Authors response on the review comments on

Mitigating wind farm cluster wakes by vertical staggering

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We would like to thank the reviewers for their detailed and overall very positive feedback. The comments helped us to improve on several points and add previously missing details. Below are the reviewer comments in *black*, directly below each point our response in *blue*, and changes in the paper in *green*. Partly line numbers of implemented changes are mentioned, which refer to the revised manuscript unless mentioned otherwise.

Reviewer #1

1. In this study the turbines are "modelled with an actuator disk model with rotation" (Line 161) but the authors do not mention explicitly in which direction each disk rotates. Since there are some previous studies on the impact of turbine rotation direction on wake developments (e.g. Englberger et al. 2020, https://doi.org/10.5194/wes-5-1359-2020; Nouri et al. 2020, https://doi.org/10.1016/j.apenergy.2020.115511) I wonder if such turbine-scale physics may be relevant to the formation and strength of the large circulations discussed in the present paper. It would be good if the authors could (at least briefly) discuss this point.

We added in the methods that the turbines rotate in standard clockwise direction. Also, we added a small paragraph in the discussion on a possible influence of the turbine rotation and choice of the turbine model (ll. 785-790).

2. In this study the authors make comparisons of farm-scale wakes for different farm configurations (i.e. vertical arrangements of turbines) but I wonder if these comparisons could be presented in a better manner. When we make a comparison of turbine wakes for different turbines, we almost always want to check the turbine's thrust coefficient and axial induction factor first, because if the thrust coefficients and/or the induction factors are different (or unknown), it would be difficult to make a fair comparison of turbine wakes. Similarly, to make a fair comparison of farm-scale wakes, it would be important to first check, at least, the total thrust of each farm for different farm configurations. The power production data in Section 3.3 are useful, but I think the thrust is more important than the power here (for comparison of wake development and recovery). If possible, it would be useful to also check the farm induction factor (see e.g. Nishino and Smyth 2026, https://doi.org/10.1017/flo.2026.10047) by calculating the farm-average wind speed across 30m < z < 270m (baseline) and 30m < z < 510m (case S1), for example.

We thank the reviewer for raising this point. We now calculated the wind farm induction factor according to Nishino and Smyth (2026) when describing the wake deficit in Sect. 3.1.1 for better quantification of the wind speed reduction within the wind farm area (ll. 317-325). For calculating the wind farm induction factor b, we performed the average for all setups over the largest height range of the rotor area, so from 30 m to 510 m, to be consistent with the wake calculation. Still, we note that for B1 this is not the true wind farm height and calculate for completeness also the induction factor over the wind farm height (30 m – 270 m). In Sect. 3.3 we then compare the power increase to the wind farm induction factor, pointing out that S1 has a larger power production than B1 despite a smaller induction factor (ll. 526-530).

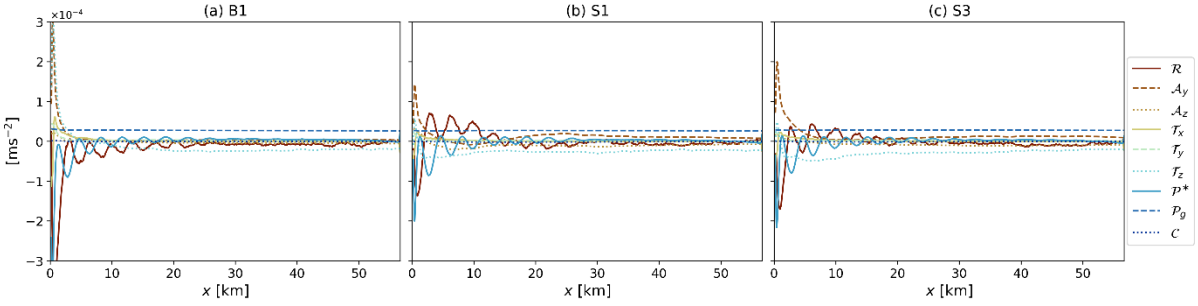
Regarding the thrust, we calculated the change in the total thrust of each wind farm and summarised it next to the power in Table 2 (see below). For WF1, the thrust increase is around 60 % of the power increase for all setups, which is roughly in line with the cubic dependency of power and quadratic dependency of thrust on the wind speed. To remove the influence of different wind speeds inside the wind farm, we calculated a thrust coefficient for WF1 of each setup, based Nishino and Smyth (2026) with $c_T = \sum_{i=1}^{N} T_i \,/\, (0.5 \cdot \rho \cdot \mathrm{REWS}_{\infty}^2 \cdot N \cdot A)$ where N is the number of turbines, T_i is the turbine thrust, A is the rotor swept area and REWS_{∞} the rotor equivalent wind speed of the upper or lower rotor plane in the inflow. We included these thrust coefficients in the paper for a more complete overview of the wind farm operation. However, we still keep the results on the power production, since we mainly want to show how heterogeneous wakes and the associated wake recovery of different setups affect the power.

