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Interactive comment on "High-resolution large-eddy simulations of sub-kilometer-scale turbulence in the upper troposphere lower stratosphere" *by* R. Paoli et al.

Anonymous Referee #1

Received and published: 29 January 2014

General comments:

This manuscript aims to study sub-kilometre scale turbulence of the upper troposphere and lower stratosphere (UTLS) via large eddy simulation (LES) with a comprehensive mesoscale atmospheric model (Meso-NH). Comparisons are made throughout to previous theoretical and computational results on stratified turbulence. The objective of this work is important and interesting, because most previous work on stratified turbulence has focused on very idealized turbulence-in-a-box, using spectral models with triply periodic boundary conditions, constant buoyancy frequency, ad hoc forcing, dry dynamics, etc. The use of a more comprehensive atmospheric model, like Meso-NH,

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allows for the study of stratified turbulence under more realistic atmospheric conditions.

I was therefore somewhat disappointed to find that the authors do not in fact do this. Their set-up is just as idealized as the usual turbulence-in-a-box studies: the buoyancy frequency is constant, the moist physics is turned off, the forcing is ad hoc random Fourier based. As a result, their results mainly reproduce the findings of previous papers. Despite the title, these are really idealized turbulence-in-a-box simulations, not simulations of the UTLS. I don't really see why it is necessary or very interesting to reproduce previous spectral model results with Meso-NH - it would have been far more interesting and valuable to use the added physics in Meso-NH to look at stratified turbulence under more realistic conditions that cannot be captured in a spectral code.

One new contribution in this work is the use of LES to study stratified turbulence. Most previous work has used either molecular or hyper viscosity, and I think the present work makes an important contribution by exploring how the SGS scheme performs on stratified turbulence. In particular, the findings on how resolved the Ozmidov scale need to be is quite useful and new. Another new contribution - less interesting than the LES but still possibly of interest to some readers - is the verification of Meso-NH through comparison with other higher-order spectral simulations.

Despite the above reservations, I found the paper to be well written and very well grounded in previous work. The simulations seem carefully designed and analyzed and the resolution is impressive. While I do think the authors could have focused their work in a more useful and novel way, I don't think it is reasonable to ask them to redo these simulations. Given the contribution made by their use of LES, all in all I have no real objections to publication.

Specific comments:

1. It would be nice to see some additional justification for the choice of forcing, especially for its relevance to the UTLS. Have you checked sensitivity of the results to forcing? Are wave and vortical modes both forced? (It is stated that vertical velocity is

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not forced, but what about horizontal divergence?) Why is the time scale set at 33.6 sec?

2. What is the point of including water vapour in these simulations, given that the reference altitude is 11 km and no moist physics is included? Surely the effect of vapour on buoyancy will be negligible.

3. In the discussion of $\langle u \rangle_h$ in and around equation (20), reference should be made to the vertically sheared horizontal flow (VSHF) modes of Smith Waleffe (2002). Do you find that the energy in these modes grows as in Smith Waleffe?

4. On page 31905, it is said to be "interesting" that the resolved KE is independent of resolution. Shouldn't this be expected, since the forcing is at large scales? The following statement that this gives "a posteriori verification that the turbulence model has no or limited impact on mean resolved quantities" seems like an overstatement. All it means is that the energy in the energy containing scales (ie the forcing scales) is independent of the turbulence model, which is to be expected.

5. It would be helpful to report the Froude number of these simulations for better comparison with previous work.

6. The horizontal kinetic energy spectra with strong and moderate forcing are actually consistently shallower than -5/3. This finding should be mentioned and investigated. It would also be helpful to compute the spectral slopes objectively with a least squares fit. Interestingly, it seems to be the weak forcing case that is closest to a -5/3 spectrum. Previous studies (Waite 2011, Augier et al 2013) found a spectral bump at horizontal scales close to the buoyancy scale, which could account for the shallower-than-5/3 spectra reported here - this possibility should be explored.

Similarly, the vertical spectra seem consistently shallower than -3. This should be discussed.

7. For the comparison with observations, I don't think it makes sense to compare the

spectra in these simulations with observed spectra at much larger scales. Indeed, why would it be justified to assume "that the spectrum can be extrapolated down to the sub-km scale" as stated on page 31913?. The simulated spectra should be compared with observed spectra at the appropriate scale.

Technical Corrections:

8. In equation (32): shouldn't you include perturbation buoyancy (not just N^2) in the definition of Ri?

9. Above equation (37) "Fouriers" should be "Fourier"

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 31891, 2013.

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