Responses to the comments from Anonymous Referee #1

General comments

Having reviewed the original manuscript, and now after reviewing the revised manuscript, I can recommend its acceptance for publication subject to minor revisions.

Generally, the english language would still need improvements here and there. The following comments must be taken into account and appropriate corrections should be made. In the following PXLY means page X line Y.

Response: We thank the Reviewer for reviewing our manuscripts and having given very positive comments. We have read the manuscript thoroughly and have improved the English with the help of a native speaker. We have highlighted our changes in the revised manuscript. We respond to each of the Reviewer's specific comments below.

Comment: *P1L30: "mesoscale scales" → mesoscales*

Response: This has been corrected.

Comment: *P2L28: "...from a downstream boundary back to the upstream inlet." It makes no sense to recycle the velocity field from the downstream boundary. This would be like in the normal cyclic boundary condition. Normally the data to be recycled is taken from some downstream plane between the inflow boundary and the principal area of interest. So, this could be written e.g. as: "...from some suitably selected downstream plane back to the inflow boundary plane."*

Response: As suggested, this has been replaced with "...from some suitably selected downstream plane back to the inflow boundary plane."

Comment: *P7L21: "wass" (please note that there are typos elsewhere, too)*

Response: This has been corrected. We have read the manuscript thoroughly and have improved the English with the help of a native speaker. We have highlighted our changes in the revised manuscript.

Comment: *P9L4: "shear turbulent stress" → turbulent shear stress*

Response: This has been corrected.

Comment: *P10L6: "...due to the SGS and molecular viscosities." Frankly, the molecular viscosity plays no role at this high Reynolds number and this grid resolution. The SGS viscosity certainly exceeds it everywhere by several orders of magnitude. However, the numerical dissipation originating from the fifth-order upwind-biased Wicker-Skamarock advection scheme used in WRF is probably of comparable importance in the spectrum high-end drop as is the SGS-dissipation. This should be mentioned.*

Response: This has been replaced with "due to the SGS viscosities and the numerical dissipation originating from the advection scheme in the WRF-LES model".

Comment: *P11L30-32: some questionable language: "...consistent with in the findings..." and "...may be owe to the...".*

Response: These have been replaced with "agrees with the findings in" and "can be attributed to".

Comment: *P12L27: "...shows a broad inertial subrange of -5/3 slope." In my view the inertial subrange in the spectra shown in Figs. 6 and 10 is not at all broad. Instead, it is just hardly distinguishable. The word broad must be erased here.*

Response: As suggested, the word broad has been erased. This has been replaced with "shows an inertial subrange of -5/3 slope".

Comment: *P25L13-18: Reference Maronga et al 2019 is outdated. That article has been published in Geosci. Model Dev., 13, 1335–1372, 2020 https://doi.org/10.5194/gmd-13-1335-2020. The reference must be updated accordingly.*

Response: This has been updated accordingly.

Responses to the comments from Anonymous Referee #3

Comment: *Review of the revised manuscript GMD-2019-165: "Implementation of a synthetic inflow turbulence generator in idealised WRF v3.6.1 large eddy simulations under neutral atmospheric conditions" by Jian Zhong, Xiaoming Cai[1] and Zheng-Tong Xie submitted for publication in the journal Geoscientific Model Development.*

In the revised manuscript "Implementation of a synthetic inflow turbulence generator in idealised WRF v3.6.1 large eddy simulations under neutral atmospheric conditions" the authors have made relatively minor changes to the exposition of the methodology for generation of inflow turbulence for large-eddy simulations based on synthetic turbulence generation approach developed by Xie and Castro (2008) implemented in the Weather Research and Forecasting model. In addition, the results presented in the revised manuscript are improved in comparison to the original manuscript.

Response: We thank the Reviewer for the positive comments on the improved methodology and the results. Below we respond to all of the Reviewer's comments.

