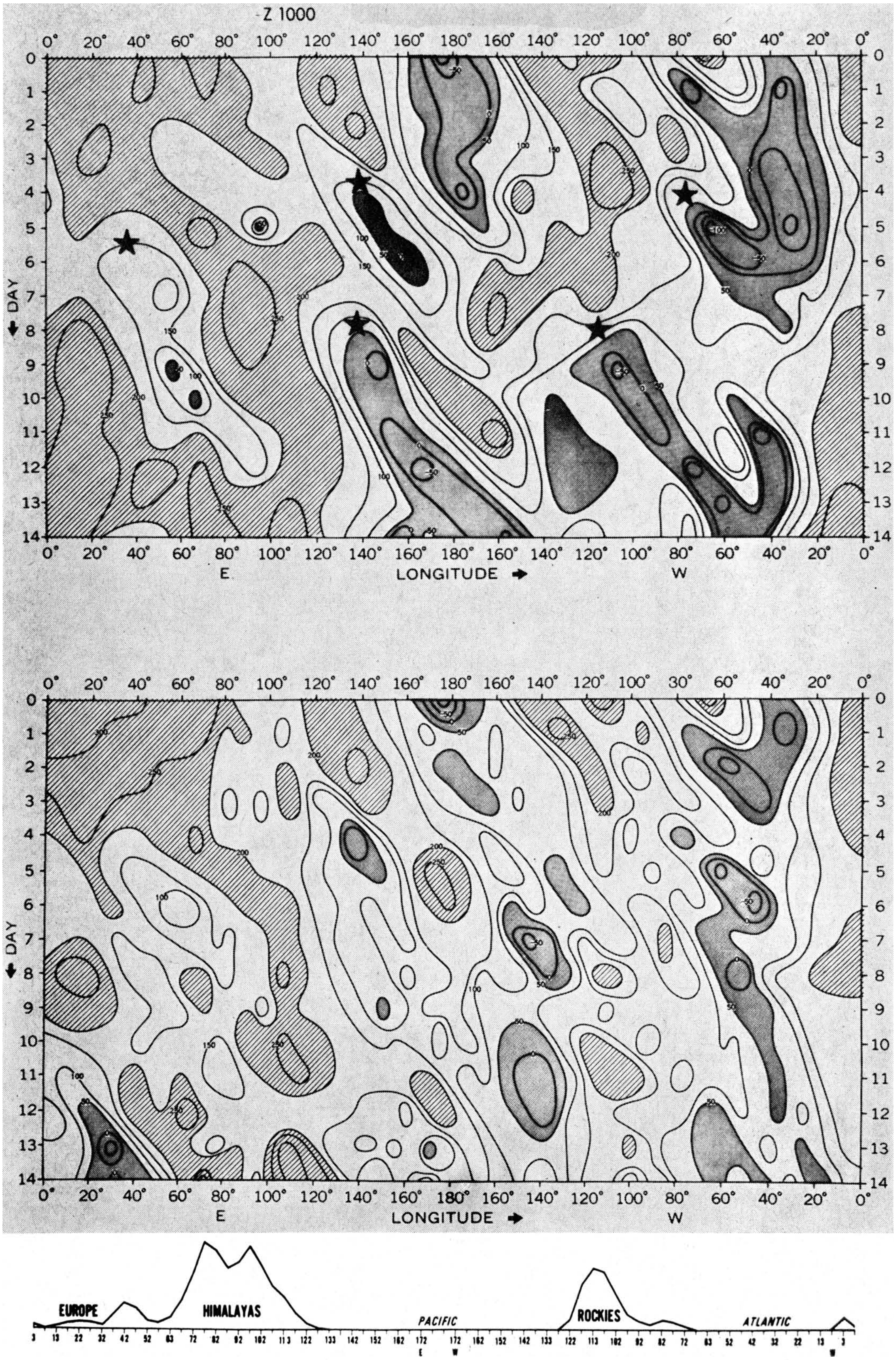


FIG. 11. Trough-and-ridge diagrams of the 1000-mb level. Upper figure is observed and the lower is the prediction. The contours are for the 1000-mb geopotential height in a zonal belt between 35 and 45N. The units are meters. The contour interval is 50 m. The ordinate is time in days, and the abscissa is longitude. The anticyclone areas with geopotential values higher than 200 m are hatched and the cyclone areas with values lower than 100 m are stippled. The stars denote the occurrence of new observed cyclonic disturbances.

able to account for second and sometimes third generation cyclogenisis off the east coasts of the United States and Asia (see Fig. 11). We found, characteristically, that there was some success in the ability to time these, being off by a day or two in individual instances.

