Dear editor,

First of all, we would like to express our gratitude for the time and effort of the editor and reviewers and would like to thank the editor for considering our paper for publication in Wind Energy Science and providing us the time and opportunity to improve our manuscript.

We thank Reviewer 2 for their helpful remarks on our manuscript. Below, we will highlight our insights on the comments and several ways in which we have incorporated suggestions to improve our manuscript.

1. **Comment:** The authors make a point of the local blockage model being much slower than the wake model either alone or combined with the new global blockage model. But this hinges on using a default setting in PyWake for the convergence of the effective wind speed. Specifically, that the effective wind speed is changing by less than 1e-6 between successive iterations. This is a ridiculously small change given the accuracy one can presume of the model. I realize that this may be the default setting of the program, but it is hardly necessary. I miss a discussion of why the local blockage model may be inherently slower from a computational point of view than the wake model and the new global blockage model.
   Response:
   First of all, we highlight the need for accuracy in the convergence tolerance hyperparameter. In a setup with a large number of wind turbines and when taking the aggregate over a longer time interval, such as presented in this study, energy production can amount to an order of 1e5 GWh (see the results of A*). The convergence criterion defines for which update in wind speed (m/s) further iterations are no longer carried (see documentation: https://topfarm.pages.windenergy.dtu.dk/PyWake/notebooks/EngineeringWindFarmModels.html). Hence, since energy production is roughly proportional to the effective wind speed, an estimate on the level of one GWh requires a high accuracy in the convergence tolerance parameter.

   Furthermore, for large sites, an additional computational problem arises, which we discuss in our revised manuscript in Section 3.5. In a relatively regular layout with many interactions, the number of turbine interactions scales with O(n^2), where *n* denotes the number of turbines. Thus, the iterative nature in practice does lead to an inherently slower model, as the iterations must converge on all interaction combinations between turbines. This motivates the need for a different type of global blockage model which does not suffer from a superlinear increase in the number of turbines.

2. **Comment:** The coupling of the global blockage model utilizes some named features and methods in PyWake. The authors should rewrite these parts of the manuscript to make the methodology easier to follow for those reader not familiar with PyWake. Write down the appropriate equations explaining the coupling procedure and then

potentially include (maybe in an appendix) a description of how this can be accomplished in PyWake using already existing functionality.
Response: We thank Reviewer 2 for highlighting this. To further clarify the implementation structure, we have included a diagram highlighting the coupling setup in Section 3. Our implementation is not necessarily specific to PyWake, and can be used in all engineering wake model setups. . To clarify the coupling aspects in PyWake, however, we have included details in Section 3.1.1.

3. **Comment:** The new global blockage model depends on the height of the boundary layer H, the size of the wind farm (l) perpendicular to the flow direction, and a drag coefficient C_d. The model is built form analogy with blockage in a wind tunnel. The parameter C_d should depend on the geometry (porosity) of the wind farm. The authors choose a value of C_d=1 for simplicity. The wind farm size l is based solely on the front row. These choices may work OK for the regular layouts considered in the paper, but it is hard to trust that they will work equally for all types of layouts. The authors would do well to at least present an avenue for considering the details of the wind farm layout in future work.
   Response: As motivated by this comment as well as comments of Reviewer 1, we have extended our experiment setup to also optimize the parameter C_d to LES data, providing an example of an implementation to model different layout geometries. Furthermore, we have added a discussion for a strategy of more extensive layout parametrization through LES studies for future work in the conclusions section.

4. **Comment:** The key parameter of the new model is the boundary layer height. This is known to have a significant influence on the wind farm flow. The authors use the mean value of H for four 90-degree wind direction sectors. Given that the LES data on which the work is based are inherently time series this seems like a peculiar choice. Why not run the calculations in a time series manner and fully leverage the dynamics of the boundary layer height? The authors make a point out the differences between performing a summation over directions before or after the errors are calculated. But for the important parameter H they are content with averaging over large wind direction sectors.
   Response: We thank Reviewer 2 for raising this question on accuracy and granularity of H. The addition of running the wake model on time-series data is indeed a promising avenue for future work, which we have included Section 3.1 and the conclusions section. For the current paper however, we limit our scope to the steady-state wind flow modeling for which wake models have originally been designed. We agree with the concerns on the accuracy of the boundary layer height. Thus, in our revised version, we have adjusted our approach to take an atmospheric boundary layer height binned to every wind direction and speed bin in the wake model (averaged over 22.5 degrees wd and 1m/s ws), included in Section 4.2. We thank Reviewer 2 for this conceptual improvement.

