Responses to the comments from Anonymous Referee #3

General Remarks

Comment G1: *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 field is inadequate. The authors make several references to derived work instead of citing the original work (more details are provided under "Specific Remarks".)*

Response: We thank the reviewer for the critical comments, of which many are constructive.

This study attempts to equip WRF-LES with a well-tested synthetic turbulence inflow method (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). This study can potentially provide a tool to bridge in WRF from mesoscale simulation (down to 1 km resolution) to the micro-scale Large-Eddy Simulations (tens of meters or less resolution) with additional turbulence information at small scales. In particular, we have highlighted the novelties in the revised paper, and have improved the review and citation of previous work in the introduction. More are detailed in the responses to "Specific Remarks".

Comment G2: *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.*

Response: This study is focused on the feasibility of the inflow generation subroutine on WRF-LES using a periodic run as a control case. It is particularly focused on the sensitivity of the integral length scales on the turbulence development in the full-scale modelling of WRF under neutral atmospheric conditions. Turning off the Coriolis force is to achieve a constant wind direction vertically, enabling an easier interpretation of the impact of the integral length scales on the simulated flows. This kind of configuration (ignoring Coriolis force) has been used in previous work. A WRF-LES study by Ma and Liu (2017) removed the Coriolis force and used pressure gradient as the driving force to achieve a constant wind direction vertically for a simulation over a hill. Testing the Xie and Castro (2008) method for other conditions, such as considering the Coriolis effect, is out of the scope of this paper and can be the future work. Users of our open source subroutine may extend the code for their own applications.

Comment G3: *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.*

Response: Munoz-Esparza et al. (PoF 2015) focused on their own developed and preferred method - the cell perturbation method, but not on the Xie and Castro (2008) method. To our best knowledge, their code of the Xie and Castro (2008) method has not been contributed as an open source. We made our inflow code (Xie and Castro, 2008) publicly available in this open source journal, i.e. Goescientific Model Development, which is one of the contributions to the community.

In addition, their numeric tests (Muñoz-Esparza et al., 2015) are based on the grid resolution of 90 m. The size of smallest eddy that can be resolved by the LES model is 180 m (i.e. twice the grid resolution). Given a boundary layer height of 500 m in their settings, there are just a small number of eddies resolved (considering the turbulence is anisotropic) in the vertical direction by their model. Our tests here adopt the grid resolution of 20 m. Munoz-Esparza et al. (2015) 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 more fetch*. However, our conclusion in the current paper is that the Xie and Castro (2008) method only 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. This is obviously a new finding derived from a better configured model for the simulations of the full-scale atmospheric boundary layer than that in Muñoz-Esparza et al. (2015), although both use the Xie and Castro (2008) method implemented in WRF-LES.

Xie & Castro (2008) has been implemented in engineering type codes and is successful for wind-tunnel scale (ie. O(1m)) problems, but have not yet been tested rigorously in a meso-scale meteorological model. The focus of our current paper is to rigorously test and explore the Xie & Castro (2008) method in a full scale (i.g. very large Re number) problem, in terms of the sensitivity of integral length scales and the adjustment distance of the mean velocity field, the turbulent Reynolds stresses and the local friction velocity. Our paper will be extremely useful to the users of the Xie & Castro (2008) method in meso-scale meteorological models, such as WRF, and the micro-scale meteorology models, such as PALM. These are the novelties of the paper.

This work bridges the gap, such as in terms of the resolution (we use higher resolution than Munoz-Esparza et al (2014, 2015)), the sensitivity of the turbulent statistics due to the change of integral length scales, for a systematic study of the properties of the method in the WRF-LES model. Of course, we are not able to address everything in this aspect. We also do agree with the authors that Munoz-Esparza et al (2014, 2015)) provide an alternative for turbulence generation for such applications.

Comment G4: *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.*

Response: In response to the reviewer's comment, we have conducted more detailed analyses, including re-postprocessinmg more model output (i.e. using the higher-frequency output - every 5 sec in contrast to every 1 min in the previous analysis) to generate better turbulence statistics and spectra (see the Figs. 5, 6, 9, and 10, for example). These have largely helped us to make more solid conclusions on the effectiveness of the synthetic turbulence generation approach. See more specific replies to the specific remarks.

The length of the fetch needed to achieve the equilibrium boundary layer has been carefully assessed for the turbulent Reynolds stresses, TKE and the local friction velocity. 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. (2015) 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 more fetch". We speculate it is mainly because Munoz-Esparza et al. (2015) used a much coarser mesh in their tests.

We have added/modified the following related text:

"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. The distance needed for different quantities to reach a converged state differs from each other, and it is about $x/H =$ 5-15."

