Review comments on "A global blockage parametrization for engineering wake models" (wes-2025-71)

This paper presents a very simple engineering model of wind speed reduction upstream of a large wind farm due to the so-called "global" (or wind farm-scale) blockage effect, to be used together with a traditional engineering turbine-wake model to predict the farm power. The proposed model is (loosely) based on the concept of a whole wind farm acting like a large porous medium to reduce the wind speed upstream of it. This is a different type of model from existing "local" (or wind turbine-scale) blockage models and, as the authors demonstrate in the paper, it allows a much faster prediction of wind speed reduction upstream of a large wind farm compared to existing local blockage models.

However, the idea of modelling the farm-scale wind speed reduction as a whole (and using it together with an engineering turbine-wake model) has already been explored extensively in the literature, e.g., the "coupled wake boundary layer" model (Stevens et al. 2015, 2016) and the "two-scale momentum" model (Nishino and Dunstan 2020, Kirby et al. 2023, Legris et al. 2023). 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). 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. Instead, this means that the global blockage effect, which is a farm-scale effect and should be parameterised through the farm-scale model parameters C_d and l, has been data-fitted entirely through the tuning of the turbine-wake expansion rate k. The authors have applied their tuned k values (fitted to power production data for the original farm layout A) to different farm layouts (B, C and A* as summarised in Table 2) to try to validate this approach, but these layouts (B, C and A*) are similar to the original layout (A) to which the k values were fitted.

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.

Based on the above reasons, I feel that this paper is not suitable for publication in Wind Energy Science. However, I do understand that the aim of this paper is to propose a simple and fast engineering global blockage model, which is needed in today's wind industry, and the proposed model may partially satisfy that need. I would therefore suggest a major revision of the current manuscript, taking into account the above points.

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.

Other minor comments/questions:

- 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.
- 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?
- 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.
- 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.

References

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