Responses to the comments from Anonymous Referee #2

General comments

Comment G1: *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.

Response: We appreciate the reviewer's comments on the challenges on implementing an inflow synesthetic turbulence generator in the WRF-LES model. The inflow method was originally developed for engineering applications, and has not been rigorously tested in full-scale atmospheric boundary layer problems. This study extended a well-tested synthetic turbulence inflow scheme (Xie and Castro 2008) into the WRF-LES model. This implementation can be applied to the WRF-LES simulation with a multi-scale seamless nesting case from a meso-scale domain with a km-resolution (where the time-averaged information is known, which can be used as the inputs for the synthetic inflow turbulence generator) down to LES domains with metre resolutions (with additional turbulent information).

Comment G2: *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.*

Response: We implemented a synthetic turbulence inflow generator (Xie and Castro 2008), which has been implemented and tested on engineering type of codes, such as Star-CD (Xie and Castro, 2009) and OpenFOAM (Kim and Xie, 2016) and the micro-scale meteorology code PALM (PALM, 2017; Maronga et al., 2019), into the WRF-LES model. The focus of this paper is to rigorously test and explore the Xie & Castro (2008) method in a full scale (i.e. very large Re number), in terms of the sensitivity of integral length scales and the adjustment distance of the mean velocity field, the turbulent Reynolds stresses, TKE and the local friction velocity. Our paper will be useful to the users of the Xie & Castro (2008) method implemented in meso-scale models, such as WRF, and the micro-scale meteorology models, such as PALM. Our conclusion in the current paper is that the Xie and Castro (2008) method needs 5-15 boundary layer depths to fully develop the turbulence, and this is consistent with those in Xie & Castro (2008), Kim et al (2013) for engineering scale problems. For a coarser grid resolution of 90 m (vs 20 m in our paper), Munoz-Esparza et al. (2014, 2015) tested both their proposed 'cell perturbation method' and the Xie and Castro (2008) method; they concluded that *the cell perturbation* *method needs a fetch of 15-40 boundary layer depths to fully develop the turbulence, while the Xie and Castro (2008) method needs a longer fetch*. A significant improvement of this fetch generated by our code is one of the novelties and, together with the study of the impact of the key variables (i.e. the integral length scales) on the simulated turbulence development represents the scientific novelties of the paper.

In response to the reviewer's suggestion to improve the interpretation of the results, we have conducted more detailed analyses (see those responses to each individual comment below, and the revised figures in the paper).

With regards to the comment on discussions of Fig. 4f, we have reprocessed the model output with much smaller time intervals (5 sec now compared with 60 sec previously). The revised profiles in Figs. 5 and 9 are now much smoother. Our statement in the previous version does not stand anymore. Therefore we have removed those sentences. Subsequently, we think it is not necessary to look into the TKE budget. The modified text (in Section 3.1.3) is as below:

"Since the streamwise velocity variance has a major contribution to TKE, the developing distance for TKE is similar to that for the streamwise velocity variance, i.e. about $x/H = 7$-8".

Comment G3: *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.*

Response: We checked our English writing, proofed read the revised manuscript carefully, and also invited a native speaker to proofread the manuscript before re-submission.

Major comments

Comment M1: *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.*

Response: We thank the reviewer for this comment. We have re-postprocessed the model output. In particular, we have now used a much big dataset to generate better statistical results and velocity spectra. These have largely helped us to make more solid conclusions rather than speculations.

In response to the reviewer's comment (also please see our reply to General Comment G2), we have conducted the following extra analyses and added interpretations of the results. Correspondingly, the following text (on Paragraph 2 of Section 2.3) has been added or modified in the manuscript:

"The further 1 h outputs with 5 second interval (~ the advection timescale of the smallest resolved eddies, which is equivalently twice the grid resolution of 20 m) were used for the analysis. In this study, by taking advantage of the homogeneous turbulence in the spanwise direction (Ghannam et al., 2015), we calculate all resolved-scale turbulent quantities by averaging in the spanwise (the *y*-direction) direction and in time *t* over the last 1 h period. This averaging is referred to as "the *y*-*t* averaging" hereafter, and is denoted by $\langle\varphi\rangle$, for example, for the *y*-*t* averaged φ. For a 4D variable, $\varphi(t, x, y, z)$, the *y*-*t* averaged φ is a function of x, z, i.e. $\langle\varphi\rangle(x, z)$; for a variable defined on the *x*-*y* plane, e.g. friction velocity $u_*(t, x, y)$, the *y*-*t* averaging u_* is a function of *x*, i.e. $\langle u_*\rangle(x)$."