Comment G5: *Taking all the above into account I do not recommend the manuscript for publication in the journal Goescientific Model Development.*

Response: Anyway, we have taken the reviewer's critical (including many constructive) points. We would like to cite here some points from the other two reviewers:

"this work deserves to be published since it involves a rather systematic study of the properties of the method in the WRF-LES model. Especially, the sensitivity study to the integral length scale provides some new and very likely useful information."

"The gray zone between the scales resolvable by the meso-scale models and the resolution requirements of LES unavoidably lead to a large gap in the resolution and therefore it becomes very important to somehow incorporate the lacking turbulence information on the inflow boundaries of the LES-domain in some more or less approximative manner."

"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."

We have carefully addressed the major concerns raised by the reviewer, and also improved the manuscript by addressing the specific remarks suggested by the reviewer as below.

Specific Remarks

Comment: *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).*

Response: We thank the reviewer for the suggestion. This reference Skamarock and Klemp (JCP 2008) was cited when the WRF model was mentioned in our previous version (i.e. in the sentence before Page 2, line 2). Here, the reference to Nottrott et al. is now replaced with "(Skamarock and Klemp, 2008)".

Comment: *Page 2, line 7 – Doubrawa et al. 2018 is certainly not the first or most important*

reference related to WRF-LES.

Response: Doubrawa et al. 2018 is a study on the downscaling from mesoscale simulation to the LES, i.e. linked to the terra incognita range of grid resolutions. More related references are added in the revised paper, i.e. "Doubrawa et al., 2018; Talbot et al., 2012; Chu et al., 2014; Liu et al., 2011".

Comment: *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.*

Response: We thank the reviewer for the comment. In response to the comment, we've added the first study of testing nested LES in WRF by Moeng et al. (2007) and other relevant studies in the revised paper. The word of "fundamental" is removed and more details about studies (with some references) are added as follows, “Therefore such periodic WRF-LES simulations are restricted to *studies* of the atmospheric boundary layer flow *with a single domain (e.g. Zhu et al., 2016; Kirkil et al., 2012; Kang and Lenschow, 2014; Ma and Liu, 2017) or the outermost domain for the nested cases (e.g. Moeng et al., 2007; Khani and Porte-Agel, 2017; Nunalee et al., 2014).*”

Nunalee et al. (2014) used periodic conditions for the parent domain in the nested WRF-LES simulations for the tracer dispersion study and also compared meteorological conditions (i.e. hourly mean vertical profiles of wind speed, potential temperature and wind direction in their Fig. 4) with the field measurements. Nunalee et al. (2014) is kept in the revised paper as an example case for the use of periodic conditions for the parent domain in nested WRF-LES cases.

Comment: *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.*

Response: This is also commented in Comment G3. See our responses to Comment G3.

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

Response: This is due to the formatting of Endnote for multiple citations. A space is added between multiple citations and this has been checked throughout the manuscript.

Comment: *Page 2, line 26 – However, the velocity profile could be modified, also it can vary in time.*

Response: These is for the discussion of the library-based method and recycling-rescaling based method, which are normally applicable to the idealised LES simulations of stationary and equilibrium flows. We have added the following here,

“The turbulence profile determined by the geometry of the precursor simulation can be added on the top of any given mean profile, which could be modified and varied in time for more realistic applications.”

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

Response: The more recent reference is added as follows:

“Munoz-Esparza and Kosovic (2018) extended the cell perturbation method of the inflow turbulence generation to non-neutral atmospheric boundary layers.”

Comment: *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.*

Response: This is modified as follows: "which computes large energy-containing eddies at the resolved scale directly and parameterises the effect of small unresolved eddies on the resolved field using subgrid-scale (SGS) turbulence schemes (Moeng et al., 2007)."

Comment: *Page 5, Equation 9, 12, etc. – The notation using plus sign is confusing since subscript*

m indicates the velocity component.

Response: We are sorry that the reviewer was confused here because we used 'm' to index two different quantities by mistake. We have now corrected it. In the revised manuscript, we've added "m, the index that the averaging operator is applied, denotes the m-th element of a vector (one-dimensional data series of, for example, the digital-filtered velocity, u, in (9) below), k is the number of elements for the two-point distance of $k\Delta x$" for explanation when these first appear in Eq. (8). In addition, "m" in Eqs. (12) and (13) is replaced by "β" to indicate velocity components**.** "j" in Eqs. (14) and (15) is also replaced by "β"**.** These notations follow those adopted by Xie & Castro (2008).

