Responses to the comments from Anonymous Referee #1

General comments

Comment G1: *This manuscript reports the implementation and testing of the existing synthetic inflow turbulence generation method (Xie and Castro, 2008) for large-eddy simulations (LES). The LES model in question is the widely used WRF-LES model for small-scale atmospheric problems. The topic is important since the question of how to deal with the missing inflow turbulence information is one of the most important issues in the context of practical applications of LES to small-scale atmospheric problems as well as to other kind of turbulent-flow problems such as e.g. many engineering related problems. The authors correctly point out the particular need for methods to handle the inflow turbulence question in cases when the LES model is nested within a meso-scale atmospheric model domain. 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.*

The degree of novelty of the present work is not particularly high. This is because the method in question was developed already more than ten years ago, and because the same method has already been implemented in some other LES models such as the PALM model which is also a LES model for small-scale atmospheric problems like WRF-LES. However, in my opinion, 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.

Response: We thank the reviewer for the overall positive comments. To echo the positive feedback from the reviewer, we wish to reiterate that the gray zone issue still remains challenging for sub-kilometre meteorological modelling and there is a great demand for a reliable nesting methodology to enable sub-hundred-metre large-eddy simulations of the atmospheric boundary layer. The WRF model is perhaps the best platform to test such a methodology, whilst PALM has no capability of meso-scale meteorological modelling. As one important step to achieve this target, this study attempts to equip WRF with a well-tested synthetic turbulence inflow scheme (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). We believe that this new capability will benefit many boundary layer modellers to run a LES for their local areas nested online within a meso-scale domain.

The focus of this paper is to rigorously test and explore the Xie & Castro (2008) method in the meso-to-micro-scale meteorological code WRF, 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. These are the novelties of the paper.

Comment G2: *A remarkable weakness of the work is that the synthetic-turbulence generation method has not been parallelised. Instead, the root process performs the whole task of the synthetic turbulence generation and then distributes it to those other processes which need this information. As shown in the manuscript (Fig. 1 b), this severely compromizes the computational speed even in this kind of rather moderate-sized simulation. In really large simulation set ups with thousands or even tens of thousands of CPU-cores employed, the non-parallelised method becomes totally impractical. Therefore the question about the parallelisation must be discussed more deeply in the manuscript. Note that the problem of parallelising this method has already been solved at least in the PALM-implementation.*

Response: 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. 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 that 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 code through GMD to allow other WRF-LES users to extend technical capabilities of our code, such as parallelisation.* In response to the reviewer's comment, the following paragraph has been added in Discussion and conclusions:

"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 G3: *Generally, the manuscript is quite well structured and written, but some improvements are needed, see the specific comments below. In part of the figures, especially in Fig. 4, the legend texts are too small, please enlarge them.*

Response: The specific comments are responded below. The legend texts in Figs 1, 4, 5, 8 and 9 are enlarged.

Specific comments

Comment: *Page 2, line 4: I find the statement: "The WRF-LES model can capture the intermittency*

of three-dimensional turbulent eddies." a bit confusing. This should be clarified.

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: *Page 3, line 16: Just a typo: white noise is typed "while noise".*

Response: This is corrected.

Comment: *Page 3, line 27: Perhaps another reference to PALM could be added here, see*

https://www.geosci-model-dev-discuss.net/gmd-2019-103/ although this is still currently in the discussion phase.

Response: This additional reference for PALM has been added.

Comment: *Page 6, lines 5 and 6: "...dominant Reynolds stress tensors..." does this possibly mean dominant Reynolds stress components, or something else? Please correct.*

Response: “dominant Reynolds stress tensors” is replaced with “Reynolds stress components”.

Comment: *Page 6, lines 14 and 15: "...the vertically same wind direction...". For instance "vertically constant wind direction" would be better wording.*

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

Comment: *Page 7, lines 21 and 22: The last sentence of this paragraph is obvious and could as well be dropped.*

Response: This sentence is removed now.

Comment: *Page 8, lines 8 and 9: "<u’ˆ2>/u_*ˆ2 has a higher value at z/H = 0.1 than that at z/H = 0.5. This is consistent with the trend that it decreases with height in the boundary layer." I find this, too, kind of obvious and unnecessary to mention.*

Response: This is removed now.

Comment: *Page 8, lines 14-16: "The slower adjustment...can be attributed to a larger shear generated TKE..." I don’t really understand the line of thinking here. I think this statement should be better justified and explained.*

Response: This sentence is removed. We have added the following discussion regarding the developing distance for TKE in Section 3.1.3.

“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 $x/H = 7\text{-}8$. The distance needed for different quantities to develop the turbulence differs between each other, and it is about $x/H = 5\text{-}15$.”

Comment: *Page 8, Sec. 3.1.4: The inflow case profiles of the second moments in Fig. 5 (and also to some extent in Fig. 9) appear wavy compared with the periodic case profiles. I assume that it is very clear for a large majority of the readers that this is because these profiles are not averaged in the stream-wise direction like those of the periodic case. However, I think this should be nevertheless explained in the text.*

Response: 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.

Comment: *Page 9, lines 16 and 17: The last sentence of this paragraph appears vague. Please, improve it. One reason for my confusion may be that there is no inertial subrange visible in the spectra shown in Fig. 10, probably because of the rather moderate resolution and/or numerically dissipative advection scheme. Moreover, I think that the term "inertial sublayer" is not good. It is better to say inertial subrange because it is not intuitive (or at least I don't find it intuitive) to think about layers in the wave-number space.*

Response: In the previous version, the spectra were calculated based on the spatially distributed data along the cross-stream direction (*y*) for given (*x, z*) coordinates. These were averaged for a number of time steps to smooth them. The limit of this approach is that a small number of data along the cross-stream direction (*y*) were used.

A slightly different approach is adopted in the revised paper. For given (*x, y, z)* coordinates, a spectrum was calculated based on a time series of 3600 s with an interval of 5 s. Five spectra for ($y/H = 1.76, 2.16, 2.56, 2.96\ and\ 3.36$) at the same (*x, z*) coordinates were averaged to obtain a smoother one plotted in Fig. 6. The same was used for the new Figure 10. In the text, "inertial sublayer" is replaced by "inertial subrange".

We also added some discussion as,

"It is noted that for a very high resolution, e.g. in the order of magnitude 1 meter, similar as that used in the simulations of PALM (PALM, 2017), the inertial subrange in the spectrum is wider."

Comment: *Page 9, line 32: "...less 1.0...", please, add "than".*

Response: "than" is added.

Comment: *Page 10, line 3: "...the 'accurate' ones...". I assume this refers to that in the case LS1.0 the integral length scales are set as evaluated from the periodic case results, but I am not sure if I understood this correctly. This should be written more clearly.*

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

Comment: *Page 10, line 3: I guess LE ratio should be LS ratio.*

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

Comment: *Page 10, line 12, "...WRF-LES (v3.6.1) models...". Why models, i.e. why in pluralis form?*

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

Comment: *Page 11, lines 5-7: I find these last two sentences of this paragraph very unclear. If this is to discuss the (so far) lacking parallelisation of the method, it is not sufficient and not at all clear. As stated above in my general comments this issue must be discussed more deeply. It deserves its own paragraph in the Discussion and conclusions section, but should also be better brought up in Sec. 2.3.*

Response: See the response to Comment G2. We have added a paragraph for the discussion about the parallelisation of the method in the third paragraph of the section of Discussion and conclusions