



Interactive comment on “Theoretical comparison of subgrid turbulence in the atmosphere and ocean” by V. Kitsios et al.

Anonymous Referee #2

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This paper essentially obtains parametrizations for the dynamical effect of unresolved eddies in quasi-geostrophic models of the atmosphere and oceans. These parametrizations take the form of eddy viscosities of deterministic and stochastic form and are found to conform to well-defined scaling laws. Their success is gauged in terms of their ability to reproduce the kinetic energy spectra of ‘truth’ simulations of much higher resolution. The authors appear to suggest that such parametrizations could bring greater accuracy and computational efficiency to operational weather forecasting and climate prediction.

This contribution is another in a long line of papers from Frederiksen and co-workers that aim to apply turbulence theory (particularly two-dimensional or quasi-geostrophic turbulence). The work has the merit of being mathematically rigorous, in contrast to

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the more heuristic methods typically used in the development of numerical weather prediction and climate models. This, of course, is made possible by omitting large components of those models that are typically referred to as 'physics' (no orography and convection). Even further simplification is obtained by using balanced equation sets for the dynamical core and in particular using the 'clean' spectra formulation in contrast to operational weather forecast models that use semi-Lagrangian advection schemes.

Whilst the authors are to be applauded for their theoretical analysis of the dynamical parametrization problem, it is less clear that their viscosity terms are in any sense comparable with other model error sources associated with unbalanced flow and mountain drag forces.

Like the earlier anonymous reviewer, I note much similarity with earlier papers such as the one in J. Atmos. Sci. 2012. I'm sure the calculations are different but what are the new scientific results ?

In the context of ensemble forecasting, it would have been interesting to address the problem of spread deficiency which is a major concern in weather forecasting currently. Ad hoc stochastic backscatter schemes have been devised specifically to deal with this issue under the pretext of missing upscale energy transfer by sub-grid eddies.

Minor Points

The choice of atmospheric Rossby deformation radius is rather small at 447 km. This may not be very important but I would prefer to see the value at ~ 1000 km.

pg. 1682 line 17. perhaps a comment could be made on the origin of the Error function dependence ?

In the Conclusions, the authors should discuss how their ideas could be modified to take account of the differing dynamical cores used in real NWP and climate models. Their advection schemes are typically quite dissipative and it's difficult to see how the

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turbulence theory could be amended to account for each specific scheme (e.g. semi-Lagrangian/semi-implicit or finite element)

Interactive comment on Nonlin. Processes Geophys. Discuss., 2, 1675, 2015.

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