Table: Change in power production (P) and thrust (T) of WF1, WF2, and both wind farms combined for all setups normalised by the respective wind farm of B1 ($\Delta P = (P_{S_i} - P_{B1})/P_{B1} \cdot 100$ and $\Delta T = (T_{S_i} - T_{B1})/T_{B1} \cdot 100$).

	B1		B2		S1		S3	
	ΔP	ΔT	ΔP	ΔT	ΔP	ΔT	ΔP	ΔT
WF1	±0 %	±0 %	+27.8 %	+16.5 %	+19.1 %	+11.6 %	+45.3 %	+27.9 %
WF2	±0 %	±0 %	+2.7 %	+1.2 %	+0.9 %	+0.3 %	−11.8 %	−7.2 %
Total	±0 %	±0 %	+17.3 %	+9.7 %	+11.5 %	+6.6 %	+21.5 %	+12.2 %

3. The authors "applied a low-pass filter along the streamwise direction" (Line 254) to remove the oscillations related to gravity waves. I understand that this filtering was helpful/necessary to distinguish the effects of vertically staggered turbine layouts from the effects of gravity waves, but I do wonder what the original flow fields (including the effects of gravity waves) looked like. Would it be possible to present one example of the original flow data (including gravity waves) such as the wake recovery budget (like Fig. 5) in this paper, perhaps as an appendix? This might be more useful than the current appendix A2 on TKE contours (about which no text has been provided).

We added an additional appendix section A3, which includes a figure of the momentum budget with the same data as Fig. 5, except that this time no filtering was applied to include the effect of the gravity waves (see below). Oscillations primarily showed for the recovery term and the perturbation pressure term, which were of similar magnitude. A brief discussion of the differences was added.



Following the comments by both referees, appendix A2 now also includes a description of the TKE results.

Other minor comments:

Line 173: The grid spacing (20m x 20m x 10m) has already been given in Line 167.

We removed the numbers and added the clarification that the grid spacing of the main run is the same as for the precursor (l.177-178).

Line 187: What do you mean by "meaningful statistics" here? A little more explanation would be helpful.

We changed "meaningful statistics" to "statistically converged turbulence statistics", to clarify that a long average is mainly relevant when looking at the turbulence of the LES.

Line 204: I guess what you mean by "wind farm power" here is the rated capacity rather than the actual power production.

Correct, to make this clear we changed "wind farm power" to "rated capacity of the wind farm".

Line 260: What do you mean by "moving average" here? A little more explanation would be helpful.

We added that the moving average was applied along y with a window of 600 m (l.276).

Line 480/481: "This results for the vertically staggered wind farms in absolute deficit differences by up to 30 % at the end of the domain." - This sentence was confusing to me. I guess "for the vertically staggered wind farms" should be placed at the start of this sentence instead.

Changed as suggested.

Line 604: The definition of the Rossby number should be given here.

Definition of the Rossby number added:
"This is mainly relevant for Rossby-numbers around or smaller than 1, where the Rossby number is defined as the ratio between the velocity scale and the product of the Coriolis parameter and horizontal length scale."