Comment: *Page 6, line 15 – Why is Coriolis turned off if simulation of flow in an atmospheric boundary layer is the goal?*

Response: This is also commented in Comment G2. See the responses to Comment G2.

Comment: *Page 7, line 6 – Doubling the computational time is a significant increase that needs to be justified.*

Response: This is due to that "the additional computational time associated with subroutine of the synthetic inflow turbulence generator and data passing, which is not parallelised, while the main code WRF is parallelised".

We emphasize again that this study is focused on the feasibility of implementing the inflow method (Xie & Castro, 2008) in the meso-to-micro-scale meteorological code of WRF and the impact of the key variables (i.e. the integral length scales) on the simulated turbulence development inside the domain. Up to the authors' knowledge, the latter has not been rigorously addressed previously. We appreciate that *the technical parallelisation of the Xie & Castro (2008) method has been done in PALM and some other researchers (e.g. Kim and Xie, 2016) have also made efforts to technically parallelise the Xie & Castro method. These suggest that technically parallelising this method is not an issue. It is our intention that we test the method inside WRF scientifically and rigorously here and publish our open source subroutine through GMD to allow other WRF-LES users to extend technical capabilities of our code, such as parallelisation.* A paragraph in the Discussion and conclusions section is added for the discussion about the parallelisation of the method, i.e.

"This study is focused on the feasibility of implementing the inflow method (Xie & Castro, 2008) in the meso-to-micro-scale meteorological code WRF and the impact of the key variables (i.e. the integral length scales) on the simulated turbulence development inside the domain. This inflow subroutine has previously been implemented in both serial and parallel mode in several codes, including engineering type of codes Star-CD (Xie and Castro, 2009) and OpenFOAM (Kim and Xie, 2016), and the micro-scale meteorology code PALM (PALM, 2017). Although the current implementation in WRF is affordable for a moderate-sized simulation (e.g. tens of meters resolutions), the technical parallelisation of this inflow subroutine in WRF-LES can be the future work for very large simulation domains with high resolutions. Users of our open source subroutine may offer this technical contribution."

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

Response: "about x/H = *5-10*" is added in the revised paper.

Comment: *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."*

Response: Symbols have been removed. The following text is added, i.e.

"horizontal profiles of normalised mean streamwise velocity component, normal and shear turbulent stresses, and TKE".

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

Response: "$x/H = 5$" is replaced with "x/H = *7-8*".

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

Response: It has been 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 $x/H =$ 10-12 , compared with those for the periodic case."

Comment: *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.*

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.

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

The time scale has been changed to "length scale".

Comment: *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.*

Response: See our reply to the Comment G4. We have regenerated and re-examined these plots carefully. Based on these smoother profiles, we are able to reach more solid conclusions. These plots suggest that the *fetch needed for different quantities to reach the equilibrium values differs only slightly between them, considering a small uncertainties (errors) due to the limited averaging time.*

The fetch needed for different quantities is also discussed in the responses to Comment G4.

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

Response: Symbols have been removed. The following text is added, i.e.

“normalised mean streamwise velocity component, normal and shear turbulent stresses, and TKE”.

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

Response: **“**The normalised mean streamwise velocity component” is added before the symbol.

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

Response: “to” is removed.

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

Response: This is modified as “The normalised streamwise velocity variance”.

Comment: *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.*

Response: In the previous version, we conducted spectral analysis using the spatial data along the cross-stream direction (*y*) with given values of *x* (*x/H*=2, 4, 6 and 10) and *z* (=0.5*H*) and then averaged the spectrum over time to generate the results in Figure 6 in the original manuscript. Now a different method is adopted: for given values of x and z, we conduct spectral analysis using the 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, $\tilde{u}(t, 2H, y_n, 0.5H)$, and then an average over y_n yields the data plotted in Fig. 6 in the revised manuscript. This method is in essence used to analyse experimental time series data from point measurement; when applied to the LES data, it yields a fairly good inertial subrange as shown in the new Figure 6, as well as in new Figure 10.

We have modified and added the following (in 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. ”

“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”

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

Response: This is also raised in Comment G3. See the responses to Comment G3.

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

Response: As suggested, this is now corrected.

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

Response: Symbols have been removed. The following text is added, i.e.

“normalised mean streamwise velocity component, normal and shear turbulent stresses, and TKE”.

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

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: *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?*

Response: This has been modified:

“It is not trivial to estimate the integral length scales as the primary input of the inflow turbulence generator.” is replaced with “It is important to estimate the integral length scales, which are the key inputs of the inflow turbulence generator.”

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

Response: This is now quantified, i.e. “after a short adjustment distance” is replaced with “after an adjustment distance of x/H=5-15”.

Comment: *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.*

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.”