General Remarks

Comment: *While, the revised manuscript includes improved results, the main deficiency of the manuscript remains – the synthetic inflow turbulence generation methodology developed by Xie and Castro (2008) was previously implemented in the Weather Research and Forecasting model by Muñoz-Esparza et al. (2015) and extensively tested. It is therefore not clear what new research is presented in this manuscript. One element that is explored in greater detail is integral length scale, however, the authors do not clearly articulate any potential differences in implementation or results of simulations. Furthermore, the authors claim that the simulations presented in the manuscript are of neutral atmospheric conditions, however, Coriolis force was not used in the simulations and therefore important characteristic of a real atmospheric boundary layer, namely wind veering, could not be reproduced. Finally, although minor changes to the background material and exposition were introduced, some of these include misrepresentation of previous work. Examples are given below under Specific Remarks.*

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

Response: As we emphasized in our previous responses to the Reviewer, here we repeat our points again. Muñoz-Esparza et al. (PoF 2015) focused on their own developed and PREFERED method - the cell perturbation method, but not on the Xie and Castro (2008) method, for the mesoscale to microscale transition. In addition, to the best of our knowledge, their subroutine code with the Xie and Castro (2008) method has not been contributed as an open source, whereas we are making 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.

The tests in Muñoz-Esparza et al. (2015) are based on the horizontal grid resolution of 90 m and the size of smallest eddies resolved by the LES model is about 180 m (i.e. twice the grid resolution). Considering their boundary layer height of $z_i \cong z_{i0} = 500$ m, this horizontal resolution is insufficient to represent the most energetic turbulent eddies (near $z/z_i \sim 0.16$ in their Fig. 7a) in the neutral boundary layer, specifically the vertical component of resolved turbulence. This is illustrated by their spectra plots for $z/z_i \sim 0.1$ of Fig. 9: the w-spectra are about one order of magnitude smaller than the v-spectra, even for the periodic B.C. setting. Our tests here adopt the grid resolution of 20 m, providing a much finer mesh to resolve more eddies for the same boundary height of 500m; for example, our Fig. 5(d) shows that the magnitude of the w-component resolved energy is a large fraction of that of the v-component. Therefore, our results are more reliable on testing Xie and Castro (2008) method in WRF.

Muñoz-Esparza et al. (2015) concluded that *the cell perturbation method needs a fetch of 15 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 (with much higher grid resolution in WRF) is that the Xie and Castro (2008) method in WRF only needs 5-15 boundary layer depths to develop turbulence to the required level, and this is consistent with those findings in Xie & Castro (2008) and Kim et al (2013) for engineering scale problems. This is certainly a novel 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 implemented independently the Xie and Castro (2008) method 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.e. in flow with a very large Re number) problem, in terms of the sensitivity of integral length scales and the adjustment distance of the mean velocity, the turbulent stresses and the local friction velocity. We found and emphasized in the revised manuscript that "*The mean velocity profiles at all tested locations were in very good agreement with the reference data, while the turbulence second moment statistics profiles were in reasonable agreement with the reference data about x/H=5-15 downwind of the inlet. An accurate estimation of the second order moments are crucial for the assessment of the synthetic inflow turbulence generator, in particular when the inflow turbulence information is not completely available. We found varying the integral length scale within +/-40% of the value in the base case has a negligible influence on the mean velocity profiles, while the effects of the variation on the turbulent second order moment statistics are visible, for example the local friction velocity was within 4 % error of the reference data at x/H=7.*" 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. This is another novelty of the paper in terms of the use of turbulence integral length scale.

For the Reviewer's comment on the Coriolis force, again as we emphasized in our previous revision:

Turning off the Coriolis force is to achieve a constant wind direction in the vertical direction, enabling an easier interpretation of the impact of the integral length scales on the simulated flows in this study. Such a configuration (where the Coriolis force is not activated) has been used in previous WRF-LES studies too (e.g. Ma and Liu 2017). *It is worth, however, a future study to examine the wind spiral case induced by the Coriolis force in the atmospheric boundary layer* (This has been added in the Section 4 Discussion and conclusions of the revised manuscript).

Further changes to the discussion of previous work are given in the responses to the Reviewer's specific remarks, below.