Reviewer #2

1 Specific Comments

line 17: the authors mention in the abstract that vertical staggering results in a "a better wake recovery at lower hub heights inside the wind farm". I would either remove or change this sentence because it is misleading and reductive in my opinion. To my understanding, this is not quite what's happening. The staggered wind farm simply has less thrust applied at the lower hub height, so it would be more correct (and this is shown by the wind speed evolution at the lower hub height) to say that wake deficit is distributed over a taller layer of the ABL. Consequently, it is straightforward to me that deficit is reduced when only looking at the hub-height. However, additional physical processes take place inside the wind farm, the most important being the streamwise streaks and the turbulent advection of low wind speed from above (for the staggered cases), which hinders wake recovery especially in the wind farm near wake. For these reasons I would leave the above-mentioned sentence out. The taller turbines and a lower rotor density per height are in my opinion sufficient to explain the increase in WF1's power.

We thank the reviewer for pointing out that some phrasings in the abstract seem contradictory, which we tried to improve now. While it is true that the higher wind speeds at hub height are largely related to the lower rotor density and taller turbine heights, we did find for S1 inside the wind farm and in the first few kilometres downstream of the wind farm an increased vertical advection of high wind speeds from above, promoting wake recovery inside the wind farm. The hindering of the wake recovery through vertical advection, as mentioned in the comment, exists only from ~5 km downstream of the wind farm onwards. We therefore think that it is accurate to mention a "better wake recovery at lower hub heights inside the wind farm" (l. 17 of the original manuscript). Still, we do see where the confusion is coming from, since we mention a bit earlier in the abstract (ll. 8-9 of the original manuscript) the hindering of the wake recovery, but do not mention explicitly that we refer to the farm wake downstream of the wind farm. We now improved our wording to make clear that the hindering of the wake recovery happens in the wake downstream of the wind farm (ll. 8-10) and that the improved recovery inside the wind farm is only observed for one vertical staggering setup and is also driven by vertical momentum advection (ll. 17-18).

lines 40-45: to better explain the role of the Coriolis force I would suggest the authors to add the citations of van der Laan and Sørensen (2017) and Bastankhah et al. (2024). These studies show interesting insights on how the Coriolis force deflects the flow inside and in the wake of the wind farm, i.e. through counteracting direct and indirect effects. Mentioning these effects is important, given the nature of the manuscript, and would in my opinion improve the completeness of the introduction section.

We extended our explanation on farm wake deflection in ll. 52-59 with the two mentioned references and added also Lanzilao et al. (2025) as their simulations further clarified the topic.

line 56: I would remove the word "realistic", as the kind of CNBL that is simulated in LES is still an idealization w.r.t. what happens in real life. Alternatively, I would suggest the authors to change it to something similar to " CNBLs increase the realism of the simulations compared to truly NBL".

We changed the sentence to:
"The strong dependence of cluster wake characteristics on atmospheric conditions emphasises the need to model atmospheric conditions such as CNBLs that include the influence of veer and a capping inversion and thereby increase the realism of simulations compared to NBLs."

line 155: Please change "Earth's angular velocity Ω" to "Earth's angular velocity magnitude Ω".

Done as suggested.

line 177: please include the value of the Rayleigh damping coefficient and how it was calculated (e.g. using the methodology proposed by Lanzilao and Meyers, 2022).

Thank you for mentioning the method of Lanzilao and Meyers (2022). Such a method, that automatically determines the Rayleigh damping coefficient based on the atmospheric condition, is unfortunately not available in PALM. In PALM, the damping starts weakly at a height defined by the damping height z_{rd} and increases with a $\sin^2$-function to its maximum value f_{rdm} at the top L_z: $f_{rd}(z) = f_{rdm} \sin^2\left(0.5\pi \frac{z-z_{rd}}{L_z-z_{rd}}\right)$ (see Maas, 2023). The maximum is set by the Rayleigh damping factor, which was 0.01 in our case. We selected this factor based on Maas and Raasch (2022).

