

ROYAL METEOROLOGICAL SOCIETY: AUSTRALIAN BRANCH MEETING

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Papers Discussion

A LIMITED AREA NESTED NUMERICAL WEATHER PREDICTION MODEL: FORMULATION AND PRELIMINARY RESULTS.

by D.J. Gauntlett, L.M. Leslie, J.L. McGregor, and D.R. Hincksman

(*Q.Jl R. met. Soc.*, 104, 103-17; paper presented by Dr Leslie)

Pearson Have you considered using sampling theory to verify the forecast?

Leslie No. The model was run over a period of six months on a daily basis during the operational trials and we feel that a sufficiently large number of different types of weather patterns would have been encountered in this time. Moreover, the cost of the trial was relatively low and there was no need to be selective in order to avoid testing on similar types of synoptic situations.

Pearson Is the whole process economically viable; could you charge or offset the improvement in forecast?

Leslie The forecast model is only part of the total automated analysis and forecast system; along with data collection, analysis, initialisation, display, and interpretation. In fact, it is one of the least expensive. The Australian Bureau of Meteorology clearly regards the total system as economically viable and our forecast model is but one (relatively cheap) component.

Bennett I am surprised that the P.E. model performs much better than the filtered model at high levels, where the motion field should be characterised by relatively large scales.

Leslie The greater skill of the primitive equation model in upper levels is largely centred on a better representation of the sub-tropical jet stream. The primitive equations model, working with the actual wind components and involving no quasi-geostrophic approximations in some of the terms predicts the intensity and location of the jet much better.

BAROCLINIC INSTABILITY ON A SPHERE IN TWO-LAYER MODELS

by P.G. Baines and J.S. Frederiksen

(*Q.Jl R. met. Soc.*, 104, 45-68; paper presented by Dr Frederiksen)

Plumb Phillips' criterion for baroclinic instability is a special case of the generalised Charney-Stern condition that the mean potential vorticity gradient must not be single-signed. In Phillips' case with no horizontal shear, this implies that the potential vorticity gradient must be of opposite sign in the two layers. It is straightforward to apply the criterion to spherical geometry (as you have done) and to include the barotropic shear term.

Frederiksen Phillips' criterion on a beta-plane with continuous wavenumbers is both a necessary and sufficient condition for instability, in the case of constant zonal velocity in both layers. For this case there is no barotropic shear and the criterion for incipient instability then takes the form that the product of the potential vorticity gradients in the two layers vanishes. It is suggested in the argument following Eqn 6.2 that one might use the vanishing of this product as a heuristic criterion even in the presence of barotropic shear. Note that for the cases discussed in the article there is no flow in the lower layer and therefore no barotropic term there.

FLUX PROFILE RELATIONSHIPS ABOVE TALL VEGETATION

by J.R. Garratt

(Q. J. R. met. Soc., 104, 199-211)

Baines Have you considered interpreting the results at low levels in terms of the phenomenon of 'bursting', which has been shown to have a substantial influence on turbulent structure in neutral turbulent boundary layers in wind tunnels?

Garratt I presume 'bursting' is here referring to the interaction of the large eddies with the near-wall layer whereby fluid is injected from this layer into the shear flow. Certainly from a number of studies, the large eddies and the associated 'bursting' seem to influence both the mean and turbulent structure to considerable depths within the boundary layer flow. Thus interpretation of measured velocity fluctuation statistics in the wind tunnel, e.g. point correlations of the three components, and of flow visualisation requires consideration of the large eddy, over and above the eddy structure normally associated with a turbulent shear flow, i.e. the small scale eddies whose length scale is of the order of the distance from the boundary.

Nevertheless the concept of an equilibrium near-wall layer with its associated logarithmic law (which may be derived simply by consideration of appropriate length scales) is essentially independent of the large eddy and 'bursting' phenomena. Indeed, the latter concepts must be consistent with such a law.

So far as the transition layer is concerned, I am now attempting to interpret the broad features of the mean velocity and temperature profiles, i.e. their derivation from the logarithmic law in neutral conditions, in terms of additional relevant length scales and mixing length/eddy diffusivity hypotheses associated with the immediate wake fields and flow patterns around the surface roughness elements. At least these are far better understood from wind tunnel studies than is the 'bursting' phenomenon. Quantitatively my present approach seems to be the one most likely to produce a physical understanding of the observations and I am not considering pursuing the possible role of 'bursting' on the mean structure. However, this could well have been necessary if detailed fluctuation observations were available; in the present experiment such data were not obtained.

R.A.P.