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ANALYSIS OF ZONAL INDEX IN WINTER

By J.C. Langford

Mr. Langford of the Bureau of Meteorology drew attention to the fact that analysis for the Southern Hemisphere involved a considerable amount of prediction because of the large areas without observations. It was therefore desirable to relate the analysis of synoptic scale systems to the large scale or macro-scale flow. One way of describing a macro-scale flow is by use of a zonal index.

Such a description should enable one to use and develop models of the synoptic scale perturbations. For instance, when the index is decreasing, momentum transfer considerations indicate that troughs should be tilted from northeast to southwest.

Mr. Langford suggested a model of zonal index and its changes in the Australian region. This would be a large-amplitude three-wave flow around the hemisphere. The waves would retrogress, with a period of about six weeks - this period was indicated by the frequency of blocking in the New Zealand region. Such a model helped to explain the variations of zonal wind profiles against latitude at the 500 mb level; sequences of such profiles were presented. An attempt had been made to relate the changes in index to the poleward flux of westerly momentum at 300 mb but so far the results had not been very definite. In relating such a model to synoptic perturbations it was found that the behaviour of these differed in different regions. Thus blocking occurred in the New Zealand region in conjunction with a long-wave ridge, but was not observed when such a ridge lay over the Indian Ocean.

In the subsequent discussion it was suggested that the absence of blocking over the Indian Ocean might be related to the presence of East Antarctica to the south, which tended to produce a cold trough to its north. In answer to a query on the evidence for retrogression, Mr. Langford indicated that the sequence of zonal wind profiles and index variations did not seem to be explicable in any other way.

A.J.T.

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EXTENDED EXPERIMENTAL PREDICTION WITH A NINE-LEVEL HEMISPHERIC MODEL

By J. Smagorinsky

Dr. Smagorinsky, Director of the Geophysical Fluid Dynamics Laboratory, E.S.S.A., Washington D.C., described a series of computational experiments using real data as initial conditions.

The models used were essentially those described in some detail by Smagorinsky et al. (Monthly Weather Review, 1965) and used for general circulation experiments. They have nine levels and a horizontal grid such that 40 intervals span the distance from equator to pole (approximately 270 km in middle latitudes). The resolution is sufficient to describe in some detail the stratosphere, tropopause, boundary layer and frontal zones, and is just sufficient to describe the effects of moist convection. Mountains on the scale of the grid are treated. Radiation, depending on observed distributions of radiating gases and clouds and computed temperatures, is included, together with latent heat release and surface fluxes of heat, water vapour and momentum. Dissipative processes are allowed for, and sea and land are treated differently (infinite and zero heat capacity respectively).

Three experiments from the same initial conditions (24 January 1964, Northern Hemisphere) were described. They are distinguished by either allowing or not allowing for external heat sources, land and sea difference and ice conductivity, while the criterion for small-scale moist convection was also varied.

Time plots of computed hemispheric precipitation and evaporation showed that the system is an equilibrating one. While the temperature is virtually unaffected by the condensation criterion, the relative humidity and precipitation are rather sensitive to it, the 80 percent criterion yielding a mean result much closer to reality than 100 percent relative humidity, and this would effectively dispose of the embarassingly high mean humidities obtained in earlier experiments.

The experiments were all started with zero vertical velocity. Previous experiments have shown that although rainfall and vertical velocity for the first two or three days are underestimated by this procedure, satisfactory adjustment ensues thereafter. In other experiments, initial vertical velocities computed from the W-equation have been used, but the improvement is not remarkable.

With the assumptions of these models, it is possible to separate computed convective from non-convective precipitation. The mean latitudinal distributions of these quantities showed that, proceeding from temperate to tropical latitudes, the precipitation changes from predominantly large-scale to predominantly convective.

An interesting recent conclusion disclosed by the lecturer was that the hitherto always observed tendency to underpredict the intensity of disturbances is now believed to be due to the constant adopted for horizontal diffusion being too great; halving this constant greatly reduced the discrepancies. An allied fault of the models is their overproduction of zonal mean as compared with eddy kinetic energy.