Specific Remarks

Comment: *Page 2, line 13 – It is stated that: "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 outmost domain for the nested cases (e.g. Moeng et al., 2007; Khani and Porte-Agel, 2017; Nunalee et al., 2014)." However, the* *simulation listed here are quite different. While Moeng et al. (2007) present simulations included two-way nested domains where periodicity on the outer domain impacts the flow on the inner domain, Nunalee et al. (2014) present a one-way nested simulations where inner domain is not impacted by periodicity on the outer domain, so that such setup can be used for simulation of heterogeneous boundary layers.*

Response: The boundary condition for the outer domain of the nested cases is periodic, while the inner domain can be either one-way nested (e.g. Nunalee et al. 2014) or two-way nested (e.g. Moeng et al. 2007). This has been further clarified in the revised manuscript, as follows:

"… or the outermost domain of either one-way nested cases (e.g. Nunalee et al. 2014) or two-way nested cases (e.g. Moeng et al., 2007)."

Comment: *Page 3, line 21 – It is stated that: "Generally speaking, these methods impose "white-noise" perturbations, thus having a flat spectrum, to a variable (e.g. temperature) at the inlet, and the model dynamics will "process" the signals once these signals are advected into the domain, e.g. to dissipate high-wavenumber signals quickly and to adjust low-wavenumber signals gradually." This statement misrepresents the temperature perturbation methodology. Temperature perturbations are introduced at a specific length and time scale related to the highest well resolved wave number in LES and therefore they cannot be considered "white noise." White noise can be defined as "random signal having equal intensity at different frequencies, giving it a constant power spectral density."*

Response: The "white-noise" has been removed. As suggested, this sentence has been changed as follows:

"These methods impose temperature perturbations at specific length and time scales related to the highest resolved wave number in the LES"

Comment: *Page 3, line 25 – It is stated: "It is thus not surprising that a large distance of about 20-40 boundary-layer depths (Muñoz-Esparza et al., 2015; Mazzaro et al., 2019) is normally required to allow a transition to fully-developed turbulence." However, Muñoz-Esparza et al. (2015) demonstrated that "From those results, it is evident that the performance of the cell perturbation method is not affected by these factors, rather induces turbulent structures that become fully developed at x/zi0 ≈ 15." See also Figure 18 in Muñoz-Esparza et al. 2015.*

Furthermore, the difference between the application of synthetic inflow turbulence generator presented in the manuscript and the temperature perturbation methodology presented in Muñoz-Esparza et al. (2015) is not recognized by the authors. Temperature perturbation is introduced for mesoscale to microscale coupling approach where smooth mesoscale flow (no resolved turbulence) is forcing microscale flow and for that purpose one-way nesting approach is used in WRF. The nesting approach necessarily represents a different challenge for transition to fully-developed turbulence due to nesting compared to just specifying inflow turbulence, e.g., there is significant difference in inflow turbulence levels between the results presented in the manuscript and those in Muñoz-Esparza et al. (2015).

Response: We have changed this in the revised manuscript accordingly:

"It was demonstrated in Muñoz-Esparza et al. (2015) that a distance of about 15 boundary-layer depths is required to allow the flow to be fully turbulent when the temperature perturbation method is adopted in the one-way nesting WRF model. Noted that the temperature perturbation method was introduced for mesoscale to microscale coupling approach where smooth mesoscale flow (no resolved turbulence) forces microscale flow by using the one-way nesting approach in WRF. Muñoz-Esparza et al. (2014)

stated “the perturbation method is to provide a mechanism that accelerates the transition towards turbulence, rather than to impose a developed turbulent field at the inflow planes as the synthetic turbulence generation methods pursue”, and “the use of temperature perturbations presents an alternative to the classical velocity perturbations commonly used by most of the techniques”.”

Comment: *Page 10, line 6 – It is stated: “The spectrum in Muñoz-Esparza et al. (2015) drops steeper at high wave numbers, mainly due to a coarser resolution…” The drop in the spectra is related to the implicit filter associated with the numerical discretization and drops at lower wave numbers due to coarser resolution, but it does not drop steeper. Steepness of the drop is related to the implicit filter. It has been determined that the effective resolution of WRF is ~7 dx (Skamarock 2004).*

Response: Thank the reviewer for clarifying this. We have changed this in the manuscript accordingly:

“The spectra in Muñoz-Esparza et al. (2015) drop at lower wave numbers than those in Fig. 6, mainly due to a coarser resolution (than the current one).”