Reply to reviewer #1
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The new or remaining remarks of the reviewer are repeated in blue point by point followed by our answers, while the main modifications in the paper are red-marked in the revised manuscript.

General Comments:
The provided revision has minor adjustments to previous comments, but does no properly address a number of my comments from the first review and some new comments/results appear misleading. This is at least partly due to important details still missing from the manuscript, which still leaves a reader guessing or assuming key details of what the authors have done.

We thank the reviewer for the time devoted to reading our manuscript and for providing helpful comments and feedback.

Specific comments:
Actuator Disc modeling and Power estimation:
In the reply to my review and the other reviewer, the implementation of the actuator disc is still lacking details. The authors state that CT is constant (e.g. line 170), but it is unclear how and if CT changes when yawing. In the reply to the other reviewer the authors repeat that CT is constant, but also state that it is function of yaw angle. This is quite critical for the present study. If CT is indeed constant, is the total thrust force also constant? If so, then a yawing turbine would reduce the streamwise induction (as the thrust is directed with an angle laterally), leading to an small increase in the mean streamwise wind speed and therefore increase in power P = u*T with constant T. That would mean that yawing turbines are not penalized in terms of power production. The LES results would essentially be meaningless and should not be published. I do not think this is what is going on, particular as the authors state that the efficiency of the turbines depends on the yaw angle, but as it is, I have to speculate and assume this. The authors need to describe this in proper detail for the readers, for example using an exponent to describe changes in CT and CP as in [2], which as mentioned in my first review is particular important for yawing turbines in wake. I'm not convinced that "efficencies are ...more suitable metric" as [1] clearly shows that the efficiency is overestimated with this actuator disc implementation. Reporting efficiencies for the simplified actuator disc makes it harder for readers to actually decipher the results. Again, having to guess/speculate on the underlying numbers due to lack of details. However, with proper explanation of how the turbines are modelled in yaw it might be OK. Finally, the authors do not address my concern about the yaw angles of the first four turbines in LES-BO being optimized to the limit of the allowable range (30°). This is clearly shows that the optimization limits are not wide enough, which is a basic flaw in optimization. The authors should address this.

We apologise for any lack of clarity in the original manuscript. The thrust predictions rely on a constant local (or "modified") thrust coefficient C'_T and the local velocity normal to the disk u_d, $T = \frac{1}{2}\rho A C'_T u_d^2$. Further to section 2.2 where these quantities are introduced, we have changed the discussion throughout the manuscript to highlight the consideration of the constant *local* coefficient C'_T (not the "nominal" one C_T) and the ability of our solver to predict power degradation as a

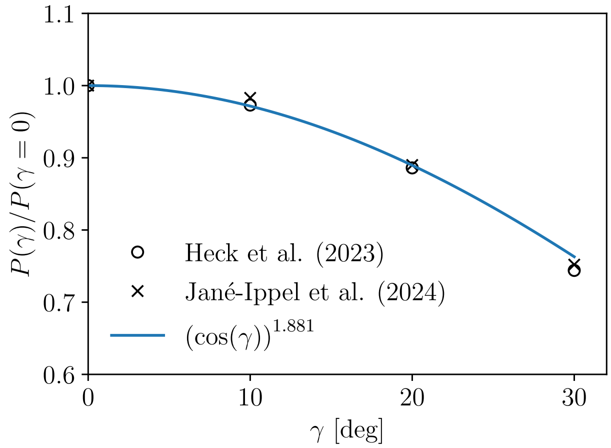

Figure 1: Power production of a yawed wind turbine with $C'_T = 4/3$, normalised by the power production of a yaw-aligned wind turbine.

function of rotor misalignment. In its most explicit and simple form (see e.g., [8]) the thrust can be expressed as:

$$T = \frac{1}{2}\rho A C'_T u_d^2 = \frac{1}{2}\rho A C_T(\gamma) U_\infty^2 \cos^2\gamma$$

where an approximation for $C_T(\gamma)$ can be found in [7]. Figure 1 shows the solver's predictions for turbine power as a function of the yaw angle (data are from [6] and were kindly provided by Mr. Jané-Ippel), together with the LES data of [5] and the approximation used by `FLORIS`.

Finally, a comment has been added in section 4.2:
"We note that the yaw angles of the first four turbines in the LES-BO design are at the arbitrarily selected yaw limits defined in the problem set-up (see Sect. 4.1). This means that there is potential for further improvements should these limits be extended or removed. In practice however, for large yaw offsets, it is important to also consider the effects of yawing the turbine on the loads it experiences [3, 4].