Within the past year a global prediction experiment (Miyakoda and Staff Members, 1968) was also attempted. It took about three years to gather the data for a period in March 1965 (in many cases from obscure and normally inaccessible sources), to check it, and to construct a consistent and full data set, extending from pole to pole (including the tropics of course), and extending from the surface to 100,000 ft. As far as I know this is the first such experiment ever attempted. It required a determination of the existing synoptic scale disturbances in the tropics at the initial time and to predict their behavior in concert with the rest of the atmosphere over a 14 day period.

The object of this rather massive undertaking was multifold. First, it was an attempt to see how well one could define the initial conditions from all presently existing sources, including satellite data and normally non-transmitted aircraft and ship reports. Secondly, the object was to perform some controlled experiments to study the influence of the atmosphere of one hemisphere on the other. We must clearly understand at which point in the forecast time span one can no longer ignore inter-hemispheric interaction. Thirdly, the tropics were of particular interest both intrinsically for their short term behavior and predictability and also in their role as the buffer through which the hemispheric atmospheres interact. Earlier today you heard Dr. Miyakoda describe the tropical aspects of these results (Miyakoda and Staff Members, 1969).

I cannot take the time today to discuss in depth this, our first, global forecast experiment. At this point I would say it was exceedingly interesting but, from my viewpoint, not as conclusive as one would prefer. Further experiments will be required in order to draw more definitive conclusions. A whole series will probably have to be done for this same case, altering properties of the initial conditions as well as some of the parametric formulations of the interacting processes. Within the next year or two additional data sets of this sort will be needed to permit more exhaustive and more complete experimental trials. Suffice it to say, there are already indications of what one can expect when the constraint of an equatorial boundary is removed. The mid-latitude forecast in the Northern Hemisphere at one week was better in many respects in the global experiment.

Upon examining the Southern Hemisphere part of the forecast of these experiments, it is evident (at least in this case) that the deterioration is markedly more rapid than in the Northern Hemisphere. One can think of two reasons, one more obvious than the other. In the Southern Hemisphere we have one tenth the aerological network available in the Northern Hemisphere, and we know that the latter is inadequate. Furthermore, one might expect that because there are a fewer continental areas in the Southern Hemisphere, the forced component, due to ocean-continent contrast and mountains, would be somewhat weaker than in the Northern Hemisphere. From this one might conclude that the enlargement of predictability in the Northern Hemisphere induced by these forced modes would be less pronounced in the Southern Hemisphere.

Nevertheless, even in light of these handicaps in the Southern Hemisphere a surprising degree of skill was still apparent at three days. This is still far short of what we have come to expect in the Northern Hemisphere. It goes without saying, the handicaps of inadequate observations to establish initial conditions also plagues us in trying to *verify* the results of experiments.

There is thus growing confidence that one can expect to predict many of the major large scale events commonly occurring in our atmosphere. But there are also relatively rare large scale events which are of importance, and the question is to what extent they may be predictable a few days or even a few weeks in advance. Some experience exists already.

For example, the stratospheric polar vortex tends to break down into a dumbell-configured flow rather suddenly in the early spring each year. Miyakoda, who has a special interest in this phenomenon, has chosen a case which puts its predictability to test. Indeed it has been possible for him and his associates (Miyakota, Strickler and Hembree 1969) to account for this rapid transition as much as five days in advance.

A related phenomenon, sudden stratosphere warming, has not yet been predicted in forecast experiments. However, Manabe (unpublished) has observed one such episode to occur in a general circulation experiment in which the seasonal variation of solar radiation was imposed. The breakdown was accompanied by a 44C rise in 16 days at 9 mb at a point at 65° latitude. The episode was not quite as rapid as one observes in the real atmosphere, but there appears to be very little question that the qualitative characteristics are generally reproduced.

Such events of course represent rather severe tests, but we cannot consider the models adequately viable until we can account for the extraordinary as well as the usual. For example, the monsoonal circulation-switch which is observed to occur once a year in southern Asia has yet to be simulated by experiment. It certainly is something to watch for.

c) What about short range prediction?