We added the Rayleigh damping factor to the paper with a reference to the study by Maas and Raasch (2022) and added additionally Klemp & Lilly (1978) and Maas (2023) for referencing the general method of Rayleigh damping and the implementation in PALM (ll. 180-184).

section 2.1 in general: it seems that the simulations conducted by the authors are quite large. From my calculation the number of cells exceeds 10 billion. If this is the case, these are among the largest wind farm simulations ever conducted. For this reason, I would appreciate more info on e.g. the number of cores used and an idea on the computational time required for both the precursor and the successor. It is important in my opinion to keep track of these parameters as well.

Thanks for the suggestion, we added a short paragraph describing the consumed computational resources. The size of the main runs is in the order of 5-6 billion cells.

line 230: eSGS cannot be calculated from the LES fields unless a prognostic equation is solved for it. Is that what the authors are doing in the PALM model?

Yes, PALM uses the Deardorff 1.5-order turbulence model, so a prognostic equation for eSGS is solved. We added this description to the methods (ll. 161-163).

lines 455-456: I disagree that "the deficit of B1 is mostly constant". I would say that, in the second wind farm, the deficit of B1 goes like S1, with the latter just exhibiting slightly higher deficit.

Agreed that the behaviour is similar for B1 and S1 and corrected the text to only address the difference in the deficit.

Figure 9 caption (and in other places, where applicable): please change "dotted" with "dashed".

Changed in caption of Fig. 9 & Fig. 6.

lines 471-472: "compared to the deficit behind...", this makes things confusing. What the authors are showing in Fig. 9a and 9b is not deficit, but rather wind speed. When the authors say that the deficit behind the second wind farm (70%-75%) is stronger than the first wind farm (80%-90%), they should either show the complementary percentages, i.e. 30%-25% vs 20%-10%, or directly refer to the wind speed.

To avoid confusion, we now added explicitly that it is "70-75% of the inflow wind speed" and "80-90% of the inflow wind speed".

Figure 13(a and d): this is just a suggestion, but I believe that the legends should be S1-B1/S3-B1 and S1-B2/S3-B2, respectively (following the rest of Figure 13), as it took me some time to understand what I was looking at.

Good point, we adapted the figure legend accordingly.

lines 524-526: the sentence "Both vertical staggering setups show throughout the wind farm an increasing power production relative to B2, where the second half of the wind farm has on average a larger power production than B2" is not very clear to me.

We now tried to improve the clarity by changing the sentence to: “For both vertical staggering setups the relative turbine power with respect to B2 increases from the first row to the last row of WF1, where the second half of WF1 has on average a larger power production than B2”.

line 644: it is not clear if the authors did or did not repeat these simulations. Please clarify.

Yes, we performed those simulations. It is now clarified in text: “For comparison, we repeated our simulation S1…”.

Section 4.3: this is just a suggestion, but I struggled a bit to relate North with left and South with right at the beginning. Please consider making this more explicit for better readability.

We specified it now the following way, similar to the description in the results section: “For our westerly flow on the northern hemisphere, the circulation zone appears at the left side of the wake when viewed in streamwise direction”.

line 767: could the stronger speedup in the staggered cases be explained simply because the effective height of the wind farm w.r.t the ABL is higher in these cases? Adding this also here would make things clearer in my opinion.

We assume that the reviewer refers to the possible effect that a wind farm with a larger height with respect to the ABL height might promote stronger flow deflection to the sides, as the deflection to the top is hindered by the capping inversion. While this could indeed be the case, we would expect to see a respective speed-up already directly next to the wind farm. However, the speed-up at the side of the wake that we refer to mainly develops after 10-20 km downstream of the wind farm, correlating with the onset of the northern circulation zone. The relation between the speed-up and the circulation is corroborated by the momentum budget (Fig. 6) that show the increase in streamwise momentum to be balanced by the cross-stream momentum advection at lower hub height driven by the circulation.

Appendix A2: please consider adding (a small) explanatory text to this section. I am personally against sections containing only figures.

Following the comments by both referees, appendix A2 now also includes a description of the TKE results.