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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 #1

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General comments

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

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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.

A remarkable weakness of the work is that the synthetic-turbulence generation method has not been parallelized. 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-parallelized method becomes totally impractical. Therefore the question about the parallelization must be discussed more deeply in the manuscript. Note that the problem of parallelizing this method has already been solved at least in the PALM-implementation.

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.

Specific comments

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.

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

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.

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

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

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

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.

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.

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.

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.

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

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.

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

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

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 parallelization 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.

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