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Nonlinear Processes in Geophysics
Discussions

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

Anonymous Referee #3

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This paper describes the use of high-resolution 2-layer qg simulations to estimate eddy viscosity coefficients, including back-scatter coefficients supplying energy to the large-scale from the small scale. The basic format is not especially new - use high resolution simulation to estimate eddy viscosities appropriate for a particular truncation scale, plug viscosities in low-res LES simulation, and show that low-res simulation can capture features of high-res simulation. One aspect included here is the back-scatter eddy viscosity, but even that is not that new (see the authors' own publications, and Jansen and Held, 2014). The authors could make more effort to describe what exactly is new in this manuscript. I am not at all an expert in this area, but I note that the authors have one publication which specifically focuses on scaling laws for atmospheric subgrid scale (Kitsios et al, 2012) and another which focuses on the ocean scenario (Kitsios et

al, 2013). What does this new paper contain which is not contained in the earlier two papers? Since I cannot immediately find any new content in this paper, compared to the authors' own previous works, I do not recommend publication.

Specific comments

Title: The title is a bit misleading. Whereas there is a wide variety of different subgrid scale turbulence processes in atmosphere and ocean, most of which are not represented here (for example, the surface mixed layer in the ocean, frictional boundary layer turbulence, breaking internal waves, shear-layers, etc etc), this paper examines only quasi-geostrophic turbulence in the simplest form possible, a 2-layer model. Many features which make ocean subgrid scale motions different from those in the atmosphere, even on geostrophic scales (e.g. side-boundaries and boundary currents) are absent here. Also, the approach here is more empirical than theoretical (e.g. the scaling laws). I suggest a new title: "Parameterizing subgrid-scale quasi-geostrophic turbulence for idealized atmospheric and oceanic regimes".

Inverse cascade range: The scaling laws for the inverse energy cascade range are not included here, but derived in a different paper. How then is this paper different from that earlier paper? How do those scaling laws differ from those in the enstrophy cascade range?

p1682, lines 15-20: Give a justification for these differences in drag for the "atmospheric" and "oceanic" regimes.

p1683, lines 21-22: I found it confusing that the benchmark calculation would have a viscosity that is determined from the scaling laws obtained using the benchmark calculation. Why make things so complicated?

Equation 15: What is the physical justification for this form of the power law? How can you apply the results obtained here, giving eddy viscosity as a function of wavenumber, to grid-point models (as most ocean models are)?

p1692, line 16-18: If the enstrophy flux is required to determine the eddy viscosity, how would this parameterization be used in a coarse resolution ocean model where the enstrophy flux is not known a priori?

Figure 4: what is the dashed line in b and d?

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