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Interactive comment on "Comparison of dealiasing schemes in large-eddy simulation of neutrally-stratified atmospheric boundary-layer type flows" *by* Fabien Margairaz et al.

Anonymous Referee #1

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General comments: This manuscript addresses the computational cost and the consequences of using different dealiasing schemes in the advective term of the momentum equations for pseudo-spectral discretizations, such as those frequently used in simulations of turbulence in the planetary boundary layer. In particular, two more economical approaches are contrasted with the exact 3/2 rule. Overall the results presented may be relevant for the community interested in turbulence simulation in geophysical settings, such as the planetary boundary layer and the ocean mixed layer. The paper is well written, even though a more concise version would probably be more appealing to the reader. I am a bit surprised by the low cost of the Poisson solver, and the choice of a very low-cost subgrid-scale model certainly helps increasing the share of

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Discussion paper

the convective term on the total cost (as briefly mentioned in the text). After reading the manuscript, I would probably still stick to the 3/2 rule, but that is just my opinion. Having a lower-cost alternative may be useful to other scientists working in the field. The topic may be a bit on the margins of interest of GMD, but I do recommend it for publication. Some important remarks follow below.

Specific comments:

1. The authors perspective on the FT method is a bit different from mine, and to be honest, I am not sure which one is the most prevalent in the community. I have always considered the FT method (which is also known as the 2/3 rule) as a slightly different implementation of the exact 3/2 rule, but with a reduced number of effective grid points (actually Neff=2N/3 instead of N). In my view, performing a calculation with 3N/2 points using the FT method should be identical (in simulation results but maybe not in computational cost) to a simulation with N points using the 3/2 rule, no? I also think the that simulations using the FT method should actually report grid resolution based on Neff and not on N (i.e. the grid spacing should be Dxeff=3Dx/2 instead of Dx), but that is usually not done. Maybe the authors can comment on this?

2. I am surprised by the high share of the cost carried by the convective term compared to a very low share carried by the Poisson solver. Is this something that is specific to the pencil decomposition parallelization? It may be the case that padding with the 3/2 rule is not as simple in a pencil decomposition approach given that one usually needs to pad the entire 2D "horizontal" wavenumber space? Maybe some details of the padding in the context of pencil decomposition would be useful. In addition, is it possible that the Poisson solver is faster when pencil decomposition is used?

3. The quantification of computation cost is section 4.1 is fine. Regarding the results in section 4.2, I wonder if the question could be posed the other way around. In my view, maybe the most relevant question would be: "given a set computational cost, do I get better results (a) using the 3/2 rule or (b) running a finer grid using a less

expensive dealiasing?". I know the experiment is not easy to design (because one needs to estimate the computational cost ahead of time), but that seems to be the more important question to be answered. If I save 30% on dealiasing and spend it on more resolution, do I get better results? From my comment above, I would expect that the FT method is almost equivalent to the 3/2 rule (if smartly coded). How about the FS?

4. Regarding the interpretation of Figure 7. I am not convinced the log-law prediction is as good as described in the text (but perhaps I am missing something here?). First, it seems that there is one log-law on top of the solid line that may extend only for 2 or 3 vertical levels (ending around z/zi<=0.02). Then there is a second log-law (only in 3/2 and FT cases) that starts around z/zi=0.04 and goes beyond z/zi=0.1, This second log-law has the incorrect roughness (if one were to extrapolate it to u=0, it would yield a lower value of z0 than the one imposed on the simulation, I think). There is no clear second log-law in the FS method. I would see this as a concern for the FS method (which is being advocated here), except that not even the exact 3/2 rule has a good log-law (as seen in Bou-Zeid et al, 2005). I am pretty sure this is due to the SGS model adopted here. In any case, if I had to choose between the FS and FT methods based on Figure 7, I would probably go with FT, since it does a reasonable job in the lower 20% of the domain (which is the region of interest in a simulation like this, I guess). Also, the FS method seems more over-dissipative near the wall (on panel b), which is opposite to what is described in the text?

5. Regarding Figure 9. I do not agree that the filtering only affects the small-scale end of the spectra. For the FT case, it seems clear to me that there is significant damping of the large scales as well. This is related to the underestimation in the variance of the streamwise velocity seen in Figure 8. This is worrisome, and probably related to the fact that the true resolution here (Neff=85 points) is too coarse to model the ABL. I wonder if this situation would persist in the 256ˆ3 simulations?

6. I see two facts that could benefit from more discussion in the manuscript (maybe in

the conclusions):

a. The importance of reducing computational cost in the advection term for simulations that use more sophisticated SGS models (maybe this can be brought up again in the conclusions?)

b. The consequence for including one or many additional scalar fields (temperature, water vapor, etc.). These also require dealising and do not require additional pressure solvers and/or very expensive SGS models. I would probably anticipate that in simulations with several scalars, the savings would be significantly larger. Is that correct?

Technical corrections:

1. Page 4, Line 33 - please check that the Nˆ3 term in the cost is correct here.

2. Page 5, Line 10 - delta implicit does not really correspond to a top-hat filter. The properties of an implicit filter are tied to the discretization scheme. As an example, for a true 3D spectral code the implicit filter is a spectral cutoff filter.

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