In this way, a better representation of resolved turbulent statistics is achieved. The various curves in the plots are smoother for clearer interpretations. The spectra cover the information of a wider range of eddy sizes.

We have modified and added the following for the explanation of the new spectrum plots (Section 3.1.5):

"For each x-location, e.g. $x/H = 10$, the spectrum for the inflow case was firstly calculated from the streamwise wind velocity component over a time series of 3600 s with an interval of 5 s for five selected sample locations of y_n ($y/H = 1.76, 2.16, 2.56, 2.96\ and\ 3.36$), namely, $\tilde{u}(t, 2H, y_n, 0.5H)$. The spectral data were then averaged over y_n to give the spectra plotted in Fig. 6. "

Another new case with mean inflow only containing no inlet velocity perturbations has been conducted. The horizontal slice of instantaneous streamwise velocity component had been added into Fig 2 as a comparison, in order to provide a better understanding of the advantage of this synthetic turbulence generator.

The spatially and temporally averaged vertical profiles of the mean velocity and the Reynolds stresses, and spectrum for *x/H*=0 for the inflow cases have now been added in the corresponding figures. These provide a better understanding of the direct output from the inflow turbulence generator.

More discussion for the interpreting the results are added, see the following responses.

Comment M2: *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.*

Response: We have run one further case with mean inflow only containing no inlet velocity perturbations. The horizontal slice of instantaneous streamwise velocity components had been added into Fig. 2 as a comparison, in order to provide evidence of the advantage of this synthetic turbulence generator. There is nearly no turbulence generated in the domain even after several hours of simulation (also indicated by the following plot for the vertical profile of TKE - note all of the data, except for the Periodic case, are zero). We have added the following discussions in the revised paper (Paragraph 1 in Section 3.1.1):

"For the inflow case without inlet velocity perturbations, there is nearly no turbulence generated in the domain even after several hours of simulation. This is consistent with other similar tests using engineering CFD codes with no synthetic turbulence added at the inlet, e.g. (Xie and Castro, 2008), which demonstrated that a very long distance (e.g 100 times boundary layer thickness) is needed to allow turbulence to develop. This indicates the importance of imposing synthetic turbulence, or at least some form of random perturbations (e.g. Munoz-Esparza et al., 2015) at the inlet. The inflow case without inlet velocity perturbations is not presented in the later sections. "

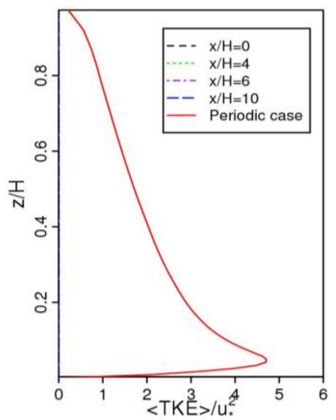

Figure R1: Vertical profile of TKE for the inflow case without inlet velocity perturbations, and for the periodic case.

Comment M3: *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.*

Response: The *x/H*=0 profiles for the inflow cases are now added into Figs 5 and 6. The turbulence statistics derived from the current periodic case are used as the input of the inflow turbulence generator. The following are added:
"It is noted that the profiles of the mean velocity and second order moments at the inlet ($x/H = 0$) are overall in a good agreement with these of the periodic case, which further suggests a satisfactory performance of the turbulence generator." (in Paragraph 1 of Section 3.1.4)

"It is shown that the spectrum at the inlet (*x/H*=0) possesses the most broad range of the -5/3 slope compared to the others. There is an evidence of the tendency in the profiles from the inlet downstream to recover to that of the periodic case. The spectrum drops slightly at high wavenumbers from the imposed spectra at $x/H = 0$ to downwind locations, and to recover towards the spectrum of the periodic case. The slight drop suggests a decay of small eddies due to the SGS and molecular viscosities." (in Paragraph 2 of Section 3.1.5)

Comment M4: *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?*

Response: Xie and Castro (2008) and Kim et al (2013) have already reported more sensitivity studies on the effect of integral length scales, including keeping *Lx* the same and varying *Ly* and *Lz*. They found that a 50% variation in *Ly* and *Lz* generated a variation less than 4% in the friction velocity, and suggested that for the integral lengths not too far from realistic ones, the turbulent statistics are not very sensitive to the length scales.

