Review of the paper “Mitigation of offshore wind farm cluster wake effects by low-specific-rating, low-induction turbines”, by Paulsen et al.

General comment

The paper deals with the comparison between an innovative low-specific-rating and a conventional one, in terms of cluster wakes, farm efficiency and power production.

The paper is well written and the employed methodology is adequate for the scope of the study.

The wind energy community may benefit from this work, as the authors have clearly demonstrated that the use of a particular low-induction rotor (hybrid-lambda, HL) can lead to higher farm capacity factors, even when clusters of wind farms are analyzed. From this perspective, the comparison between the HL concept and an upscaled version of the IEA 15-MW turbine provides the key case for demonstrating the potential of the HL concept.

I recommend the acceptance of this paper with some minor requests, which are listed here below.

Minor comments

1. Section “2.4 Data post-processing and applied metrics”: the exclusion of the sector 180 deg < ϕ < 270 deg should be better clarified, as the reader could benefit from a simple explanation, in addition to the referenced work of Van Wijk (1990). Moreover, would it be possible to have an indication of the probability of wind coming from that sector? This piece of information is important to understand to what extent the employed data are representative of the real field conditions.
2. Section 2.1: typically, such tools may be strongly impacted by the grid resolution and by the number of refinement levels. Did the authors performe a sensitivity and convergence analysis of the simulations?
3. Lines 186-188: The sentence expresses a correct idea, but as currently written is unclear: C_P and C_T functions do not exhibit purely cubic and quadratic behavior, hence a decrease in a has a larger impact on $C_P$than $C_T$only close to $a = 0.33$. In contrast, around $a = 0$, the two functions show the same trend. Please, clarify.
4. Line 249-250: “we observe that in south-westerly direction and further away from the German coast higher capacity densities are planned compared to the clusters at the north-easterly boundaries of the EEZ”. It is not clear if this observation is taken from Vollmer and Dorenkamper (2024) or if it results from an optimization performed in the present work.
5. Line 285: the repetition of v_{eq} can be avoided to render the sentence smoother.

6. Fig 5: is there a reason for the increase in the REWS between IEA and HL cases observed in the inter-farm regions (those colored in light red)? This could indicate that the reference turbines generate stronger wake deficit but that their wakes recover faster.
7. Tab. 4: excellent table summarizing the main results. The fact that the reference upscaled turbine type (GB-IEA-D) is associated with a slightly lower CF than the HL one is the most important outcome of this analysis and should already be discussed at this point (without waiting for the discussion in section 4).
8. Line 384 and Fig. 8: is there a reason for the results obtained in the cluster N12 and N19? It seems that the improvement due to HL concept is more significant for those two farms of the cluster.
9. Section 3.3: can the obtained results be simply explained by the fact that the HL turbines generate weaker wakes? If so, what additional insights can the authors derive from the farm-cluster analysis compared to single-farm simulations?
10. Table 6: Why do Authors choose to express the differences between CF values in percentage terms? It seems that they computed the percentage of a percentage. Please, consider expressing the differences in percentage points. For example, the difference between GB IEA and GB-NL-IEA is 41.1-40.3=0.8 percentage points.
11. Line 467-469: this sentence explains the higher winds outside the farms observed in fig. 5. Could it be more appropriate to introduce this concept when commenting on Fig. 5? Please, consider this possibility.