Review of *The Jensen wind farm parameterization for the WRF and MPAS models* by Y. Ma et al.

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The authors develop a meso-scale wind farm parametrization based on the Fitch scheme coupled with an engineering wake model to represent the internal wake losses. In addition, a new wake superposition method is proposed. The predicted internal wake losses are validated with field measurements.

Wind farm parametrizations cannot resolve internal wake losses and I think it is very interesting to overcome this by coupling an existing parametrization with an engineering wake model. The article has a focus on the internal wake losses; however, wind farm parametrizations in meso-scale models are mainly employed to estimate wind farm wake losses (for example wind farms situated in a wind farm cluster). Hence, I lack a validation of a wind farm wake produced by your proposed parametrization. You could for example look at the Horns Rev I wind farm wake as used by Volker et al. [1] or simply compare the wind farm wakes of the employed models (new and old) for the present idealized setup. More detailed comments are listed below; they need to be addressed before the article can be considered for publication in Wind Energy Science. Note that I cannot provide detailed comments on the numerical setup of the meso-scale models due to a lack of experience regarding this type of model. Also note that I have not yet looked at any other reviewer's comments before uploading this document because I want to provide an independent review.

Main comments

1. Line 26: You mention that mainly two numerical approaches are employed to study wind turbine wakes: LES and WRF. However, there are a lot more numerical approaches to simulate wind turbine interaction, each model has its application area and purpose. Ranking steady-state wake models from low to high model fidelity one could think of: engineering wake models, simplified Reynolds-averaged Navier-Stokes (RANS) models as 2D RANS, parabolic RANS models [Iungo et al. (2018) [2]], linearized RANS (FUGA); and full 3D RANS with actuator disks (AD)[3]. Then we have transient wake models: Dynamic wake meandering model, unsteady RANS with actuator lines, LES-AD, LES with actuator lines (AL), WRF-LES-AD and WRF with a wind farm parametrization. Many of these model are described in Göçmen et al. (2016) [4], which you already refer to.

2. Line 74: Here you mention that the Jensen model works well for any wind farm layout based on the work of Archer et al. (2018) [5]. I do not agree with this strong statement since the performance of the model heavily relies on the wake superposition method and wake expansion coefficient. For example, the Jensen model does not work well for a tightly packed wind farm as Lillgrund and a below-rated inflow wind speed when the original quadratic wake superposition is applied (M2 method in the present work) [6], as you also show in the results section. I could not find the chosen superposition method in the work of Archer et al. (2018) [5], which makes is difficult to understand the results in that work. I think it would be better to write that the Jensen model could be calibrated to get the desired result for a given wind turbine type and wind farm layout.

3. Section 2.2.2: It is interesting that you investigate different superposition methods. What is the physical meaning of method M4? I find it strange to superpose the absolute wind

speed instead of the wake deficit. It means that you take a L2 norm of the wake wind speed normalized by the number of upstream turbine wakes, which is a form of averaging rather than a summation or superposition method. In other words, method M4 will always smooth out the wake effects between the largest and the smallest (upstream) wake wind speeds. There are also situations where the superposition method does not make sense. For example if one has a regular layout with a large lateral spacing (in y) but a small stream-wise spacing (in x), such that the turbines do not interact laterally, then a downstream wind turbine, will still experience reduced wake effects from the lateral turbines because you divide by the number of upstream turbines N.

4. How is the freestream wind speed defined/ calculated in your model? If it is the cell average, then this would mean that you would need to iterate over the Jensen model, since the cell average influences the internal wake model results, which influence the cell average. In case you use the freestream from the inflow, which is possible for an idealized case, how would you extend this to a realistic meso-scale setup where the inflow is transient?

5. How do you calculate the cell average wind speed from the Jensen model? Do you use the cell area only (and thus you would disregard the part of the wakes that extend beyond the cell area) or do you use an area that covers the entire wake of all the turbines within a cell?