Again, we emphasise that the aim here is not to generate a particularly accurate simulation of turbulent atmospheric boundary layer flow. Rather, our intention is to assess the adequacy and potential of the inflow generation technique for the prediction of up to second order moments of turbulent statistics.

It is difficult to analyse mathematically the effect of the step change of integral length scales. However, practically we have not noticed an evident issue. These are consistent with Veloudis et al (2007) and Xie and Castro (2008).

Veloudis, I., Yang, Z., McGuirK, J.J., Page, G.J., Spencer, A.: Novel implementation and assessment of a digtial filter based approach for the generation of LES inlet conditions. Flow Turbul. Combust. 79(1), 1–24 (2007)

We have added the modified text on Paragraph 1 of Section 2.3:
"The streamwise length scale (L_x) is specified based on the mean streamwise velocity profile ($\langle u\rangle$) and a constant Lagrangian time scale T (prescribed in Eq. 13), i.e. $L_x = T\langle u\rangle$ using Taylor's hypothesis (turbulence is assumed to be frozen while it is moving downstream with a mean speed of $\langle u\rangle$). The spanwise length scale (L_y) is specified a constant value. The vertical length scale (L_z) is specified a smaller constant value near the bottom and a larger constant value for the upper domain to be closer to the measured length scales, as explained in Xie and Castro (2008) and Veloudis et al. (2007). We conducted a sensitivity study of integral length scales by varying all three baseline $L_x, L_y\ and\ L_z$ with a same ratio of 0.6, 0.8, 1.0, 1.2, or 1.4; these individual cases are denoted by "LS0.6", "LS0.8", "LS1.0", "LS1.2", "LS1.4", respectively, in which "LS1.0" is the base case."

Comment M5: *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.*

Response: For the neutral boundary layer, the results at any altitudes scaled by the boundary layer height could be interpreted for and applied to the cases with other boundary layer heights, e.g. 1000 m.

We has added the following information about the top boundary conditions used in this WRF-LES model, to respond to the reviewer's comment:

"At the top boundary, a rigid lid (top_lid in the namelist.input file of the WRF-LES model) is specified, and a Rayleigh damping layer of 50 m is used to prevent undesirable reflections (Nottrott et al., 2014; Ma and Liu, 2017) and to maintain a neutral atmospheric boundary layer."

Minor comments

Comment: *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.*

Response: This sentence is deleted and has been replaced by a statement attached to the previous sentence: "At the microscale, a large eddy simulation (LES) can be activated in the WRF model (WRF-LES), enabling users to simulate the characteristics of energy-containing eddies in the atmospheric boundary layer."

Comment: *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.

Response: More details are added on Paragraph 1 Section 1:

"There still remains a challenge for downscaling from a mesoscale simulation (resolutions down to 1 km, capturing mean information only) to an LES scale (tens of meters or below, capturing additional turbulence information) (Doubrawa et al., 2018; Talbot et al., 2012; Chu et al., 2014; Liu et al., 2011), e.g. the appropriate inflow conditions for an LES domain, and the sub-grid scale turbulence schemes suitable for appropriate treatment of the "gray-zone" resolution domain where neither planetary boundary layer (PBL) nor LES parametrisation schemes apply well."

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

Response: "uses" is replaced with "use".

Comment: *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.*

Response: This sentence is improved as follows:

"However, implicit in the use of periodic boundary conditions is the assumption that atmospheric fields and the underlying landuse have repeated periodic features. This assumption may be unrealistic for real landscapes where landuse patterns - and the atmospheric phenomena coupled to them - can be very heterogeneous."

Comment: *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.*

Response: As mentioned in a response above, there are two key challenges: appropriate sub-scale turbulence schemes and suitable inflow conditions. In this study, we are focusing on the latter, as one step moving forward. Without the synthetic turbulence inflow scheme, it would take a large distance in the LES domain for the simulated turbulent fields to fully develop. The modified text is:

"Here we implement a well-tested synthetic turbulence inflow scheme (Xie and Castro 2008) in the WRF-LES model (v.3.6.1), in which the meso-scale model could provide the mean flow information as the input of the synthetic turbulence inflow scheme. This scheme provides a step towards enabling WRF's capability of nesting micro-scale turbulent flows within realistic meso-scale meteorological fields."

