Nonlin. Processes Geophys. Discuss., 2, C704–C708, 2016
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2, C704–C708, 2016

Interactive Comment

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

V. Kitsios et al.

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Received and published: 16 March 2016

GENERAL COMMENTS:

We are pleased to learn that the reviewer found the results to be very impressive, and we appreciate the efforts made in this review. As mentioned in the response to the other reviewers, we further emphasise the new and novel aspects at the end of the introduction in the revised manuscript, by including the following points

1. We believe that this is the first ever study to systematically compare subgrid models of quasigeostrophic (QG) turbulence in the atmosphere and ocean. In particular it is the first study were simple unified scaling laws have been presented that

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apply to both media.

2. The study uses a much larger set of simulations covering a much broader range of flow parameters, including an order of magnitude change in the Rossby radius of deformation and the energy containing scale, compared with previous studies.

3. By focussing on the enstrophy cascading inertial range in both media, the large number of simulations and wide parameter range has enabled the establishment of robust scaling laws.

4. The scaling laws presented here are particularly simple with eddy viscosity magnitudes that are proportional to T_R^{-1} and power exponents that are approximately proportional to T_R. These results, and the fact that $\nu_{\mathbf{d}} \approx 2\nu_{\mathbf{b}}$, are suggestive of robust fundamental properties of QG turbulence.

We now address the reviewer's specific points. Associated changes to the manuscript are marked in blue text.

SPECIFIC POINTS:

- *You say that 'an increase in resolution will not necessarily improve the accuracy'. This seems like a secondary consideration; isn't the primary concern that at low (fixed) resolution the accuracy may be poor?*

The resolution dependence problem is an important issue for all model resolutions, both in terms of accuracy and computational efficiency. If the subgrid model is not applied in a self-similar manner, then the small scales (high wavenumbers) are not modelled correctly, and are typically overly dissipated. This range extending from wavenumber, k_D, to the truncation wavenumber, T_R, is referred to as the dissipation range. If we are interested in scales up to wavenumber k_I, then one must ensure that

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$k_D > k_I$. To ensure that the dissipation range does not overlap with the scales of interest, overly high resolutions are required which increase T_R and in doing so also k_D. If the subgrid model is applied in a self-similar manner (using the prescribed scaling laws for instance) then there is no artificial dissipation range and all scales can be trusted (see figure 5 of the manuscript). This is becoming increasingly important as the research community is increasingly using smaller scale (higher wavenumber) information to study aspects such as extreme events.

- *It might help to note that alpha, D0, and kappa in equation (1) are not constants but operators.*

The updated version of the manuscript now states the following in paragraph 2 of section 2:

Using standard fluid mechanical nomenclature, D_0^j is the bare dissipation operator representing the unresolved eddy-eddy (or inter-eddy) interactions in the benchmark simulation (McComb, 1990). The constant α^j parameterises the drag by dampening the large scales of motion. Simulations are nudged toward a climate $\tilde{q}^j$ by the constant relaxation parameter κ^j.

- *It's not clear to me why you would ever have j not equal to k in equations 6 and 7. Why not use the version from the Kitsios et al 2012 paper (eqns 8 and 9)?*

This is the more general case, where one can calculate the flux of enstrophy (and hence energy) from level 1 to level 2 (or from level 2 to level 1). This is particularly important for the situation in which baroclinic instability is not resolved, which is addressed in Kitsios et al. (2013). You are correct, however, in that our current study we are primarily concerned with the case in which baroclinic instability is resolved, and hence the only components of $\eta^{jk}(n)$ that are important are for those where $j = k$.

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- *In the discussion between equations 11 and 12, and in equation 12 I suspect that there are some places where a subscript 0 is missing on t.*

In fact on page 1687 line 15 and line 16, t_0 should be t. This is corrected in the updated version of the manuscript.

- *page 1689 line 20: The fact that the drain is negative in this regime is very interesting; it might be worth mentioning the recent related work by Jansen & Held (2014) who have proposed a parameterization in the oceanic regime also have negative viscosity.*

The updated version now includes the following sentence in the above referenced place:

Jansen & Held (2014) developed heuristic general purpose oceanic subgrid models for this regime that also have negative viscosity.

- *page 1690 line 9 you say "as more eddies are being explicitly resolved, less enstrophy is transferred to fewer subgrid eddies". This statement seems to conflict with the fact that the enstrophy flux is approximately constant through this range of wavenumbers?*

Yes you are correct. The statement in the updated version of the manuscript now reads as follows:

as more eddies are being explicitly resolved, the enstrophy (and energy) is being transferred to fewer subgrid eddies.

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- *Although the energy spectra of the LES show excellent agreement with the DNS, this is only one measure of accuracy. One might also check things like meridional heat flux or large-scale EOFs.*

The Lyapunov equation (equation 12 in the manuscript) ensures that the subgrid terms (effectively Reynolds stresses) balance exactly; therefore, the mean heat and momentum fluxes must also be appropriate in order to get the mean flow correct. In our studies the mean climate state of each of the LES variants matches with that of the DNS with pattern correlations of at least 0.9994. Recall that the subgrid modelling is undertaken in terms of the potential vorticity variable. As defined in equation 2 of the manuscript, the potential vorticity by definition incorporates both the effects of vertical shear ($\zeta_1 - \zeta_2$) and also horizontal shear, via the relationship between the streamfunction and vorticity. The potential vorticity fluxes are essentially equivalent to heat and momentum fluxes.

The updated manuscript can be found in the supplementary material.

Please also note the supplement to this comment:
http://www.nonlin-processes-geophys-discuss.net/2/C704/2016/npgd-2-C704-2016-supplement.pdf

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

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