Much of what I have had to say, paradoxically, bears on the short range prediction problem, particularly on the order of a day. You will remember that the very origins of numerical forecasting were concerned with such time spans. It was then asserted that over such short periods the atmosphere behaved inertially so that the sources and sinks of energy can for the most part be ignored. Even baroclinic developments were secondary to the barotropic adjustments. This supposition was largely based on Rossby's work of the late 30's and early 40's. It provided a powerful framework for predicting short-range evolutions of the flow regimes in mid-troposphere. As more sophisticated models developed, it became clear that it was very difficult to improve upon the barotropic forecasts at 500 mb. What *was* gained, was an ability to forecast the short period variations of other properties and of other parts of the atmosphere. The feeling has thus been generally prevalent that the limit of skill for the one-day forecast was essentially attained a long time ago.

The recent experiments that were performed on the nature of the adjustment process in assimilating initial data, however, are beginning to lay question on this conclusion. It appears that a significant portion of the remaining discrepancy between a one day forecast and reality may be due to the transients which are excited by transplanting data from the atmosphere into the more complex models. This is particularly true in the models which are governed by the primitive equations and in which water vapor is a thermodynamically active element. I think the suspicion was clear from the figures I showed earlier that the adjustments required about one day, but more generally from 3 hours to 2 days. These relaxation times are the critical ones for the short-range forecast.

When we learn to assimilate observational data gracefully, with negligible transient excitation, I suspect we will suddenly find ourselves at a significantly higher level of skill for the short range forecast.

There are also some important space-scale characteristics distinguishing the short-range from the longer-range forecast. These differences will probably color both the data requirements and the computational requirements for each. Intuitively, as well as through actual experiment, it is clear that the smaller scale components of the flow have sufficient predictability to be attainable, in say, a one-day forecast. On the other hand, only their statistical properties would be of importance for longer periods. In part, I think this has already been demonstrated by the high resolution experiments conducted by Bushby and his associates (e.g., Burwell and Timpson, 1968) at the British Meteorological Office. This therefore implies, that a very high resolution observation network is more critical for the short range forecast. On the other hand, in both long and short range forecasting, the larger scale components of the flow would be of importance. By the same token, the computational resolution would probably have to be larger locally for the short range forecast. These considerations acquire even sharper focus when one thinks in terms of the filamented water vapor fields in the atmosphere which give rise to significant macroscale precipitation in middle latitudes. Again, this is well reflected by Bushby's results.

5. The other technological hurdle —computers

Around 1950 the very first experimental numerical forecasts (they were barotropic forecasts) were conducted on the ENIAC in Aberdeen by the Institute for Advanced Study group under Charney. These, of course, were one level forecasts for a modest portion of the Northern Hemisphere. When everything went well, and all systems were "go," it required 24 hours of computation to simulate a day's evolution. 18 or 19 years later, computers have grown in speed by fantastic increments (Fig. 12). You can see what the change has been from the IBM 701 in 1953, which in itself was a marked improvement over the ENIAC. Yet despite this we find that the most complex models that are currently under development still require of the order of one hour to simulate one real hour of atmospheric evolution. Now of course, the models are global, and they may have 10 to 20 levels and the physics they embody are indeed more complicated.

It must occur to you that this is a manifestation of Parkinson's Law—a gas fills out its container. One tries to exploit the absolute maximum capacity of available facilities. As in 1950, we still are making compromises with the limitations of extant computational power. As you can see, factors of 2 or 4 are irrelevant.

It is quite clear now that serious mathematical errors are generated in model experiments. These tend to mask the physical similitude of the model behavior to that of the real atmosphere. It is thus often difficult to tell when the physical laws we are imposing are right or wrong, and why and how one must improve them.

Realistic estimates indicate that right now we need computing machines one hundred to one thousand times faster than the best currently available. This is not to meet future needs, but the currently existing ones. Very happily the computer industry does not feel completely at a loss to meet these rather severe demands. It may very well be that the real hope lies in the exploitation of parallel logic, marking the first real departure in logical computer design since von Neumann's MANIAC.