Comment: *6. Page 2, Line 18 "turbulence" not "turbulences".*

Response: This is corrected.

Comment: *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".*

Response: We thank the reviewer for pointing this out. Figure 7 in Munoz-Esparza et al. (2014) is just a contour plot without any quantitative information. In their later paper using the Cell Perturbation Method (CPM) for neutral boundary layer, Mazzaro et al., 2019 concludes that "*while the CPM significantly reduced the effect of these high-TKE regions, with a shorter fetch of 15–20 km*" (See their conclusion), which is expected to be consistent with the Fig. 7 in Munoz-Esparza et al. (2014). The neutral boundary layer height used in their papers is 500 m, and a fetch of 15–20 km is equivalent to 30 –40 boundary layer depths. Also, in another paper Munoz-Esparza et al. (2015), Fig. 10 shows a quantitative profile of Reynolds-shear stress and the resolved TKE for the development distance, in which a fetch of 15-40 boundary layer depths is mentioned for the turbulence development, while 15 boundary layer depths can achieve values within 10% of the quasi-equilibrium solution for cell perturbation method.

It is to be noted that in Munoz-Esparza et al. (2014, 2015) and (Mazzaro et al., 2019), the inflow forcing is implemented at the west and south boundaries (i.e. both *x*- and *y*-directions), while we implemented the inflow turbulence generation at the x=0 boundary only. Again, we agree with the authors that the

Cell Perturbation Method (CPM) provides an alternative way of turbulence generation in the modelling of atmospheric boundary layer.

We have added the reference and modified this sentence (on Paragraph 3 of Section 1):

"It is thus not surprising that a large distance of about 20-40 boundary layer depths (Munoz-Esparza et al., 2015; Mazzaro et al., 2019) is normally required to allow a transition to fully developed turbulence."

Comment: *8. Page 3, Line 25, "flows" not "follows".*

Response: "follows" is replaced with "flows".

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

Response: "large energy-taking resolved eddies" is replaced with "large energy-containing eddies at the resolved scale".

Comment: *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.*

Response: The WRF-LES solves the fully compressible equations in the flux form which implies an application of the Favre filter, formulated using a terrain-following hydrostatic-pressure vertical coordinate. For an LES domain with flat terrain, the momentum equations can be presented by Equation (2). With an assumption of incompressibility of the atmospheric boundary layer, the continuity equation can be expressed as Equation (1). Being rigorous, we change Equation (1) to the original format by removing the assumption of incompressibility. These are also adopted by other WRF-LES studies (Nottrott et al., 2014; Munoz-Esparza et al., 2015).

Comment: *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.*

Response: In response to the comment, we have added the buoyancy term in the equation. Since this study is focused on the inflow turbulence generator using WRF-LES in which the subscale TKE equation is coded based on parameterised terms, we consider it appropriate to present the equation in the parameterised forms.

We have added "dissipation coefficient (for more details about the parameterisation see Moeng et al. (2007))."

Comment: *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?*

Response: To avoid any misunderstanding, α_{ij} is changed to $[\alpha_{i\beta}]$ to represent the matrix form, while $\alpha_{i\beta}$ in the matrix represents an element of the matrix, following the notations of Equation (18) in Xie and Castro (2008). The calculations of $\alpha_{i\beta}$ follow an iterative order: α_{11}, α_{21}, α_{22}, α_{31}, α_{32}, and α_{33}. This has been added in the manuscript.

Comment: *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?*

Response: This is explained in more details on Paragraph 1 of Section 2.3:

"The streamwise length scale (L_x) is specified based on the mean streamwise velocity profile ($\langle u \rangle$) and a constant Lagrangian time scale T (prescribed in Eq. 13), i.e. $L_x = T\langle u \rangle$ using Taylor's hypothesis (turbulence is assumed to be frozen while it is moving downstream with a mean speed of $\langle u \rangle$)."

Comment: *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?*

Response: These have been removed as they are not very relevant to this paper. The modified text is:

"The vertical length scale (L_z) is specified a smaller constant value near the bottom and a larger constant value for the upper domain to be closer to the measured length scales, as explained in Xie and Castro (2008) and Veloudis et al. (2007)."