Rigorous verification of the results against real data yielded some encouraging conclusions. Agreement with real data at 11 days and 500 mb was excellent, and this required the correct prediction of new disturbances and of blocking. Root mean square errors were less than those based on persistence up to 11 days, and correlation between observed and predicted height changes remained above .7 up to 10 days at 500 mb, and above .8 at 50 mb throughout the experiment. The most marked degeneration occurred, as always, during the first 24 hours. Precipitation verification over U.S., by the use of a dense network of rain gauges, led to the noteworthy conclusion that the forecasts retained measurable value out to seven to eight days.

A brief mention was also made of Miyakoda's recent experiments, based on March 1965 data, in which he was able correctly to predict the breakdown of the symmetrical polar night vortex, a phenomenon associated with blocking and explosive warming. A re-run of the forecast beginning three days earlier, repeated this signal performance.

Question time produced a number of queries, the answers to which truly revealed the depth and breadth of the lecturer's grip of his comprehensive subject.

In answer to the question, how initialisation procedures could be improved, it was stated that the shock at the beginning of the forecast routine, responsible for the early rapid degeneration, was due to inconsistencies in specification of the prediction elements. For instance, the wind field has to be specified from the mass field, and this inevitably leads to trouble, especially in the boundary layer; also "some of the data are more equal than others". It is better to let the model itself create its own internal consistency wherever possible.
Dr. Smagorinsky reverted to a favorite theme: analysis and prediction should go hand in hand, each new piece of data being assimilated to the model as it becomes available, to reduce the shock of a sudden start. Ideally this will require two computers, one slow and one fast. The former will keep pace with the weather, continuously modifying the fields by the assimilation of

new data, the latter will race ahead of time at required intervals for the purposes of prediction. An observation system optimum for such a procedure, must be designed and put into operation.

Other interesting views gathered from the answers included the following:

The Southern Hemisphere is probably slightly less predictable than the Northern, because of the relative lack of continents to anchor the disturbances.

The stratosphere-tropopause system is reproduced by the general circulation model. In the stratosphere kinetic energy is maintained by coupling with the troposphere. It acts as a sink through dissipation and reversed energy transformations (kinetic to potential). Mr. B. Hunt, of the Australian W.R.E., working in Washington, has recently shown that the polar night jet stream is maintained in roughly equal proportions by tropospheric sources and by baroclinic instability in the stratosphere.

Evaporation of falling rain is not allowed for in the model, but is considered to be an unimportant process and can easily be included at the appropriate time.

The varying roughness of land and sea is not yet included in the models. Its effect is highly interactive and non-linear, as demonstrated by earlier experiments. More should be learnt about the physical processes over the sea.

"Explosive warming" is produced by the models - its "cause" has not been analysed, but it is certainly not extraterrestrial.

Fronts are not the result of land-sea differences. They are produced by the models (as frontal zones, since resolution is insufficient to simulate their natural full sharpness) and are necessary and important as sources of energy through baroclinic instability.

The boundary layer is modelled by allowing for stresses in the lowest three layers. This indirectly forces Ekman effects. More levels are needed adequately to perform this in the tropics, where the computed easterlies are of insufficient depth. The boundary layer experts should give us a theory of stress profiles in the Ekman layer, instead of wind profiles.

The equatorial free-slip wall used in present models prevents the interchange of mass, momentum, etc., between the hemispheres. If we are not interested in the tropics, this is acceptable for predictions up to a week. For longer periods, global integration is necessary. This is in hand, with the adoption of the Kurihara global grid.

The lack of a mechanism for small scale vertical momentum interchange is serious; this is one facet of our difficulties with, and ignorance of, the whole convection process which is particularly important in low latitudes.

Tropical cyclones ("hurricanes", "typhoons") have not been reproduced in the experiments. They represent a second level of instability, depending partly on sea surface temperature. Our sea temperatures have probably not been high enough. There is doubt about the resolution being sufficient to model such small-scale storms, although the scale of the less intense tropical disturbances which appear regularly in the experiments was unaffected when the resolution was doubled.

No attack has yet been made on the problem of very long period fluctuations. A necessary precondition is a better understanding of atmosphere-ocean interaction, which is currently being tackled by Bryan and Manabe. A difficulty is the very long relaxation time of the oceans as compared with the atmosphere.

R.H.C.