[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.

[wes-2024-79] Authors' response to Referee #2

We thank the referee for providing detailed comments on our manuscript. Many of the comments have helped us understand which part of the manuscript should have been explained better. In the following we provide our response to each comment one by one (with changes to the manuscript highlighted in red) but first of all, we would like to highlight 4 major points which the referee seems to have overlooked or misunderstood:

1. The referee states that the two-scale momentum model proposed by Nishino and Dunstan (hereafter referred to as ND20) is "like the equivalent roughness model proposed by Frandsen". However, a key difference is that the equivalent roughness models (also known as 'top-down' models) are for infinitely large wind farms, whereas the ND20 model is for a finite-sized wind farm, i.e., the ND20 model accounts for the effect of wind farm size (as the wind extractability factor ζ

depends on the farm size). To make this point clearer, we have added the following short paragraph on page 3 of our revised manuscript:

Note that the derivation of Eq. (1) given by Nishino and Dunstan (2020) was for an idealised case where the flow through the farm was assumed to be fully developed. However, they also discussed (in Section 3 of their paper) how the same form of equation could be derived for more general cases, where the net momentum transfer through the side and top surfaces of the farm control volume should also be considered as part of M. See Kirby et al. (2022) for the full expression of M.

2. The referee mentions "average layout" several times in their comments, and claims that the turbine-scale and farm-scale efficiencies (η_{TS} and η_{FS}) do not have as much "physical importance" as implied in our manuscript, because the ND20 model (from which η_{TS} and η_{FS} have been derived) is specifically for this "average layout". The question here is whether this "average layout" has any significant meaning in terms of physics, and we believe it does. In our previous LES study (Kirby et al. 2022) we have tested 50 different turbine layouts and showed that most of them give a lower farm-average C_P than the ND20 model prediction (i.e., η_{TS} < 1). It is true that some layouts could exceed the ND20 prediction but only slightly (see, e.g., Fig. 12 of Kirby et al. 2022), indicating that the ND20 model does capture important physics. To stress this point, we have added the following sentence in the first paragraph of Section 4.3:

Kirby et al. (2022) have shown, using LES of flow over a periodic array of actuator discs for 50 different layouts, that the `near-ideal' farm performance predicted by Eq. (6) is a good measure to differentiate the turbine-scale power losses from the farm-scale power losses.

After the referee's comments, we have realised that the term "ideal farm" (used in our original manuscript) could mean "best farm", which would be misleading in this case, so we have changed "ideal farm" and "ideal power coefficient" to "near-ideal farm" and "near-ideal power coefficient", respectively, in our revised manuscript. We have also added the following sentence on page 4:

We describe this as 'near-ideal' since this is close to but slightly less than the maximum possible (as shown later).

3. The referee mentions "η_{FS} could be less than 1 even in a non-atmospheric flow as a consequence of wakes". Although the meaning of "non-atmospheric flow" seems a little unclear, we believe that the referee misunderstands our concept of farm-atmosphere interaction here. For example, if we consider a hypothetical scenario where the wind farm is placed in a rectangular channel of height H_F (or consider that a capping inversion layer exists at $z = H_F$ to act as a rigid lid, and the wind is forced to go through the nominal farm layer of height H_F) then we would have $\beta = 1$ and thus $\eta_{FS} = 1$. The point here is that, when each turbine generates its wake, the flow bypassing the turbine must accelerate (due to the conservation of mass at each turbine scale). This means that the generation of turbine wake does not, on its own, cause any reduction of "farm-average" wind speed, and hence, farm-atmosphere interactions are required for the "farm-average" wind speed to decrease (or for the values of β and η_{FS} to decrease from 1). To explain this point explicitly, we have added the following sentences in the first paragraph of Section 4.3:

Note that, when each turbine in a wind farm generates its wake, the flow bypassing the turbine locally accelerates due to the conservation of mass (at each turbine scale); hence, we consider that any reduction of farm-average wind speed is caused by external (farm-atmosphere) interactions. This means that the power losses accompanied by a reduction of farm-average wind speed are `farm-scale' power losses (caused by external interactions) and not `turbine-scale' power losses (caused by internal interactions).

4. The referee mentions that $\eta_{TS} > 1$ is interpreted as "turbine performing better than an isolated one but in a farm". This interpretation is clearly incorrect (or imperfect). The correct interpretation of $\eta_{TS} > 1$ is that the turbines in a farm (with the farm-average wind speed of U_F) are performing better than how they would perform when they are isolated and their incoming wind speed is U_F. To make this point clearer, we have modified the relevant sentence on page 18 as follows:

Note that η_{TS} is slightly greater than 1, which means that these `clustered' turbines perform slightly better than the isolated ideal turbines (of the same size) that have the same upstream wind speed as the farm-averaged wind speed U_F

Based on the above 4 points, we disagree with the referee's comments that η_{TS} is "simply a correction factor for the equivalent-roughness model" and that "the scale separation is more a convenient modeling tool rather than the result of different physics playing out". We hope that the above changes made in our revised manuscript will have resolved the referee's concerns.