Comment: *15. Page 6, ": : :,explained in Xie and Castro (2008)". Please fix your grammar.*

Response: "explained in Xie and Castro (2008)" is replaced with "as explained in Xie and Castro (2008) and Veloudis et al. (2007)".

Comment: *16. Page 6, Line 14-15, "the vertically same wind direction", please fix your grammar.*

Response: "the vertically same wind direction" is replaced with "the constant wind direction vertically".

Comment: *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.*

Response: "in the lateral direction" is replaced with "in the spanwise direction". This is also corrected in elsewhere of the manuscript.

Comment: *18. Fig. 1, caption, use "relative computation time" as in your main text, rather than "relative computation".*

Response: This is removed.

Comment: *19. Fig. 1, "dashed grey line of 1.0 indicating", indicates, not indicating.*

Response: This is removed.

Comment: *20. Page 7, Line 9, "filtered velocity" rather than "filter velocity".*

Response: "filter velocity" is replaced with "filtered velocity".

Comment: *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.*

Response: "Fig." is replaced with "Figure" all over the manuscript now, if it is at the beginning of a sentence.

Comment: *22. Fig. 2. Caption, "(b) The : : :" change to "(b) the: : :"*

Response: "The" is replaced with "the".

Comment: *23. Page 7, Line 16, "are advected and decay downwind: : :", please fix your grammar.*

Response: "are advected and decay downwind: : :" is replaced with "are advected in the domain: : :".

Comment: *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.*

Response: We have rephrased it to

"This suggests that the synthetic inflow turbulence generator can generate realistic well-configured turbulence structures from an adjustment distance downwind of about $x/H = 5\text{-}10$"

Comment: *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.*

Response: This sentence is removed now.

Comment: *26. Page 7, Line 28, "plan" or "plane"?*

Response: "plan" is replaced with "plane".

Comment: *27. Fig. 3, I suggest using the "global friction velocity u_* " 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.*

Response: Now the friction velocity for the inflow case in Fig 3 is scaled by the "global friction velocity" from the periodic case. The relevant modified text is as below:

"The variation of the local friction velocity is within ±0.5% u_* along the streamwise direction for the periodic case and is slightly higher (within 1.5% u_*) than that for the inflow case after a downwind distance of x/H =7."

Comment: *28. Fig. 3. caption "(laterally and temporally)", laterally and temporally averaged?*

Response: "laterally and temporally" is replaced with "the y-t averaged".

Comment: *29. Page 8, Line 10, "a good agreement against?" Please improve this sentence.*

Response: This is modified as follows:

"The horizontal profiles of normalised cross-stream velocity variance ($\langle v'^2 \rangle / u_*^2$) for the inflow case are in a good agreement after a developing distance of $x/H = 10\text{-}12$, compared with these for the periodic case."

Comment: *30. Page 8, Line 12, can you comment on the possible reason for the slow convergence (long fetch distance) of w^2 at 0.1 z/H ?*

Response: This comment was for the figure in the first version. In the current version, as the profiles are smoother, we noticed that the difference is not evident. Therefore, we have revised this in the paper (Paragraph 1 of Section 3.1.3)

"The development of normalised vertical velocity variance ($\langle w'^2 \rangle / u_*^2$) is achieved after a developing distance of about $x/H = 5\text{-}10$."

Comment: *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.*

Response: See the above responses, e.g. the reply to Comment 30. This sentence is removed. The modified relevant text is:

"Since the streamwise velocity variance has a major contribution to TKE, the developing distance for TKE is similar to that for the streamwise velocity variance, i.e. about $x/H = 7\text{-}8$."

Comment: *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.*

Response: See the responses for Comment G2 regarding this comment.

Comment: *33. Page 8, "The red circle dots", just "red circles" will do.*

Response: "The red circle dots" is replaced with "red line", to be consistent with new plots.

Comment: *34. Page 8, Line 21, "noticed again" or "noted again"?*

Response: "noticed again" is replaced with "noted again".

Comment: *35. Fig. 6, caption "$< u >$ and $< u' >$ the laterally averaged mean and streamwise normal Reynolds stress", how are these Reynolds stresses? These are first-order moments.*

Response: $< u' >$ is replaced with $< u'^2 >$.

