Wind Energ. Sci. Discuss., referee comment RC1
https://doi.org/10.5194/wes-2021-57-RC1, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on wes-2021-57

Anonymous Referee #1

Referee comment on "Quantifying wind plant blockage under stable atmospheric conditions" by Miguel Sanchez Gomez et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2021-57-RC1, 2021

The authors present a study on wind-plant blockage in stable atmospheric conditions. Although this work is very timely, I'm not convinced that it is a useful addition to the existing literature. As further detailed below, there seem to be some very serious issues with the set-up, and a number of the results. I'm not sure whether this can be easily fixed – this could require rethinking the whole simulation setup.

Major comments

- The authors discuss an inertial oscillation that they later 'subtract' from there simulations (affecting the streamwise evolution of the flow). This seems a rude fix, and a good set-up should simply avoid this type of issue. Also, inertial oscillations occur in time (at a pretty low frequency) – how can they affect streamwise flow over such a relatively short fetch? Unfortunately, in combination with some of the strange results reported in the paper (cf. below), this issue raises some serious doubts on the correctness of the methodology
- There are serious spanwise fluctuations in the inflow velocity (see, e.g., Figure 9). State of the art LES simply does not have this kind of problem. Also there is no real analysis on the cause of this issue. Presumably this comes from the parent domain, but not much analysis is performed (are these streaks existing in the parent domain as well, is this a result of the coupling methods, …).
- The authors use a 500m Rayleigh damping layer to avoid reflection of gravity waves. However, nonreflecting damping layers are tricky. I would expect that the layer should be at least one, possibly better two dominant vertical wavelengths. What is the vertical wavelength that can be expected based on wind-farm length and Brunt–Väisälä frequency? Can you report the level of reflection in your simulation – this can, e.g., be simply estimated using the method of Taylor and Sarkar (JFM 2007).
- Figure 5: the authors claim that upstream of the symbols marked on the figure, there is no significant measurable blockage effect. Why is it then that all simulations still provide a negative deficit far upstream – I would expect, statistically speaking, some of them to be positive. The chance at four heads is only about 6%.
- Figure 8. The inversion displacement keeps growing downstream of the farm. I would expect that it goes down again. You seem to define z_i based on max of dtheta/dz . If

so, this measure would include possible turbulent mixing at the interface (thus overestimating displacement, which should be based on a streamline). However, more problematic is that there should not be any turbulent mixing at the interface in a stable boundary layer situation. Looking at your forcing methods, it seems that you force up to the capping inversion, so also in the residual layer, which should not have any turbulence. If correct, this does not make any sense!!

- Section 4, and the analysis related to Figure 6 and 7: I'm not sure what exactly the point is of this exercise (apart from the fact that it is possible). Also, turbulent flux divergence is not enough to study the momentum balance. Other terms that definitely seem relevant are the mean momentum transport (e.g. in the entrance region of the farm), which are not discussed here. Other terms (e.g. pressure forcing) are probably negligible, but this should be discussed.
- Figure 11: as far as I understand, the figure discusses the effect of using a wrong upstream reference to define the normalized velocity deficit 2.5D upstream of the farm (a measure for the blockage). I would expect that using a reference that also is 2.5D upstream, should lead to a deficit that is zero. Any reference that is taken farther upstream should than lead to a larger deficit… Why is it then that using the true freestream velocity leads to the lowest deficit 2.5D upstream???

Other comments

- Abstract, first phrase: blockage does not just impact on the performance of the first row

- page 4: a sketch of the nested domains would be useful

- page 5: can you better justify the combination of surface cooling with 9h30min spin-up? This is an overland situation – most of the night has already been passed after 9+ hours. (I realize that often long spin-up is used to arrive at some sort of steady state in SBLs, but given that you are forcing with a mesoscale model, this feels unnecessary?)

- page 5: what is geostrophic wind? What is pressure gradient – are you using a geostrophic balance and barotropic conditions?

- Figure 2: would be interesting to see the profiles op to the top of the domain (up to 2500m). Also, can you add the slopes 0.01 K/m and 0.001 K/m into Fig. 2a

- line 147: please improve sentence

- Use of brackets for spatial averaging is ambiguous: sometimes it is averaging in y, sometimes in x and y (eg. in eq 3). Please improve notation throughout the paper for clarity

- Figure 4: explain in caption that averaging is only in y-direction

- z-statistic and \alpha: not clear from the text how they are related. Also lag-1 autocorrelation: not defined, no reference. Please provide a decent statistical analysis. I would prefer 95% confidence intervals on the results in Figure 5 (let the reader appreciate what differences are significant or not). Also statistical analysis can be based on moving block bootstrapping, rather than handwaving arguments on possible gaussian distribution and a proxy for integral time scale (which is presumably what you are implicitly doing with the lag-1 autocorrelation)

- line 172: not a proper sentence

- line 192: “The turbulent fluxes are calculated from 5-minute averages of the velocity field”… What do you mean by this? Not clear why 5 min averages should be used; seems an incorrect definition of turbulent flux

- Figure 12: please use confidence intervals rather than deciding for the reader what is significant and what not

- page 17, line 336: “A spectral analysis on the vertical and horizontal velocity at multiple locations in our simulations shows no statistical significant evidence of waves moving through our domain.” What do you mean with waves moving through the domain? Do you mean that you did a frequency analysis? That does not make sense, since the gravity waves would be stationary…