[wes-2024-79] Authors' response to Referee #1

We thank the referee for providing very positive and constructive comments on our manuscript. In the following we provide our response to each of the points raised by the referee (with changes to the manuscript highlighted in red).

Major Points:

1. Section 3 describes the LES setup very briefly and refers to Lanzilao & Meyers [JFM, v. 979, 2024] for more details. However, there are a few additional details that should be part of this paper itself to make it self-contained. For example, please mention the surface roughness, Coriolis frequency, the driving force (presumably it is a geostrophic wind) and the upstream fetch. Also mention what are the additional 5 simulations performed here.

A. We have added the following details in Section 3 of our revised manuscript:

The simulations are performed with SP-Wind, an in-house LES code developed at KU Leuven (Allaerts and Meyers 2017, Lanzilao and Meyers 2023a). The streamwise (x) and spanwise (y) directions are discretized with a Fourier pseudo-spectral method. For the vertical dimension (z), an energy-preserving fourth-order finite difference scheme is adopted (Verstappen and Veldman 2003). The effects of subgrid-scale motions on the resolved flow are taken into account with the stability-dependent Smagorinsky model proposed by Stevens, Moeng and Sullivan (2000) with Smagorinsky coefficient set to Cs = 0.14. The constant Cs is damped near the wall by using the damping function proposed by Mason and Thomson (1992).

To break the streamwise periodicity and impose an inflow condition, we use the wave-free fringe region technique (Lanzilao & Meyers 2023a). At the top of the domain, a rigid-lid condition is used, which implies zero shear stress and vertical velocity and a fixed potential temperature. To minimize gravity-wave reflection, we adopt a Rayleigh damping layer in the upper part of the domain.

In this study we fix the geostrophic wind to 10 m s^{-1}, which is in line with previous studies (Abkar and Porté-Agel 2013; Wu and Porté-Agel 2017; Allaerts and Meyers 2017, 2018; Lanzilao and Meyers 2022). This value is also chosen so that all turbines operate below their rated wind speed, justifying the use of constant thrust coefficient noted earlier. Finally, we fix the Coriolis frequency to $f_c = 1.14 \times 10^{-4}$ s^{-1}, and the surface roughness to $z_0 = 1 \times 10^{-4}$ m for all simulations.

2. Lines 170 – 180: Fig. 5 plots the wake efficiency against the farm-averaged yaw angle and shows that there is a weak correlation between them. I think it is inappropriate to take an average of the yaw angles across all turbines in a wind farm. This is because all turbines do not yaw in the same direction, i.e. some yaw clockwise and others yaw anticlockwise, as seen in Fig. 7 of Lanzilao & Meyers [JFM, v. 979, 2024]. Thus, farm-averaged power and farm-averaged turbine yaw angles are likely never going to be correlated. Perhaps it would be better to check some measure of power of each turbine against the individual yaw angles across all the LES cases (no. of data points would be 38 cases times the number of turbines in each case) to arrive at a conclusion regarding whether effective turbine layout is correlated with the wind farm performance.

A. We agree with the referee that all turbines do not yaw in the same direction and therefore it would be inappropriate to take an average of the yaw angles (e.g., positive and negative yaw angles would be cancelled out). However, what we have plotted in Fig. 5 is the farm-averaged "magnitude" of the yaw angles, and therefore we think that this does give a good indication as to the degree of turbine yawing within the farm.

3. Lines 195 – 200: Figs. 8 and 9(b) show that a lower wake efficiency is obtained for higher k* values. Is the initial wake width (ε) almost the same across the turbines? It is possible that between two wind farms, the wake growth rate (k*) is larger but the total wake width (k*x + ε) is actually smaller, and hence the wake efficiency is smaller. Do the authors ensure that this does not happen in their LES results?

A. We have confirmed that the total wake width also correlates negatively with the wake efficiency (in a similar manner to how the wake growth rate k* does). We have added a new figure 9(c) to show this trend, and we have also added the following sentence:

This trend can also be confirmed from the negative correlation between η_w and the farm-averaged turbine wake width (at 10D downstream of each disc) shown in Fig. 9(c).