I apologize that I made an error in my previous assessment of the blockage. However, I still suspect that the reply and additional text by the authors is misleading. Evaluating blockage in terms of the difference in the maximum streamwise velocity within the domain is not representative of the impact of numerical blockage on the operation and power production of the wind farm. I suspect/guess the small increase in maximum velocity occurs high in the domain, where the velocity is already high due to the atmospheric boundary layer. In order to assess the numerical blockage, you should report speed ups of mean wind speed at the turbines and differences in power production with the two different domains, one 3 times wider than the other. Typically, physical wind farm blockage is results in 1-2% changes in power, see e.g. [5, 6], not 5-8% as shown in Figure 7.

The maximum streamwise velocity was reported as it was relevant to a comment of the second reviewer. In the revised manuscript, we have clarified that it is measured at the hub height, which is a characteristic level for the wind turbines. Additionally, we report the difference in farm power production from the simulations with the two different domains. This was found to be $\approx 1.36\%$. The large increases relative to the production of a single wind turbine are a consequence of local blockage (i.e., flow being locally accelerated due to streamtube contraction), similar to what might be observed in the second row of staggered wind farms (see e.g., [9]).

I find it a bit hard to understand this sentence and where it is placed in the text. Does "The turbines can align themselves..." refer to the optimisation setup or to reality? If it relates to the simulations and the setup, then it implies that the authors have utilized a numerical wind direction controller. If so, is the reference baseline before optimizing the yaw angles including alignment to local wind directions for all turbines? The footnote clearly relates to reality, but placed in the setup of the optimisation. I suggest to rephrase to correctly represent if the simulations does or does not include a wind direction controller utilized to determine the baseline, and that this is a challenge in reality. I suggest to place such comments in a more appropriate section, for instance while discussing limitations and uncertainties of the present study.

This refers to the fact that we have a six-directional wind rose (see, for example, figure 2 in the manuscript). The wind rose shows the mean direction of the incoming wind, and the sentence in question notes that the turbines have a zero yaw with respect to that *mean* direction (no real-time or local mean controllers are used). The sentence has now been changed to reflect the above:
"The turbines are aligned with the mean inflow wind direction in all cases considered in this section (no controllers are considered). In reality, accurate and robust estimation of the wind direction poses a significant challenge [1]."

OK. The authors have added a few scattered comments on uncertainty, but the uncertainties remain in the presented results, and comments on uncertainties and limitations remains limited. Therefore, I suggest the authors to be less certain and take more reservations to their own results, for instance on wake models performing "outstanding" compared to an simplified turbine representation in CFD. In my opinion, it is good scientific practice to be critical and outline limitations and uncertainties of ones results. It does not take away value, but builds confidence and facilitates scientific discussion.

The sentence has been changed to "The performance of wake models was also found to be remarkable."

The sentence has been rephrased, but the meaning is the same: Larger wind farms are less efficient than small. This is still misleading and I think this statement requires a solid reference, as I have provided reference which shows the opposite and as commented in my previous review this is (to my knowledge) not general knowledge and stands as an unsupported statement.

Our statement refers to long (or deep) wind farms, i.e., that the twentieth row of a wind farm will typically produce less power than the fourth (row numbers are indicative). Then, making the farm larger by adding rows at its "end" would decrease its overall efficiency. The sentence in question has been changed to reflect the above:
"One key issue is that wind farms that consist of many turbine rows are typically less efficient than less deep wind farms (i.e., downstream rows produce less power than upstream ones) [2]."

"f" is now defined as "the standardised overall farm power output", but this is not a standard definition nor is it clear. Please provide a definition in terms of an equation how "f" is computed.

The following has been added in the caption of figure 5:
"f is the standardised overall farm power output, $f = (P_F - \text{mean}\,(P_F))\,/\sigma(P_F)$, with P_F denoting the farm output."

My point was that only two of the six EI-LES-BO bars are hatched. What is the difference between hatched and non-hatched bars? If no difference, please provide consistent plotting.

All six EI-LES-BO bars are hatched. This might be an issue with the software used by the reviewer to read the document.

Additional comments:
- Line 179: Boundary conditions of the simulation setup is detailed in both Sections 2.2 and 3.1. I suggest to move the LES description to Section 2.2.
- Figure 2: I'm not familiar with how precisely regulators define areas and if it is based on location of towers or full extend of rotor, but it is clearly visible in Figure 2 that the rotor of the top turbine extends outside of the available area. Please comment.
- Figure 7: Please extend the y-axis to cover the full range so readers can assess increase in efficiencys above 100.

- The discussion in section 3.1 has been changed to: "The flow field planes stored from the precursor simulation (see Sect. 2.2) are used at the inlet, a convective condition is used at the outlet, and the conditions at the remaining boundaries are similar to the description in Sect. 2.2."
- The discussion in section 3.1 has been changed to: "The land where the turbine towers can be placed is a square of size $18D \times 18D$ (for this set-up, the blades can extend outside of the available area)".
- Done.

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