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 1 for the helpful comments and remarks on the manuscript. We appreciate the insights from the conceptual modeling point of view and would like to elaborate on how we incorporated these to improve our manuscript.

1. **Comment:** Compared to these physics-based models derived from the conservation of momentum, the global blockage model proposed here is not based on the principles of fluid mechanics and relies heavily on empirical tuning (or fitting) of the model. A fundamental issue in the proposed approach is that the wake expansion rate “k” in the turbine-wake model needs to be tuned for a given wind farm, using its power production data (which may not be available when we need to predict the power of a future wind farm). As shown in the paper (see Table 3), the value of k required to achieve the correct farm power is significantly different between the case without using the global blockage model (k = 0.0131) and the case using the global blockage model (k = 0.0210). This means that, although k is a parameter in the turbine-wake model, this tuning process is to data-fit the global blockage model itself (as the authors also mention in their conclusions near the bottom of Page 17).
Response: We agree that these parametrized wake and global models are indeed a simplification of physical phenomena. However, exactly because of this simplified structure and their ubiquitous use in industry, we are convinced of the need for explorative research in extending the parametrized approach to also include global blockage. The sensitivity of the k-parameter to the global blockage parametrization illustrates in our opinion the importance of developing a more extensive parametrized modeling approach. When taking a wake-only approach and fitting on data, parameter *k* essentially has to accommodate for this lack of global blockage effect, as we discuss in our results and conclusions.

2. **Comment:** This means that, although k is a parameter in the turbine-wake model, this tuning process is to data-fit the global blockage model itself (as the authors also mention in their conclusions near the bottom of Page 17). The other key parameter in the proposed model, namely the farm’s drag coefficient “C_d”, is not tuned in this study (C_d = 1 is assumed in this study) and still the proposed model appears to give satisfactory results, but this does not mean that the global blockage effect is insensitive to the value of C_d.
Response: First of all, we would like to emphasize that as of now the global blockage model and wake model take a sequential order of operations and are not coupled: the global blockage model outputs a reduction in incoming wind speed, which is then used as input for the parametrized wake model. To further highlight this, we have included a diagram highlighting the current setup in our manuscript in Section 3. We do agree that in our first version, the assumption having global blockage

parameter $C_d = 1$ constant leads to an experiment setup with a parameter fit on wake shape parameter k only. Even though this setup already highlights an interesting shortcoming in the classical no-blockage wake modelling setup, we agree that varying C_d increases the model validity. Therefore, we have changed our global blockage experimentation setup to also optimize on the global blockage parameter C_d. Taking the wake shape parameter as fitted in the non-global blockage case and $C_d = 1$ as starting point, we find similar values when fitted to our LES data, with similar order of magnitude increase in wake shape parameter k and a value of C_d in the order of magnitude of 1. This leads to a more robust conceptual result, providing a fully "data-fitted" approach for all unknown parameters in our current modeling setup, and we would like to thank Reviewer 1 for highlighting this. To showcase the sensitivity on C_d, we have included a grid-search showcasing sensitivity on C_d and k in Figure 9 of the manuscript.
As an aside, please note that this two-parameter setup requires an optimization over the mean squared error to prevent ill-posedness of the calibration step. For clarity and comparison, we have thus changed the calibration goal for all setups to minimize the mean squared error instead of the bias.

3. **Comment:** As shown by recent LES studies (e.g. Lanzilao et al. 2024), the reduction of farm-upstream wind speed depends significantly on the characteristics of the atmospheric boundary layer (as well as on the design of the wind farm itself) since the global blockage effect depends significantly on the characteristics of farm-induced gravity waves (among others). This suggests that, to sufficiently tune or train the proposed global blockage model (via the fitting of the parameter k) for different wind farms and for different atmospheric conditions, we would need the farm's power production data for those different wind farm scenarios. Since it would be difficult (or computationally expensive) to have power production data for a range of different scenarios, I suspect that the applicability of the proposed approach to different wind farm scenarios would be rather limited.
Response: We would like to highlight that this fundamental issue in calibrating k is manifest in the whole field of parametrized engineering wake modeling (see for instance the work of Binsbergen et al (2024) and Teng and Markfort (2020) included in our references). In practice, without operational data, this obstacle of fitting the wake shape as well as global blockage parameters can be overcome by calibrating on high-fidelity LES production and atmospheric data such as carried out in the manuscript. As we present in the results and as discussed in the conclusions section of our manuscript, we show that incorporating our parametrized global blockage approach considerably improves validity of the parametrized model when applied to different layouts and atmospheric conditions. This is demonstrated in the manuscript in the result of model test setups B, C and A*. This thus enables a smaller calibration training setup from which a model parametrization can be derived for variations in the site layout and atmospheric conditions. We agree with reviewer 1 that more extensive research is required for extending the validity of our approach to conditions differing significantly from the training setup.

4. **Comment:** I would also suggest that the authors refine and proofread the whole manuscript before submitting it for further review. I am afraid that I found it difficult to read many parts of the current manuscript, largely due to inaccurate choice of words and unclear explanations.
   Response: We have revised the writing in all sections and would like to thank reviewer 1 for pointing this out.

5. **Comment:** How do you define the ABL height (i.e. how do you calculate "H" from your LES data) in this study? There are several different ways to define the ABL height.
   Response: We have highlighted our method of estimating the boundary layer height in Section 4.2 in the manuscript for additional clarity. We would like to thank Reviewer 1 for pointing this out.

6. **Comment:** The near-surface vertical grid resolutions adopted in this study (25m in the LES and 40m in the meso-scale simulations) seem rather coarse compared to other studies. Could you justify the use of these coarse grids?
   Response: We have added two references to validation studies that used similar simulation settings for wind resource assessment to Section 4.2. Moreover, we reason the use of the coarse grid for the actuator disc method in Section 4.3.

7. **Comment:** Also, the horizontal grid resolution adopted in the LES (100m) seems rather coarse, considering that the turbines are represented using an actuator disc model. The authors refer to Baas et al. (2023) for the LES set up, but I suggest more details of the LES be included in the present paper.
   Response: Please see previous response.

8. **Comment:** Appendices A and B do not have any text. I would suggest at least a few sentences be included in each appendix to explain about the figures presented there.
   Response: We have added a short description and thank the reviewer for pointing this out.