6. Equation (14): Should the left hand side be $\frac{\partial v_k}{\partial t}$?

7. Line 240: You propose to use a Gaussian filter with a standard deviation of 2°, where did you base this value on?

8. Line 284: Why do you use a latitude of 50°? Lillgrund and Anholt are located at 55.5°and 56.6°, respectively.

9. Section 3.1: Why are the measurements not filtered for neutral conditions? Without such a filter it is not fair compare the numerical results for neutral conditions with the measurements. If you prefer to keep the current measurements results you need to clearly state (in results section and conclusion) that the model validation is not entirely fair.

10. Section 3.2: It seems that you are modeling an idealized setup in the mesocale models. How do the steady-state inflow profiles of wind speed, wind direction and TKE/TI look like for both WRF and MPAS? You could plot these results in Fig. 11. What is the actual roughness length applied by the Charnock relation? What is the turbulence intensity at the investigated hub heights?

11. Figures 3-8, measurements: What is the wind direction bin size used for the observations? In addition, you could consider to plot the error bars as the uncertainty of the mean (standard deviation dived by the square root of the number of samples).

12. I do not think it is fair to normalize the wind turbine power of Fitch with the power of the first row wind turbine, as Fitch is meant to model the average power of a number of wind turbines (or entire wind farm for the single cell case). You could overcome this by normalizing the wind turbine power of all the models and measurements with a hypothetical free-standing wind turbine power (using the free-stream value at the wind farm location [without any wind turbines present] and the power curve. The measured free-stream could be evaluated by using the power of a group of front row turbines and the power curve. A similar approach was performed in Hansen et al. (2015) [7].

13. Line 478: You mention that the Jensen parametrization is insensitive to grid resolution: *The Jensen parameterization tends to underestimate the power, regardless of the alignment or non-alignment conditions and regardless of the grid resolution. The consistent sign of the bias (negative) in column power output and the absence of sensitivity to the grid resolution or wind direction are all desirable properties.* However, in order to show a grid independence you need to compare results of at least three different grid levels and show that the results converge with horizontal grid refinement. For example, you can add another grid level in Figs. 9 and 10, for example for 0.5 km or for 4 km. (The 4 km cell size might not be interesting for

Lillgrund as this is the same at the single cell approach. (I am aware that grid-independence is challenging for WRF due to parametrizations that rely on large horizontal cells, but it might be possible for an idealized setup.)

14. The cases considered in this article only look at high thrust coefficients, since a below rated inflow wind speed is used. The conclusion on the best superposition method might change if you had considered an above rated wind speed, where the thrust coefficients are smaller. This it because the performance of a super position method can be shown to be dependent on the thrust coefficient, see for example Machefaux et al. (2015) [8]. You could add an above-rated case or you could add a comment/discussion in the article.

15. Line 370: You write: *Both M3 and M4 reproduce well the feature of power output becoming steady after the fourth turbine in an alignment column (Fig. 4).* I understand what you mean, but I think it is better to write about a balance instead of a steady-state. Here, I mean a balance between the momentum extracted by the wind turbines and the momentum transport into the wind farm from the boundary layer.

16. Figure 11: I would normalize the wind speed by the freestream wind speed, U_∞, and the TKE by $\sqrt{2/3\text{TKE}}/U_\infty$. The height can be normalized as $(z - z_H)/D$, with z_H and D as the hub height and rotor diameter, respectively. The same applies to Figs. 12 and 13, where the cell power production could be normalized by the total wind farm power.

17. Line 496: You mention that a wind direction of 222° results in the largest wake losses for Lillgrund but this should 120 or 300°, where the wind turbine spacing is smallest.

18. Line 547: When referring to the Gaussian wake model, I think you need to refer to the original work of Bastankhah and Porté-Agel (2014) [9].

Minor comments

1. Thanks for referring to my work. My last name is written with small letters for the first two words: *van der Laan*.

References

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