The paper compares various approaches for the dealiasing of the non-linear terms in large eddy simulation of atmospheric flows. Given that spectral methods are widely used in studying such flows under idealized conditions since they offer higher speed and better accuracy, the general theme is of interest to GMD readers. The paper does a good job in presenting the fundamentals of the problem, the proposed solutions, and how they compare when implemented in an actual code. But major revisions are needed. In particular, the study is a valuable comparison of the methods that is not available (to the best of my knowledge) in the literature and the authors should not try to conclude that one is more optimal than the others. They can simply present their findings and let the users determine which method is suitable for their needs.

Major comments:

1. While the FS method seem to be giving an acceptable performance as the authors argue, I wonder whether the ABL LES community should be going in a direction of saving computing time rather than maximizing the accuracy of the computation. We push for higher resolution to gain better accuracy and, with increasing computing power, I wonder whether a 20 % drop in simulation time is worth it. We use dynamic SGS models that increase the computing time by 20% all the time. The plots in Fig 7 do not indicate that the FS method is as good as the 3/2 method. So in general I think the authors should not focus on the conclusion that the FS method is a good surrogate. They should present the information and findings, which will help modelers decide on the tradeoffs they want (on my end this convinces me that using a 3/2 method is indeed worth it.).

2. How do the FT and FS method influence the potential use of dynamic models that require good accuracy on the smallest resolved scales? If as the spectra show they damp these scales, than that would preclude using dynamic models and would be a significant disadvantage of FT and FS. The authors have in their code some dynamic models, they could perform the dynamic computations while still using the Static Smagorinsky (compute a dynamic Cs but don't use it).

3. Fig 8d and the associate sentence "Interestingly, results of the vertical flux (or stress, resolved and SGS) of stream-wise momentum (figure 8(d)) illustrate a good agreement between the different scenarios." The authors should be careful in this interpretation. The constant pressure gradient forcing requires and forces the stress profile to be linear. Regardless of how turbulence ends up looking like the turbulent fluxes have to adjust to balance the mean $\partial P/\partial x$. What this figure indicates is that the SGS fraction is not strongly affected by the choice of dealiasing method, which is a good thing.

4. Figure 9, and more generally: I would have liked to see a direct comparison of the largest scales (by filtering all simulations at $n\Delta$, where n correspond to start of the damping or cutoff in figure 1) to see if the differences are only on the smallest scales or not (although given the mean velocity profiles, I suspect they are not).

Minor comments

1. Title is long and too descriptive: how about replacing the wordy "atmospheric boundarylayer type flows" with "atmospheric flows". One in fact could foresee using such methods for cloud resolving LES outside the ABL.

Same on last line of abstract: why restrict the applications only to ABL flows?

- 2. Abstract line 3: better to replace "integrating" by "time advancing"
- 3. Abstract lines 4-5: not sure what is meant by "This is of special relevance when using high order schemes." Spectral schemes are always "high order"
- 4. First 3 lines (19-21) of introduction. In fact the # of grid points in LES has not been following Moore's law. See https://doi.org/10.1017/jfm.2014.616 . This shows that the LES community has not been taking full advantage of increasing computing power to improve model accuracy, which I think is a remiss.
- 5. Page 2, line 2. Add comma after "With increasing computer power". I think there are a few other missing commas after introductory phrases.
- 6. Correct **wall bounded flow** to **wall-bounded flow** on page 2 line 7.
- 7. Page 2, Lines 30-32: authors talk about the need to expand the grid onto 3/2N and then say "As a result, due to the non-linear dependence on N" This example make it sound linear. I think they are referring to the non-linearity of the FFT cost with N, which they explain later. Clarify.
- 8. Also maybe they should clarify that if FFTs are used in 2D, the cost rises even more quickly as N rises.
- 9. Page 3 line "via a set of LES of fully developed ABL type flows and with a corresponding comparison on the effect in turbulent flow statistics and topology". Convoluted phrase. Simplify.
- 10. Page 3, line 8 "environmental fluids"
- 11. Page 3, line 19 "thus discouraging its use in most practical situations" are the authors certain of this statement? The 3/2 rule is use very very widely. If not maybe their paper would be a good reference for modelers to see the advatanges of using it.
- 12. Page 5 line 13, and maybe other places. Referring to the production range as the energy containing range is inaccurate and misleading. The statement "For this technique to be successful, the low-pass filter operation must be performed at a scale smaller than the smallest energy containing scale, deep in the inertial sub-range according to Kolmogorov's hypothesis (Kolmogorov, 1968; Piomelli, 1999)." For example makes no sense if that jargon is used. Please fix and use energy production range instead.
- 13. Eq 8, the τ should have a d superscript if the trace is already in p* as the authors write.
- 14. Page 6, line 18: the log law is not inviscid since it is derived from matching the viscous sublayer and the outer layer. If the wall is smooth for example z₀ depends on viscosity.
- 15. Page 6 line 25: what does "module" mean? Do they mean modulus?
- 16. Should equation 12 include f_i to be consistent ?

- 17. Page 8, lines 10-15: Authors should clarify this is with the baseline 3/2 dealiasing I presume. Also how does the parallelization method impact these numbers?
- 18. Page 10, lines 1-2: The z_0 they impose is 1cm, which corresponds more to a grass field than to a sparse forest of to a farmland. I suggest they check Brutsaert's books rather than to Stull for z_0 .
- 19. Figure 5 and other are difficult to read. Why not use colors for the online version (Color is free with EGU, no?)
- Page 11, lines 9-11: 30% drop in the convective term cost is good but I would not say it is significant. It would only be equivalent to about 20% drop in total computing time (given Fig 1), which would only be equivalent to a 5% reduction in the resolution. So I would remove "significantly" on line 9.
- 21. Page 11, lines 10-11: "the predicted computational cost predicted by" remove one of the "predicted".
- 22. Page12, line 2: replace "extend" with "extent"
- 23. Page12, line 9: correct the misspelling of "stream-wise"
- 24. Page 12 line 25, and page 14 line 10: "differentiated" is an unclear word. Please remove and clarify the two sentences.
- 25. Page 14 lines 15-16. There won't be any dispersive stresses in their simulations over homogeneous terrain so why mention them?

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