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Geoscientific Model Development Discussions

Interactive comment on "Mesoscale nesting interface of the PALM model system 6.0" *by* Eckhard Kadasch et al.

Anonymous Referee #1

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This article describes the development of a preprocessor (INIFOR) that enables nesting of the PALM LES model within the mesoscale model COSMO. Besides the required coordinate transformations and interpolations required to provide boundary conditions for PALM, which are then read in as the netCDF files, the authors describe two additional steps performed within PALM that consist of an approach to remove residual divergence required for their incompressible LES solver, and a method to superimpose synthetic turbulence fluctuation into the smooth mesoscale velocity field from COSMO, employing the method from Xie & Castro (2008). The mesoscale-LES nesting capability is demonstrated with an idealized diurnal cycle, with a focus on the effectiveness of the synthetic turbulence generator. The article is well written and organized, and contains a good level of details required to understand the specific model development

and subsequent capability being developed. However, there are a number of important aspects that need to be addressed before the manuscript can be considered for publication in Geoscientific Model Development. My comments are listed here below in order of appearance in the manuscript:

1) Page 3, line 4. "Muñoz-Esparza et al. (2014) implemented the" should be "developed and implemented".

2) Page 3, lines 9-10. Where is the 15 km taken from? It seems to out of context. Please be more specific. Fetches of $\sim$5 km are typically reported for convective ABLs in Munoz-Esparza and Kosovic (2018).

3) Page 3, lines 15-17. This is an incorrect statement. Even in neutrally stratified conditions, there is thermal variance, although small. The fact that the cell perturbation method uses thermal effect to break up the two-dimensionality of the incoming flow does not mean is not applicable to shear-driven cases. Munoz-Esparza et al. (2015) explains these aspects in the context of neutrally stratified ABLs, so please correct this statement. In addition, for SBLs, there is buoyancy suppression, but this does not mean that the cell perturbation method "changes the physics of turbulence generation" as the authors mention either.

4) Page 3, lines 25-26. Here again the authors seem to be confused with what the "physics of turbulence" is and what the inflow turbulence techniques provide. The synthetic method, even if "inspired" by some scaling arguments, do generate "artificial" turbulence, which is not consistent with either forcing or the discretized governing equations. I see the authors are attempting to justify their choice by this is a biased statement. It is sufficient to mention it is a different approach, so I would suggest removing this sentence.

5) Page 3, lines 28-29. Also, in fairness, you should mention the computational cost, and how that compared to the cell perturbation method you have previously mentioned.

6) Page 4, line 24. “stochastic” -> “synthetic”.

7) Page 7, line 27. Is COSMO a truly fully compressible formulation or is there acoustic filtering being applied?

8) Page 16, line 11 to end of the page. I do not think this discussion is required. This is a nested capability, and the great majority of NWP and LES model practitioners understand the concept of nesting. If the authors are willing to keep the information in the manuscript, I would suggest moving it to an appendix, since it is not part of the main body of content of the paper.

9) Page 18, Eq. 26. The use of this equation for convective conditions is highly questionable. The assumption that turbulent velocity correlations can be approximated by an exponential function is only reasonable for neutrally-buoyant shear flows. The presence of stability effects, breaks this assumption, so the use of a 2D synthetic flow field does not appear to be justified. The authors need at least to mention this strong limitation, and clearly acknowledging in the paper that they are using a method that is not designed for what they are using it for.

10) Page 20. These parameterizations of velocity variances from Brost ewt al. (1982) are for offshore boundary layers. How do you justify this choice? Such limitation needs to be acknowledged.

11) Page 23, Figure 7. I would suggest including wind direction profiles to help the reader appreciate the amount of directional variability (as described in the text).

12) Page 23, lines 5-6. Terrain is a key aspect in this type of nested simulation, where the terrain resolution changes drastically between the mesoscale and LES domains. Even if the authors use a flat terrain for their demonstration simulation, they should comment on how different terrain and atmospheric profiles are matched at the lateral boundaries of the LES domains.

13) Page 24, lines 14-16. This step does not seem to be necessary since you know

the mesoscale time-varying forcing.

14) Page 25, lines 10-11. Please define explicitly what the inner part is, both location and extent.

15) Page 25, lines 18-19. This could be related to the lack of turbulence in the residual layer.

