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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 #3

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In the manuscript "Implementation of a synthetic inflow turbulence generator in idealised WRF v3.6.1 large eddy simulations under neutral atmospheric conditions" present a methodology for generation of inflow turbulence for large eddy simulations using the Weather Research and Forecasting (WRF) model.

General Remarks

The manuscript attempts to address a timely and relevant problem of inflow turbulence generation in large-eddy simulations of realistic atmospheric boundary layer flows. While there is nothing fundamentally wrong with the methodology applied the manuscript has a number of significant deficiencies. The review of previous work in the

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field is inadequate. The authors make several references to derived work instead of citing the original work (more details are provided under “Specific Remarks”.) Only neutral boundary layer simulations are carried out and the Coriolis force was not activated. Such setup does not produce a realistic atmospheric boundary layer. Furthermore, the synthetic turbulence generation approach of Xie and Castro (2008) was already implemented in WRF by Muñoz-Esparza et al. (2015), so it is not clear what is the original contribution of this work. Finally, some of the conclusions about the effectiveness of the synthetic turbulence generation approach are not supported by the results presented in the manuscript. In particular, the length of the fetch needed to achieve the equilibrium boundary layer is underestimated.

Taking all the above into account I do not recommend the manuscript for publication in the journal Goescientific Model Development.

Specific Remarks

Page 2, line 2 – The reference to Nottrott et al. is not appropriate, since Nottrott et al. did not develop WRF. Proper reference would be Skamarock and Klemp (JCP 2008).

Page 2, line 7 – Doubrawa et al. 2018 is certainly not the first or most important reference related to WRF-LES.

Page 2, line 11 – This is not an example of a fundamental study. Nunalee et al. (2014) reported on LES using WRF model based on a tracer dispersion field study and compared simulation results to field study observations.

Page 2, line 14 – Munoz-Esparza et al. (PoF 2015) have already implemented synthetic turbulence inflow scheme by Xie and Castro (2008), so it is not clear what is the original contribution of this work.

Page 2, line 20 – A space is missing between year and semicolon, here, and on numerous places throughout the manuscript.

Page 2, line 26 – However, the velocity profile could be modified, also it can vary in

time.

Page 3, line 16 – More recent reference that expands and improves on Muñoz-Esparza et al. (2015) is Muñoz-Esparza and Kosovic (2018).

Page 4, line 4 – A subgrid scale scheme does not parameterize small unresolved eddies, instead it parameterizes the effect of small unresolved eddies on the resolved field.

Page 5, Equation 9, 12, etc. – The notation using plus sign is confusing since subscript m indicates the velocity component.

Page 6, line 15 – Why is Coriolis turned off if simulation of flow in an atmospheric boundary layer is the goal? Page 7, line 6 – Doubling the computational time is a significant increase that needs to be justified.

Page 7, line 7 – The adjustment distance should be more precisely quantified.

Page 8, line 2 – Instead of symbols, the stresses should be defined as: "horizontal profiles of normal and shear turbulent stresses normalized by surface friction velocity."

Page 8, line 7 – Normalized streamwise variance matches well at x/H = 7 or 8 and not at x/H = 5.

Page 8, line 10 – Below z/H = 0.3 the profile of cross-stream variance differs significantly for any x/H.

Page 8, line 12 – The development is not achieved at all, since only at the end of the domain the values of <w'2>/u*2 obtained using the synthetic turbulence generation method are the same as those from the simulation involving periodic domain. Also, what is shown in the figures is the fetch, not the time scale.

Page 8, line 13 – Figures show that the fetch needed for different quantities to reach the equilibrium values differs significantly between them. For example, vertical velocity variance does not reach equilibrium. Since it is a component of TKE, TKE also requires

a long fetch to reach the equilibrium.

Page 8, line 18 – Same as above, these should be labeled as normal and shear turbulent stresses normalized by surface friction velocity.

Page 8, line 22 – A sentence should not begin with a symbol.

Page 8, line 23 – In “matches closely to that. . .,” "to" should be omitted.

Page 8, line 24 – Same as above, instead of symbols names of the terms should be used.

Page 9, line 9 - The spectral roll-off depends on the numerics not on the turbulence generation scheme, so this is questionable conclusion. Also, flat spectra over a decade of wave numbers is not realistic. Furthermore, there is not apparent inertial range (-5/3) slope in the results presented in Figure 6.

Page 9, line 14 – If current work does not differ from Munoz-Esparaza et al. (2015), what is new in the present manuscript?

Page 9, line 24 – Instead of “slightly affects,” it should be "affects slightly."

Page 9, line 30 – As before, words should be used instead of symbols.

Page 10, line 3 – It is not clear what is meant by " ’accurate’ ones..."

Page 10, line 16 – It is not clear what is the purpose of the statement starting with “It is not trivial. . .” This statement by itself is of little relevance, the question is: What is the relevance?

Page 10, line 21 – The adjustment fetch should be quantified. It is not short.

Page 11, line 12 – The statement related to “. . .a satisfactory accuracy” is an imprecise qualitative statement. It should be stated what is the accuracy satisfactory in comparison to.

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