4. In the algorithm shown in Fig. 13, β can be calculated directly from the LES (from velocities U_F and U_{F0}). This is used to calculate M_{LES} and then ζ_{LES}. Then another β is calculated in Step 3. The existence of two values of β is confusing. Is an iterative procedure used, i.e. Steps 1, 2, 3 are repeated until convergence? If not, how different are the values of β and β_{LES}? What is the meaning of two different β values? Why not use β_{LES} directly in Step 4?

A. The aim of Step 3 is obtain β for the "near-ideal" (hypothetical) wind farm subjected to a given ζ_{LES}. Therefore, the value of β (obtained from Step 3) is different from β_{LES}, and the value of β (not β_{LES}) should be used in Step 4 to calculate the farm-scale efficiency (which is the efficiency of the "near-ideal" farm, not the actual farm simulated in the LES). To make this point clearer, we have added the following sentences to the caption of Fig. 13:

Note that C_T^* required in Step 3 is not $C_{T,LES}^*$ in Fig. 11 but the theoretical C_T^* given by Eq. (4). This is because the aim here is to obtain β for the 'near-ideal' (hypothetical) wind farm subjected to a given wind extractability factor ζ_{LES} (obtained from LES using Steps 1 and 2).

These Steps 1 to 3 do not require any iterative procedure, since the equation solved in Step 3 is a quadratic equation for β, which can be solved analytically.

5. Lines 280 – 285: C_T* values shown in Fig. 11 are not 0.974. What is the justification for using this value for C_T* in Section 4.3? It does not appear to be adjusted upwards when compared to Fig. 11.

A. As explained in our response to the previous point, the value of C_T^* used in Step 3 is not $C_{T,LES}^*$ but the theoretical value from Eq. (4). To make this point clearer, we have changed "we used 0.974" to "we used $0.88/N^2$ = 0.974" in our revised manuscript.

6. Lines 275 – 285: The multiplication by the correction factor N or its powers following Shapiro et al. (2019) seems to be an ad-hoc fix. Are the results of the analytical model sensitive to this ad-hoc fix? I wonder if it is possible to conduct one simulation where these corrections are incorporated and check whether an ad-hoc fix is no longer needed?

A. Essentially, the analytical model is not dependent on the correction factor N, since Eq. 20 does not require any information from the wind farm LES results as an input to calculate ζ. The reason why we apply the correction factor N in Step 1 in Fig. 19 is that, in order to make a fair comparison between the analytical model predictions and the farm LES, we need to account for the fact that the actual turbine thrust in the LES is slightly higher than it should be. To make this point clear, we have added the above explanation to the caption of Fig. 19. We agree that it would have been better (less confusing) if we had adopted the correction factor N in the simulations rather than in this post-processing step, but unfortunately these simulations are computationally expensive and we are unable to run additional simulations in a timely manner.

Minor Points:

1. Section 2: It would help to know under what conditions (if any), $C_{P,\ Nishino}$ reduces to C_P, i.e. Eq. (6) reduces to eq. (7).

A. Thank you for suggesting this. We have added the following sentence after Eq. (7):

Note that Eq. (6) reduces to Eq. (7) in two special cases: (i) when λ/C_{f0} = 0 and (ii) when ζ is infinitely large.

2. Lines 215 – 225: The last paragraph on pg. 12 and first paragraph on pg. 13 refer to Eqs. (11), and (12a), (12b), (12c). However, these equations are written after the text, which is usually not done. Please reorder the text and the equations and reword appropriately.

A. Thank you for pointing this out. We have made these changes now.

3. Are the intermediate quantities, such as T_i, U_F, U_{F0}, $C^*_{T,\ LES}$, needed to compute M_{LES} and β_{LES}, provided in the dataset? It would be very helpful for other researchers to have access to these quantities for all the LES cases.

A. Yes, these data are available in our GitHub repository.

4. In Eq. (20), is τ_{t0}/τ_{w0} obtained from the precursor LES? That seems to be the only parameter that responds to the atmospheric conditions and is the key that leads to different wake efficiencies. It would be instructive to show this value for the three cases in Fig. 20.

A. We thank the referee for this suggestion, but in our revised manuscript we have removed the original Fig. 20(a) and instead added a new Fig. 20(b) to show the results for all 29 cases instead of the 3 selected cases, following the other referee's suggestion. We believe that this new Fig. 20 is more informative than the original Fig. 20.