5. **Comment:** Another nice addition would be to consider a less regular layout with a different wind rose to assess how well the calibrated model can be applied at a very

different site.
Response: We highlight that already in the case of our site for wind setup A*, the corner as seen from the south-west direction gives a much more irregular shape compared to the north-east direction which is used for model training purposes. Furthermore, since new wind directions are included with different wind characteristics compared to the training data (as can be seen in the wind rose), this in essence gives a view of how the calibrated model parameters carry over to another site. In view of the model assumptions, we deem it to be out of the scope of this research to include even more irregular (e.g. highly non-convex) layouts, although this is an interesting avenue for further research, which we have included in the conclusions section of our manuscript.

We now consider the numbered comments:

1. **Comment:** *What is the hub height and rotor diameter of the turbines? This information is needed if one is the replicate the results.*
   Response: this information is included in our citation of Gaertner et al. (2020), highlighting the technical specifications of the turbine. For additional clarity we have restated this in-text.
2. **Comment**: *How is the boundary layer height calculated from the LES data? Specifically, given the many different definitions of the boundary layer height what is the chosen definition in this work meant to signify physically and how is this related to the proposed global blockage model?*
   Response: As in the response to Reviewer 1, we have included a description in Section 4.2.
3. **Comment**: *How is the coupling between the wake model and the new global blockage model done? Is it only one-way or is it iterated?*
   Response: as highlighted in our response to Reviewer 1, we have presented a schematical overview of this (one-way) coupling in a diagram, included in Section 3 of the manuscript.
4. **Comment**: *What was the period for the LES data?*
   Response: we have included the LES data period (the calendar year 2023) in Section 4.2.
5. **Comment:** *Was any filtering applied on wind speeds or did all the cases include all wind speeds from the LES?*
   Response: Cases A, B, C include only northeastern wind speeds, whereas A* includes all wind speeds, as highlighted in our manuscript. For additional clarity, we have highlighted this again in Section 4.4.
6. **Comment**: *From Figure 2 it seems that the chosen local blockage model does not modify the flow field does not modify the flow field downstream of the rotor. However, in reality the flow is accelerated around the rotor. There are other local blockage models that include this effect.*
   Response: We thank Reviewer 2 for raising awareness of this "speedup" effect. In fact, this model (SelfSimilarityDeficit) does create a speedup acceleration effect (see e.g. https://topfarm.pages.windenergy.dtu.dk/PyWake/notebooks/BlockageDeficitModels.

html#SelfSimilarityDeficit). We note that the colorbar in our plot was "clipped off" for wind speeds > 10 m/s which hid this graphical result. We have updated the plot and included a mention of this effect in Section 3.

7. **Comment**: *Line 206: should northeastern wind speed be northeastern wind directions?*
   Response: We thank reviewer 2 for noticing this mistake and have corrected this in the manuscript.
8. **Comment**: *Figure 8: the grey dots are hard to see in the plot. Choose a more distinctive colour.*
   Reponse: We have updated the Figure and thank Reviewer 2 for raising this concern.
9. **Comment:** 2: *Line 253: are predictions errors the RMSE?*
   Response: These errors are indeed RMSE and we have highlighted this in the text
10. **Comment**: *Line 301: what does deficit inducing wake shape mean?*
    Response: We have adjusted the phrasing used for clarity.
11. **Comment**: *The crescent features of the plots, see first panel near the point (14, 14), are so prominent that they call for an explanation. What are we seeing here?*
    Response: We have added an explanation and thank Reviewer 2 for pointing out this effect.
12. **Comment**: *Figure 11: the caption states that values for the A*case appear more correlated because they are aggregated over all wind directions. It would be useful then to see the results from this case for isolated wind direction (sectors). Perhaps the four quarters of the unit circle.*
    Response: We have added this plot (see Figure 14) and thank Reviewer 2 for highlighting this.
13. **Comment**: *Figure 11: I assume there is one dot per turbine, but this should be made clear in the caption.*
    Reponse: we have highlighted this fact in the description for additional clarity.