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# Comment on wes-2022-112

Gonzalo Pablo Navarro Diaz (Referee)

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Referee comment on "A new RANS-based wind farm parameterization and inflow model for wind farm cluster modeling" by Maarten Paul van der Laan et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2022-112-RC1, 2022

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**general comments**

Dear authors, the topic addressed in your work is very innovative and important for the wind energy industry. This is because you study how to simulate the wake interaction between neighboring farms and its impact on production, a problem that is emerging in current and future offshore wind farm projects. The development of wind farm parameterization was something missing in CFD-RANS, and you seem to have made the first step towards it. The comparison of this new development with the parameterization in WRF and the engineer wake model TurbOPark adds a good perspective with another way to simulate wind farms at a lower computational cost. The comparison with SCADA measurements has not been entirely satisfactory. The writing is clear and easy to follow. Below you can find my major and minor revisions.

**specific comments**

**Major revisions (require changes in the results):**

line 125- The lack of inflow velocity measurements, as well as non-availability of the SCADA data from neighboring farms make the comparison between simulations and SCADA on the first row of turbines very weak. I suggest deleting the comparison with SCADA from this work.

line 290- The efforts of your research group to couple the Apsley and Castro mixing length limitation methodology with the wake simulation using k-e-fp model is not mentioned in the paper (https://iopscience.iop.org/article/10.1088/1742-6596/1934/1/012012 ). Despite this, the new methodology that includes the inversion layer seems to solve the problem associated with the limitation of the mixing length in the wakes. Unfortunately, you do not investigate this advantage, for example by comparing both methodologies for a particular case. In the end, the reader will not know if this problem in the wake is solved and raises doubts about the validity of the results presented. Please, I would like to see more results related to this topic.

line 335- The new methodology of simulating neutral ABL including the inversion layer looks promising. It would be very important that in figure 4 you add the precursor results using the previous methodology of mixing length limitation (https://iopscience.iop.org/article/10.1088/1742-6596/1934/1/012012 ).

**Minor revisions (require changes in the text):**

line 150- The placement of the farm parameterization in the cell extrusion buffer zone can cause problems both on the Gaussian distribution of the drag force as well as in the resolution of the wind farm wakes. Please discuss this in the manuscript.

line 160- I would like you to explain the meaning of "inlet boundary condition" applied to the top.

line 180- Why do you use generic Ct and Cp values for the turbine installed in Dudgeon?

line 180- Could you review the description of the AD model used? Different AD models are mentioned in the text and it is not clear to the reader what you finally use.

line 185- Have you verified if the resolution of the mesh on the disk (D/8) is sufficient for applying non-uniform thrust and tangential forces?

line 195- The use of a constant Ct throughout the farm is a very strong simplification, especially when simulating velocities close to or higher than the rated velocity. At these speeds the Ct of the first row of turbines is lower than the turbines impacted by wakes and therefore with lower velocity. Please clarify this limitation in the text.

line 200- The justification for why you don't inject extra TKE is a bit vague. Could you justify in a more complete way?

line 215- You propose to distribute the drag force in a Gaussian way. Have you checked if farm size and equivalent wake spanwise width is affected by this strategy?

line 235- One of the main limitations of the farm parameterization that you propose is the need to use pre-calculated wind farm drag force magnitude, which depends on both velocity and direction. This brings with it a large computational cost of simulating each farm with AD for different inflow conditions. Even though you mention this in line 580, in the abstract you do not take into account this extra cost in the percentages. In my opinion, this makes the use of the parameterization that you propose very impractical. I know this is your first approach to the farm parameterization in RANS, but in future work I would like to see more developments on the model.

line 260- it is not clear if the parameterization can respond to a non-uniform inflow condition (such as the partial wake impact of another upstream farm). Looks like in line 530 you found the problem related to this. Please clarify this in the text.

line 370- Why do you use the explicit wake parameterization (EWP) instead of the Fitch parameterization? Especially when a good comparison has been found between RANS and Fitch (https://wes.copernicus.org/articles/7/1069/2022/)

Thank you for your good manuscript and I hope you find my comments adequate and useful to improve the quality of the work.