16) Page 26, figure 9. RES does not seem to be defined. Do you mean REF?

17) Page 26, lines 3-6. It would really helpful to plot vertical profiles of theta and other quantities very near to the LES domain boundaries. This may shed some light into how PALM decreases temperature from the boundary forcing.

18) Page 26, line 15. It should be “W m-2”.

19) Page 27, lines 5-6. Then, what is the issue? Looks like there is some imbalance, likely occurring near the lateral boundaries. This needs to be further investigated as it is a key aspect of the coupling between mesoscale and LES models.

20) Page 27, lines 9-10. This is confusing. Higher than what?

21) Page 27, lines 23-26. This can be prevented by truly embedding the LES domain within the mesoscale solution. I suggest the authors do so and report on their findings.

22) Page 27, lines 33-34. See previous comment. I believe this is due to your specific forcing settings.

23) Page 27, line 34 to end of the paragraph. It is unclear what the authors are trying to covey in this paragraph. Please rephrase.

24) Page 31, line 6. Why wouldn’t you use u*, w* and H0 from the mesoscale too? These turbulent properties are not going to be reliable in the inflow region, as your resolved turbulence is not yet spun up.

25) Page 32, lines 3-4. There is a spurious kink toward the top of the ABL, likely

induced by the 'fading function' that does not look very smooth or reasonable. This aspect should be explicitly mentioned.

26) Page 32, line 16. This is not representative of what Fig. 14a shows, which is more ~25 km to somewhat stabilize (and not even at all heights). I suggest the authors quantify the fetch. This can be done by using the last 10 km of the domain, where the solution looks stabilized, and use the average over that region as the 'target'. Then, you define equilibrium when you are stably within a 10% of that value.

27) Page 32, lines 18-19. This value is again highly biased and underestimated. At least 15 km are required. Please see my previous comment.

28) Page 35, line 5. If they are too energetic and keep varying their TKE with fetch, one cannot claim these structures have a reached a quasi-equilibrium state.

29) Page 34, lines 11-14. It is not until the surface reaches equilibrium that the flow can do that, since forcing at the surface is evolving, so it makes sense TKE is delayed compared to surface properties.

30) Page 34, line 23. Munoz-Esparza & Kosovic (2018) propose Uzi/w* as the parameter that indicates when inflow turbulence does not make any difference vs progressively increased fetches. It would be appropriate to mention that here.

31) Page 34, lines 25-26. This number is significantly underestimated. Please correct.

32) Section 4.4. Given the presence of under-resolved convective structures in the mesoscale solution, the 1-h time frequency of the lateral forcing mentioned earlier in the manuscript seems insufficient. The authors likely need to make that ~1 min and rerun the simulation.

33) Page 35, last line. The estimated wavelength is 2-4delta, which seems too small to be resolved given the effective resolution of the fifth-order upwind advection scheme used by the authors. Could the authors describe how is the wavelength estimated?

34) Page 39, lines 21-23. Munoz-Esparza et al. JAMES2017 discusses a way to eliminate these structures, and that would be pertinent to mention here.

35) Page 40, Figure 19. Please include wind speed and potential temperature contours as well. This may help you diagnosing the issue with the LES over-cooling.

36) Page 40, lines 6-7. Is this a result from the divergence-free adjustment not being totally effective? Please comment on this.

37) Page 43, lines 1-3. Again, this should be updated to report a more realistic value according to the presented results. It is more ~3.0.

38) Page 43, lines 4-5. Munoz-Esparza et al. (2015) showed on a apples to apples comparison (i.e., same LES model, forcing, etc) that the cell perturbation method required shorter fetches compared to Xie & Castro (2008). This should be mentioned. Also, for convective conditions, cell perturbation results reported in Munoz-Esparza & Kosovic (2018) are smaller than 2.0uhzi/w*, which is shorter than required fetches presented herein, more ~3uhzi/w*. I agree these can be called 'similar', but there are considerable differences that deserve to be mentioned. Also, the behavior of the fetch development with the cell perturbation is more systematic and have been show to produce well equilibrated solutions, while here there is for certain cases a lack of development. The authors need to mention this aspect.

39) Page 43, line 20. This is likely caused by the lateral boundary conditions that do not match mesoscale variability (i.e., they are uniform in space and do not change between the LES domain boundaries).

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