Comment: *36. Page 9, Line 17, "is able to sustained", please fix your grammar.*

Response: "is able to sustained" is replaced with "is able to be mostly sustained".

Comment: *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?*

Response: The spectrum at the inlet $x = 0$ is added and the inertial subrange of -5/3 slope is shown in Fig. 6. The relevant modified text is:

"The spectrum drops slightly at high wavenumbers from the imposed spectra at $x/H = 0$ to downwind locations, and to recover towards the spectrum of the periodic case. The slight drop suggests a decay of small eddies due to the SGS and molecular viscosities"

Comment: *38. "A length scale (LS) ratio : : : are tested." Please fix your grammar.*

Response: "A length scale (LS) ratio : : : are tested." is replaced with "Length scale (LS) ratios : : : are tested."

Comment: 39. *Page 9, bottom line "Fig. 8 (a) shows that $< u >/u_*$ is slightly greater for the LS ratio less 1.0 (see Fig. 8a for comparison). This is due to a greater Reynolds shear stress $< u'w' >/u_*^2$. 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?*

Response: We are sorry that this was confusing. This has been revised to

"Figure 8 (a) shows that $\langle u \rangle/u_*$ is slightly greater for the length scale ratio less than 1.0. This is likely due to a slightly smaller u_*, which is common for smaller integral length scale cases (as shown in Fig. 7)."

Comment: *40. Page 10, Line 1, "Figs. 8(b-d) and (f)" rather than "Fig. 8(b-d)".*

Response: "Fig. 8(b-d) and (f)" is replaced with "Figures 8(b-d) and (f)".

Comment: *41. Page 10, Line 3, what is the "LE ratio"? did you mean your "LS 1.0" case?*

Response: Yes, it is fixed. "LE ratio equal to one" is replaced with "the LS 1.0 case".

Comment: *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"?*

Response: "the 'accurate' ones" is replaced with "the 'accurate' (compared with the periodic case) one". The 'accurate' is for the comparison to the periodic case.

Comment: *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.*

Response: More discussion about Figure 9 is added on Paragraph 3 of Section 3.2:

"For $x/H = 10$, both mean and turbulent quantities converge approximately to the periodic case. In general, there are slight differences in $\langle u \rangle / u_*$ between each case. The magnitudes of turbulent quantities for smaller integral length scales are slightly smaller than those for larger integral length scales. This suggests that the mean velocity and the turbulent Reynolds stresses are not very sensitive to the integral length scales if they are not too different from the realistic values."

Comment: *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.*

Response: Please see our reply to Comment M1. At x/H=10, all cases varying integral length scales generally converge to the periodic case with slight changes of the spectrum for small wavenumber turbulence. In the text, "no significant change" has been modified as "slight changes" for the new spectrum. There is no issue of flat spectra at the low wavenumbers for the new plots. In this paper, we present the spectrum plots with the inertial subrange of -5/3 slope (indicated in new plots), consistent with those in Xie and Castro (2008). The relevant text is modified (Paragraph 4 of Section 3.2)

"For all cases in the current study, the spectra with various integral length scales generally match those of the periodic case at a developing distance of $x/H = 10$, albeit with slight changes of the spectrum for small wavenumber turbulence. A very small variation of the spectra is within the uncertainty of the calculation of spectrum from the raw data. The spectra show an inertial subrange of -5/3 slope, which are consistent as those in the references, such as Xie and Castro (2008). "

"The spectrum in Munoz-Esparza et al. (2015) drops steeper at high wavenumbers, mainly due to a coarser resolution (noticing that their plots were for kE_{u_i} with the inertial subrange of -2/3 slope). Our spectrum for E_u has a broad range of the inertial subrange of -5/3 slope, indicated in Fig. 6."

Comment: *45. Page 10, Line 12, "idealised WRF-LES (v3.6.1) models", model not models*

Response: "idealised WRF-LES (v3.6.1) models" is replaced with "an idealised WRF-LES (v3.6.1) model".

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

Response: The inertial subrange is now shown in the new spectrum plots Figs. 6 and 10.

Comment: *47. Page 11, Line 12, "yields a satisfactory accuracy". Please, fix your grammar.*

Response: We have improved this statement, i.e.

"These tests on WRF also confirm that this method yields a satisfactory accuracy, after having compared *the local friction velocity, the mean velocity, the Reynolds stresses and the turbulence spectra* against the reference data."