Review of *The Jensen wind farm parameterization for the WRF and MPAS models, R1* by Y. Ma et al.

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The authors have mostly responded correctly to my comments and I accept the article to be published in WES. It is rather unfortunate that authors cannot change any figures because the first author is unavailable and this person was the only capable of making the plots. The proposed changes in figures from my side are minor so I think it should not be a big problem for the present article. However, I would recommend that more than one author can actually produce and modify the figures for future article submissions.

In my personal opinion, if an author is not willing to be part of the review process one could consider to remove this author from the author list and possibly add this person to the acknowledgments. This may seem harsh but all authors should be part of the review process as all authors can be held responsible for what is written in the article.

I have a few comments regarding the responses from the authors (that do not need to be addressed for present article):

Main comments

3. Thanks for clarifying M4, I was clearly confused about the parameter N. I follow your argument on M4 that it is beneficial to have a superposition method that does not lead to zero wind turbine power for those cases where we are not expecting below cut-in wind speeds. It is true that none of the superposition methods follow mass or momentum conservation but I am not yet convinced that M4 makes sense as a wake superposition method. I think a comparison the new superposition method with a full CFD model could be an interesting verification study.

4. You could consider an inversed 1D momentum method to estimate the freestream from a local cell-averaged wind speed similar to Abkar and Porté-Agel 2015 [1].

8. The effect of Coriolis forces on a small wind farm is indeed negligle as you have shown, but if you had investigated a larger wind farm, then the effect should be much stronger and you would probably have found larger impacts on wind turbine power, see for example Fig. 6.2 in [2]. I do agree that using 50 or 55 degrees for the latitude is probably not going to be critical for the results as the Coriolis parameter or surface Rossby number would only be 10% different and the effect of the surface Rossby number in the inflow is small for offshore surface Rossby numbers (10^9), see for example Figs 6 and 7 from [3].

References

[1] M. Abkar and F. Porté-Agel, "A new wind-farm parameterization for large-scale atmospheric models," *Journal of Renewable and Sustainable Energy*, vol. 7, no. 1, p. 013121, 2015. [Online]. Available: https://doi.org/10.1063/1.4907600

[2] M. P. van der Laan, K. S. Hansen, N. N. Sørensen, and P.-E. Réthoré, "Predicting wind farm wake interaction with RANS: an investigation of the coriolis force," *Journal*

of Physics: Conference Series, vol. 625, p. 012026, jun 2015. [Online]. Available: https://doi.org/10.1088/1742-6596/625/1/012026

[3] M. P. van der Laan, M. Kelly, R. Floors, and A. Peña, "Rossby number similarity of an atmospheric rans model using limited-length-scale turbulence closures extended to unstable stratification," *Wind Energy Science*, vol. 5, no. 1, pp. 355–374, 2020. [Online]. Available: https://wes.copernicus.org/articles/5/355/2020/