Wind Energ. Sci. Discuss., referee comment RC3
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Comment on wes-2021-101

Anonymous Referee #3

Referee comment on "Impact of the wind field at the complex-terrain site Perdigão on the surface pressure fluctuations of a wind turbine" by Florian Wenz et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2021-101-RC3, 2022

The manuscript draft presents a significant amount of work on a topic relevant to the wind energy community. The authors focus on analyzing pressure fluctuations around a wind turbine in three different cases. I believe this is one of the first studies looking into this in so much detail, and this can be relevant information for the community and readers of the journal.

However, the presentation of the main conclusions from this work should be improved and made more precise, and it should be ensured that what is written in the manuscript text and abstract/conclusions is consistent. Now there are problems with this, in part because the English should be improved throughout the manuscript. The interpretation of what the authors want to convey is convoluted due to imprecise (and sometimes incorrect) formulations. For example, in the abstract
- "The conservation of the flow field," --> what does conservation mean here?
- "It is shown that a sophisticated DDES of the complex terrain plays a decisive role in the unsteady aerodynamics of the turbine, due to its specific flow characteristic" --> I am not quite sure what the authors want to precisely convey (state that DDES captures unsteady dynamics better than RANS?) here. Although it should be noted that this is not shown in the present work as the turbine dynamics are only studied in the FLOWer simulations. Also, what is "decisive," i.e., how much is the influence?

Furthermore, it is unclear the accuracy of the obtained results. Significant differences between the field observations and simulations are observed; however, after section 4.1, these are not discussed further. The case under consideration is challenging, so differences with observations by no means disqualify the importance of the study. However, please convey to the reader how they should interpret the "accuracy" of the results in section 4.2. For example, to what degree do the results depend on the numerical resolution, the domain size, and the forest modeling. Especially on the latter, the authors repeatedly say that it is essential, but there is no demonstration of the importance of forest modeling. Were the authors able to perform validations beyond the comparison of the flow fields?

In short, the manuscript should be carefully revised to avoid confusion or misinterpretation of the results. Various specific points are indicated below.

* Line 31: Large-scale meteorological effects are often captured by Large Eddy Simulations (LES) with meteorological codes such as the Weather Research and Forecasting (WRF) model --> This suggests WRF is LES code, which is not the case.
* There are various references to what seem to be "variables" or "settings" within the code (such as RSF=1; HF=0 (around line 129), FSF=10 (Line 150), nutkAtmRoughWallFunction) (lines (153), and various others so please double-check the entire text). In contrast, the user codes are not fully available. Please clarify these accordingly such that the manuscript can be fully understood by readers that do not have access to the text.
* Section 3.2.2. Just above equation (2), it is stated that the LAD is calculated based on the LAI. However, it is not clear how. In fact, in equation (4), the LAI is just defined as the vertically integrated LAD. Please clarify the section correspondingly. Note that also zm is undefined.
* Section 3.2.2. How do you select the different zones in figure 3? (note that 18m is used twice in figure 3; see Table 3).
* In figure 2, you show that the region in front of the turbine is smoothed to ensure "periodicity of the terrain". Is the terrain smoothing not larger than adding the Forrest in that region?
* Section 3.2.2. Is it correct that you only model forest in part of the computational domain? Why was this choice made?
* line 222: Wind shear (u(z)), wind veer (v(z)) and flow inclination (w(z)) --> The definition of these terms is incorrect. Please ensure proper use of the terms in the entire manuscript.
* "laterally averaged E-Wind k values" --> Please define how k is determined in E-wind
* Equation (6) epsilon does not seem to be defined.
* Figure 4: As E-Wind solution is used as inflow for FLOWer simulation. How is it possible that these "inflows" are not the same? In particular, the u-component. Can you explain how this coupling is done?
* Table 4: How is the difference in TI defined? Percentage points or relative difference?
* lined 363, see also line 565: This conclusion is quite strong, considering that a significant difference with field data is observed at various places in the domain of interest. Yes, at mast 20, the agreement is good, but at the other masts, less so. Presumable results are tuned to the mast 20 location in some way.
* Improve figure quality; in particular, make sure the used font size for axis labels and legends is sufficiently large.
* Figure 16: What happens at the inflow boundary at x=-700m? Why is the solution not continous there?
* Towards the end of the manuscript, the text is not divided into proper paragraphs, but paragraphs only have 1 or 2 sentences. The manuscript's structure can be improved by collecting relevant material in relevant paragraphs/sections.
* line 569 "the expansion of the turbine wake could also be compared with lidar measurements and was found to be simulated similarly." Where is this discussed in the manuscript?
* line 577: This contrasts with what you write at 395. Please rephrase.
* The manuscript would benefit from proofreading by someone with native English proficiency.