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Comment on wes-2021-36

Anonymous Referee #2

Referee comment on "Meso- to micro-scale modeling of atmospheric stability effects on wind turbine wake behavior in complex terrain" by Adam S. Wise et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2021-36-RC2, 2021

Reviewer general comment: The current manuscript presents results of a suite of WRF-LES configurations around the complex topography of the Perdigão field site. Results represent the first time that WRF- LES is used to study the flow interaction in complex terrain and wind turbines. Results of the numerical simulations are qualitatively compared to the experimental measurements. According to the authors, the features of interest for the present work are: (a) mountain waves, (b) recirculation zones, and how these interact with a wind turbine and the corresponding wake. The manuscript is very nicely written, clear, and with superb "candy-to-the-eye" type figures. Therefore one could say that the article is excellent (despite a few typos or unclear elements). However, to this reviewer's opinion, while the general idea of the work is interesting and filled with interesting challenges and scientific unknowns, the work presented here remains poor in scientific content, and falls short of addressing any of the initial science-based goals. Let me explain, intercomparisons between numerical models and experimental measurements are critical to be able to objectively determine the quality of the simulation results. However, if the effort stops at that, then it only becomes a mere technical work that anyone outside of the academic world could do. There are indeed publications where industry models are intercompared with experimental data, which have been used in the past to provide a sense of confidence or trust to industry. Alternatively, there are also publications from the academic world, where intercomparisons between models and experimental data are done, but in those there is traditionally a critical analysis of the influence of using different numerical schemes, subgrid models, filtering approaches, etc. So at the end there is an evident scientific gain. Unfortunately in the present work the potential scientific gain remains hard to find. What have we learned out of this manuscript? – That we are now entitled to do WRF-LES simulations in complex terrain with turbines? WRF simulations are done on a daily basis around the world, so how will this change what is done on a daily basis? One could have taken advantage of these great simulations to make a more robust objective/quantitative comparison between the simulations and the experimental data. For example, reading that there are wind differences observed at certain times of 1-2 m/s doesn't really mean much. That could be a 50% difference in a weak mean wind, or a tiny % difference in a strong wind scenario,... Or that the flow looks similar or dissimilar here and there, has little scientific rigor. Once the rigurous/strict comparison of results done, then one could have an additional section with more scientific insight. Research questions that come to mind could be:

1. What is the effect of the surface conditions? – I want to understand that your WRF-LES is using IBM for the topography in regards to the momentum, but is it using the same approach for the ther- mal/moisture field? Based on what I was able to gather from the manuscript it seems that there are no surface conditions for temperature,... does this mean that your flow is insensitive to the surface con- ditions? This is strange since one of the driving mechanisms to thermal stratification is the ground,... but not much is discussed about the impacts or not of the surface conditions on the flow?

2. What is the effect of your turbulence initialization, and is it really worth it to run a suite of mesoscale WRF simulations just to provide time varying boundary conditions? Mesoscale WRF provides an ensemble flow solution that evolves with time, said otherwise, provides a more or less accurate mean flow and thermodynamic conditions of the region. However, the LES simulations provide an instan- taneous realization of the turbulent ABL, the question then could be posed as how does that compare to instead using profiles of experimental data to force the LES? Also, one can only wonder, what is the effect of the cell perturbation method to generate turbulence? Sure enough the mountains will spur some turbulence, but are the incoming turbulent flow conditions representative of all turbulent scales, including large scale perturbations?

3. An alternative, potentially the most interesting research question is how can one use the outcome of the rich simulations to understand flow configurations at other locations? Can results be scaled such that the results become generalizable in terms of stratification, mountain slope, terrain complexity? It would be a lot more interesting if the authors used the rich dataset to come up with generalizable relations that enabled one to extract conclusions at other locations without having to run expensive simulations,...

To this reviewer’s opinion this manuscript provides another missed opportunity by science and the com- munity in general to develop new critical advancements. Instead one could argue that this work is just designed to become another publication like the ones referred in the recently published article in the Atlantic magazine (https://www.theatlantic.com/science/archive/2021/05/xkcd-science-paper-meme-nails-academic- publishing/618810/). It is for all these reasons that at the present time this reviewer can only recommend major revisions, and can not recommend publication of the current manuscript until more scientific value is added to the manuscript.

More Specific Technical Comments

- Line42; when talking about ‘length-scales’ one should clarify that these are "turbulent length-scales"

- Line 53; it is important to clarify the difference in time scales between the LES and the WRF mesoscale simulations. LES provides an instantaneous realization of the flow at high-temporal frequency, while WRF mesoscale provides results representing the outcome of an ensemble of flow marching in time, but that per the ergodicity definition can not be interpreted at the same frequency than otherwise the LES inout/output.

- In line 57; can the authors provide a brief description of the GAD model? Meaning, I don’t need to read the reference to find out whether the models is an actuator disk with/out rotation, etc.

- In line 82; the authors mention that “the goal of this work is to model realistic atmospheric conditions and the associated turbulent flow phenomena to better understand wind turbine wake propagation in complex terrain”. At the end of the manuscript, what have we learned about this that maybe of use as a function of thermal stratification, or in other locations, mountains, terrain, etc?

- By the end of section 2.1; what about the surface conditions? If they were not measured (besides roughness) how are they taken care of in the simulations? Is z0,t taken as a fraction of z0?

- In Figure 2; I could only wonder but what happens to the near surface heat and momentum fluxes? – It is well known in the community the existence of counter gradient heat fluxes, specially in complex terrain. Why not include subpanels with the near surface heat fluxes?

- Around line 185; It seems like simulations are initialized with a uniform heat flux. However, it is known also that the heat flux will be rarely uniform in complex terrain. How is that potential effect assessed? Are we left to interpret that the results are indifferent to that surface forcing?

- First line in Section 3.3; “Having demonstrated the ability...”; I dare say that to this point not much has been demonstrated besides showing two beautiful figures. Maybe the authors can tone this a bit down.

- Line 210; the authors mention that “adequate turbulence is developed”, where is this shown? What is the premised/argument used to judge that? A certain amount of energy at certain scales? A tke scaling of $k^{-5/3}$? A certain spectral comparison of tke with the field experiment at different heights? – Some of this might be more “objective”.

- It is unclear how the Cell Perturbation Method generates different type of turbulence for the stable and unstable stratification.

- From line 265 onwards, the text is riddled with subjective comparisons. I would suggest using percentages instead of absolute values when for example comparing wind speeds. Or avoid using comments like:“matches the measurements well”, without an actual metric of it. See line 311; "errors are small” in comparison to what?

- Why is the wind speed and direction outputted at different frequen- cies?

- Around line 367; the authors comment that the errors/discrepancies observed during the convective periods are larger than during the stable periods, but they don’t provide any comment, hypothesis, argument, detailed inspection, trying to investigate that in details,...

- Figure17, great visualization for oral presentations, proposals or others where “candy-to-the-eye” is well accepted; but what is the use of such an image here? What is the intended message, outcome extracted? – I am not suggesting the authors remove the figure but instead include scientific argumentation around it.

In conclusion, I am convinced the data presented in this manuscript is of high quality, and has the potential to become of high value for the community if made publicly available. My only concern is that there is not enough scientific value at presenting the data itself, when additional analysis (some of which does not require extensive work) could be added that would increase the scientific value of the work.