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Geoscientific Model Development Discussions

Interactive comment on "Implementation of a synthetic inflow turbulence generator in idealised WRF v3.6.1 large eddy simulations under neutral atmospheric conditions" *by* Jian Zhong et al.

Anonymous Referee #2

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The manuscript "Implementation of a synthetic inflow turbulence generator in idealised WRF v3.6.1 large eddy simulations under neutral atmospheric conditions" by Zhong et al. submitted to the Geoscientific Model Development (GMD) describes the implementation of an existing synesthetic turbulence generator to the Weather Research and Forecasting (WRF) model, with the aim of reducing the inflow fetch distance for nested simulations down to the large-eddy simulation (LES) scale. They tested a neutral boundary layer (NBL) case, and performed sensitivity study of a key length scale in their turbulence generator. The results were then evaluated against a standalone periodic LES simulation.

This work will benefit the atmospheric community by providing then with a practical engineering tool for improving nested simulations at the LES scale. Implementing a piece of code like this into WRF is no "a walk in the park", it must have taken the authors a great deal of time and effort. For that I appreciate their efforts, and applaud them for making their code publicly available with this manuscript.

But regarding the contents, I am afraid that I fail to see the scientific novelty with this manuscript. It seems that all they did were to document the performance of an existing method on one particular case. One way to improve this manuscript is for the authors to interpret their results based on more detailed analysis rather than speculation, so that the readers have a more fundamental understanding of the strength and weakness of the synthetic turbulence generator applied to the atmospheric boundary layer flow. For example, regarding Fig. 4f, the authors observed that the TKE profiles at 0.1H requires a longer fetch to converge to the periodic solution, and commented that this maybe due to "downward turbulence transport from above". My suggestion is then don't stop at this speculation, investigate it by plotting the resolved TKE budgets and prove or disapprove your hypothesis. I have listed a few suggestions in the major comments, but the list is by no means exhaustive.

Finally, please, please improve your English writing, proof read it carefully and invite a native speaker to proofread the manuscript before submission. Overall, I suggest major revisions.

Major comments:

1. Add more analysis to help interpret your results, as I have mentioned in the overall comments, speculation is hardly helpful. After you document the various mean profiles and turbulence statistics, analyze them to help us understand why.

2. I suggest the authors add a control case where inflow contains no turbulence information, just the mean profiles. This way the readers could have a much better sense of the advantage/power of the turbulence generator by comparing the results to the

control case.

3. When presenting the various profiles and spectra, I suggest adding profiles/spectra at x/H = 0, i.e., the inlet profiles directly from the turbulence generator. This way, we have a better sense of the direct output from turbulence generator.

4. I wonder if the shape of the integral length scale profiles in Fig. 1a matter for the results. The step function like integral length scale in the streamwise direction Lx worries me a little bit, and please elaborate on your "canopy" argument for Lx. Furthermore, the relative importance of these integral length scale profiles is also of interest. For example, what if you only vary Ly but keep Lx and Lz the same in your sensitivity tests?

5. The model setup also worries me. In Page 6, your domain depth is 0.5 km, and if I understand correctly based on your Line 7, the boundary layer depth is also 0.5 km. Such a shallow domain depth might cause undesirable reflections back into your domain, unless you are using radiative top boundary conditions. Is that implemented in WRF? Please comment/give more information on the top boundary condition used.

Minor comments:

1. Page 2, Line 4, "The WRF-LES model can capture the intermittency of three-dimensional turbulent eddies". Could you provide a reference please? It would be useful to the readers. I am also curious to learn about studies on turbulence intermittency using WRF-LES.

2. Page 2, Line 5, "There still remains a challenge for downscaling from mesoscale simulation (down to 1 km) to the LES scale (tens of meters or below) (Doubrawa et al., 2018)." Please, summarize brief what this challenge is.

3. Page 2, Line 7, "Most WRF-LES models . . . uses. . ." please fix your grammar.

4. Page 2, Line 8, By "These brave assumptions", you actually meant the one brave assumption of periodic boundary conditions only. Please improve this sentence.

5. Page 2, Line 12. I am confused about your "As one step moving towards enabling WRF's capability of nesting. . .". Why and how would the synthetic turbulence inflow scheme help with nesting? I guess this is related to my earlier point that you need to lay out clearly the difficulties of meso-to-microscale nesting first, before diving into your proposed method.

6. Page 2, Line 18 "turbulence" not "turbulences".

7. Page 3, Line 21, "It is thus not surprising that a very long distance, e.g. 20 – 40 boundary layer depths, is normally required to allow a transition to fully developed turbulence." This statement might be misleading. My understanding is that the cell perturbation method (CPM) of Munoz-Esparza et al. (2014) applied to potential temperature requires only a short distance before turbulence is properly spun up, even for the neutral boundary layer (see their Fig. 7). This is also true when CPM is applied to velocity (Mazzaro et al., 2019, JAMES, 11(7):2311-2329). The author should clarify or give a proper reference to the fetch distance of "20-40 boundary layer depths".

8. Page 3, Line 25, "flows" not "follows".

9. Page 4, Line 4, "energy-taking resolved eddies" ? This sounds very strange.

10. Eqs. 1-2, perhaps you are using the Favre filter in these equations, or perhaps you are using the Boussinesq approximations for the PBL, please clarify. Eqs.1-2 are not the governing equations for compressible flow as you indicated in Line 3.