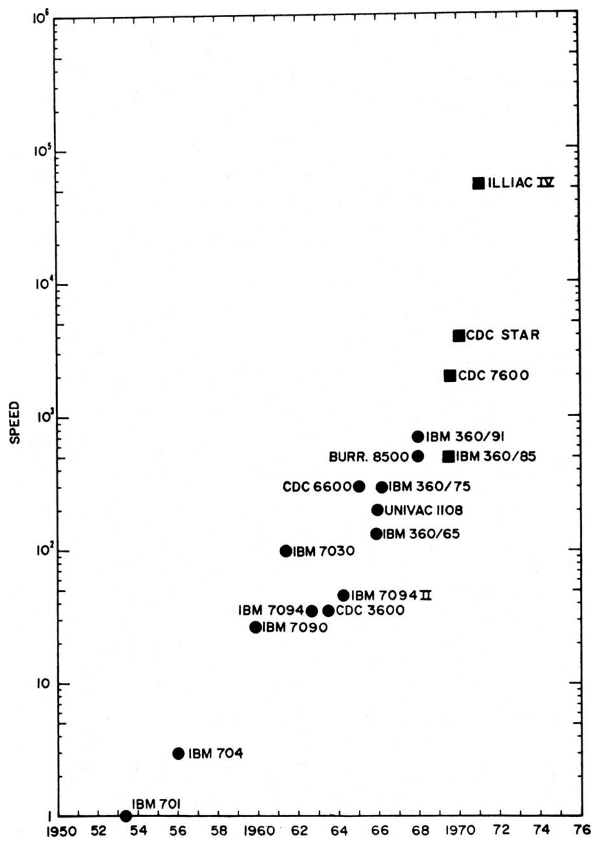


Fig. 12. A "through-put" measure of relative speed of top-of-the line computers at the time of their commercial introduction. The circles are for existing computers as of one year ago and the squares for planned models.

6. Epilog

I think that my remarks of this evening can be summarized as follows.

Numerical experiments within these past few years with the current theoretical models, applied to carefully culled observations taken from the marginal existing network, and compromising mathematical approximation to accommodate present computer power have yielded predictions which for the first time traversed the threshold of accounting for the actual occurrence of new extra-tropical cyclones which in no way were evident from the initial conditions. Even the occurence of the next generation disturbances has been simulated to a discernable extent.

Although the degeneracy of such forecasts at one week is already quite evident, the feasibility is quite clear. In fact there is sufficient integrity even at two weeks to give assurance that diminution of known sources of degeneracy will systematically yield increased fidelity and an extension of the useful range of prediction. We thus should expect to witness an orderly closing of the gap between the *practical* level of predictability and the inherent *theoretical* deterministic limit of the atmosphere.

If I have been at all successful this evening, then it must be clear that, although there is substantial reason to be optimistic about the feasibility of useful deterministic long range forecasting, imposing and difficult obstacles still stand in our way. The intellectual demands are no different from those to be found in other branches of science, except that one might view them in the present context with a greater sense of urgency and a need for single-minded purpose.

By the same token there are technological hurdles to be met. These problems are also far from being satisfactorily solved. However, the utterly fantastic technological advances of the past century, capped most recently by the Apollo 8 expedition, render me confident of ultimate success for our needs.

We become inured by this age of miracles—one moment's sense of amazement transforms immediately to acceptance and a readiness for the next miracle. No doubt when deterministic extended range forecasting becomes an operational reality it too will quickly become an expected and normal fact of life. I suppose the Weather Bureau will continue to receive its quota of letters complaining about the rained out picnic which was scheduled a month in advance based on a routine forecast product.

To conclude I wish to remind you that the Global Atmospheric Research Program is a recognition by the international community of scientists and governments that the time is ripe for the exploitation of key scientific and technological advances, if mankind is to benefit. GARP's organic purpose is to accelerate our understanding of the atmosphere and to make positive a systematic transformation of the gleam in our eye to a working reality. As such, GARP is a fitting epitaph to Harry Wexler.

Acknowledgments

This lecture was based largely on results of research in progress at the Geophysical Fluid Dynamics Laboratory of the Environmental Science Services Administration. Although many of these results have already been published, some are so recent that they are still in a pre-publication stage. However, I felt that the rapid enlargement of scientific experience bearing on deterministic long range forecasting demanded reference to some of these as yet unpublished results.

I am indebted, therefore, to my associates for permitting me to extract conclusions for their recent work. Many of the perspectives I have developed are based particularly on the research of the Experimental Prediction Group, which is directed by Dr. K. Miyakoda. I would especially like to acknowledge his collaboration on the predictability study, which by its appearance here constitutes its formal publication.

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