11. Eq. 7, this is a parameterized TKE equation where turbulent transport and pressure correlation terms are parameterized. It is also written without the buoyancy term, and should therefore only applicable to a vertical depth within the NBL, but not above the boundary layer where stable stratification prevails. Unless the authors intend to adopt an isentropic background state for their simulations, I suggest including the buoyancy terms for completeness. The use of the mixing length "l" as the dissipation scale is another assumption that should at least be mentioned.

12. Eq. 15, please explain the meaning of the “alpha” inside the matrix. It also looks strange that you shall write a_{ij} in a matrix form in Eq. 15. Shouldn’t alpha_{ij} be an element of your matrix, rather the entire matrix itself?

13. Page 6, Line 9, what do you mean by “a constant Lagrangian time scale T (Eq. 13) using Taylor’s hypothesis” ? please give more detail here, how did you determine your “constant T” value?

14. Page 6, Line 10-11, “canopy height”? Why suddenly canopy height? What’s the purpose of placing a canopy layer in your NBL simulations?

15. Page 6, “. . .,explained in Xie and Castro (2008)”. Please fix your grammar.

16. Page 6, Line 14-15, “the vertically same wind direction”, please fix your grammar.

17. Page 6, Line 19, “in the lateral direction”, did you mean “spanwise” direction? Same for the rest of this paragraph. Lateral suggests both x- and y-directions.

18. Fig. 1, caption, use “relative computation time” as in your main text, rather than “relative computation”.

19. Fig. 1, “dashed grey line of 1.0 indicating”, indicates, not indicating.

20. Page 7, Line 9, “filtered velocity” rather than “filter velocity”.

21. Page 7, Line 14, and elsewhere. Please double-check on the GMD conventions, but I think you should spell out “Figure” if it is at the beginning of a sentence.

22. Fig. 2. Caption, “(b) The . . .” change to “(b) the. . .”

23. Page 7, Line 16, “are advected and decay downwind. . .”, please fix your grammar.

24. Page 7, “can generate realistic well-configured turbulence structures from a short adjustment distance downwind”. The adjustment distance does not look short to me. Judging from your Fig. 2b, it looks like a fetch distance of x = 5H is required at least. Please comment on this.

25. Page 7, Line 21 to 22, “and there is no adjustment distance, and instead, an adjustment time to generate fully-developed turbulence structures”. Please fix your grammar.

26. Page 7, Line 28, “plan” or “plane”?

27. Fig. 3, I suggest using the “global friction velocity ðİŠŰâĹŮ” from the periodic case to normalize u* for the inflow case. This way, we could detect the presence of systematic biases in the inflow case, if any.

28. Fig. 3. caption “(laterally and temporally)”, laterally and temporally averaged?

29. Page 8, Line 10, “a good agreement against ?” Please improve this sentence.

30. Page 8, Line 12, can you comment on the possible reason for the slow convergence (long fetch distance) of wˆ2 at 0.1 z/H ?

31. Page 8, Line 15, why would “a larger shear-generated TKE” slow down the adjustment at 0.1z/H? Shouldn’t this accelerate the adjustment because more TKE is generated locally independent of the TKE contained in the inflow.

32. Page 8, “downward turbulence transport from above” Did you look at the TKE budget? The transport term of TKE is quite insignificant in the NBL. Unless the inflow case is doing something less. It would be nice if you could present the TKE budgets and compare between the two cases.

33. Page 8, “The red circle dots”, just “red circles” will do.

34. Page 8, Line 21, “noticed again” or “noted again”?

35. Fig. 6, caption “< ðİŠŰ > and < ðİŠŰâĂš > the laterally averaged mean and streamwise normal Reynolds stress”, how are these Reynolds stresses? These are first-order moments.

36. Page 9, Line 17, “is able to sustained”, please fix your grammar.

37. Fig. 6, could you include a spectrum at the inlet x = 0, so that the readers have an idea of what the synthetic turbulence spectrum looks like?

38. “A length scale (LS) ratio . . . are tested.” Please fix your grammar.

39. Page 9, bottom line “Fig. 8 (a) shows that < ðĪŚć >/ðĪŚćâĹŮ is slightly greater for the LS ratio less 1.0 (see Fig. 8a for comparison). This is due to a greater Reynolds shear stress < ðĪŚćâĂšðĪŚďâĂš >/ðĪŚćâĹŮ2”. I do not understand your explanation. The velocity profile at z/H = 0.5 is affected by the divergence of the stresses, rather than the stress itself. How could a large stress value at z/H = 0.5 explain the overestimation of the velocity?

40. Page 10, Line 1, “Figs. 8(b-d) and (f)” rather than “Fig. 8(b-d)”.

41. Page 10, Line 3, what is the “LE ratio”? did you mean your “LS 1.0” case?

42. Page 10, Line 3, why is “LE ratio equal to one” the “accurate ones”? First of all, please fix your grammar. Second, what do you mean by “accurate”?

43. Page 10, Line 5, if all you have to say about Fig. 9 is that it “confirms the findings suggested from Fig. 8”, I would suggest you remove that figure.

44. Page 10, Line 9-10, “There is no significant change of the spectra”, depends on what you mean by significant. The differences among these LS cases are similar to those presented in Fig. 6. I would suggest you plot your data on kE – log(k) plots. First, this avoids the flat 1D spectra issue at the low wavenumbers. Second, it would be much easier to tell the differences if the y-axis is not on a log scale. 45. Page 10, Line 12, “idealised WRF-LES (v3.6.1) models”, model not models

46. Page 11, Line 11, “The spectrum of these data shows an inertial subrange”. I strongly recommend you show these in your spectra plots.

47. Page 11, Line 12, “yields a satisfactory accuracy”. Please, fix your grammar.

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2019-165/gmd-2019-165-RC2-supplement.pdf

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