Response to specific comments:

L22: "Measurements" sound too generic, the cited papers refer to operational turbine data. Indeed, there is vast literature of wake observation through remote sensing (e.g. [2,3,4]) that it is worth mentioning.

A. We thank the referee for suggesting these papers. We have added two of them which we found most relevant to our context.

L31: the fact that there are velocities reduction within the farm "in addition" to wake is quite philosophical. Apart from pressure-induced effects (like blockage, channelizations, speedups), one could argue that all the momentum deficit in the ABL is the result of superposed wakes. Also, the internal boundary layer growth can be seen as merging and vertically expanding wakes. It should be made clearer that the distinction between the "wakes" and the "farm effects" is merely based on the spatio-temporal scales considered and not due to intrinsically different physics.

A. This comment is related to the major point "3" discussed above, and we hope that our answers provided there have explained about our concepts sufficiently.

L47: I suggest revisiting the word "validate" when referring to the two-scale hypothesis. "Assess", "test" sound more appropriate and less definitive.

A. We believe that it is appropriate to use the word "validate" in some sentences where it does not imply that the two-scale separation assumption has been fully validated. However, following the referee's suggestion, we have changed "validated" to "evaluated" in two sentences where "validated" could sound a little too definitive (on pages 13 and 25 in the revised manuscript).

L63: τ_w may have not been defined.

A. Thank you. We have added the definition now.

Eq. 3: Nishino and Dunstan also have a σ_1 factor in their Cp equation, please justify $\sigma_1 \sim 1$ used here.

A. Thank you for pointing this out. We have added the following explanation on page 3 where we define the farm-layer height H_F (since this σ_1 factor is essentially the factor required when U_{F0} differs from U_{T0}):

The exact value of H_F defined originally by Nishino and Dunstan (2020) depends on the undisturbed wind profile, to ensure that U_{F0} matches exactly with the undisturbed wind speed averaged over the turbine swept area, U_{T0}; however, as shown later by Kirby et al. (2022) the fixed definition of $H_F = 2.5H_{hub}$ is a good approximation for a wide range of ABL profiles.

L82: “upper limit” with respect to which independent variable? Is the maximum Ct attainable by changing the induction of the turbines (like Betz’s theory)?

A. This “upper limit” is with respect to the turbine layout (for a fixed value of C_T'). We have added “with respect to the turbine layout” in our revised manuscript.

L85: C_T' should have an i index but it does not. If as stated later it is assumed constant, it is a good point to state it (e.g. “the i-index is dropped because we assume [$\cdots$]”)

A. Yes, C_T' is assumed constant (C_T' = 1.94 in our LES as noted later in Section 3). Now we have stated “assumed to be constant for all turbines in the farm” in the sentence right after Eq. (4).

Eq. 4: please explain α right after the equation.

A. We have decided to remove α from Eq. (4) in our revised manuscript, since the aim of this equation is to give the (modelled) relationship between C_T^* and C_T'.

L91: is the thrust or thrust coefficient that needs to be uniform across the farm?

A. We have revised the paragraph right before Eq. (5) to explain this point better. It is the turbine resistance coefficient C_T' that is assumed to be uniform across the farm.

Eq 5: please define explicitly Cp. Is it the average power over the farm divided by an available kinetic energy? Is it the average of the individual Cp? Or something else?

A. The definition of C_P has already been given at the end of Section 2.1. It is the “farm-averaged” power coefficient as noted in the sentence right before the equation.

Fig. 13: why do you use a β_{LES} (presumably equal to the velocity ratio U_0/U_{F0}) and then a β from Eq. 1 again? I understand that the first two steps are needed to estimate the ζ which is the only unknown of the model. However, there should be information on, for instance, how close the β_{LES} is from the β, which can be an indication of the physical soundness of Nishino’s model based on control volume analysis vs LES.

A. Here the referee seems to have misunderstood the concept of our analysis summarised in Fig. 13. The aim of our analysis here is obtain β for the “near-ideal” (hypothetical) wind farm subjected to a given ζ_{LES}. This means that comparing β (obtained from Step 3) against β_{LES} will not give an indication of the physical soundness of the theoretical model (because the value of β for the “near-ideal” farm should be different from the value of β_{LES} for a real farm). 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).

It should also be noted that the soundness of the two-scale approach has already been evaluated in Section 4.2. The focus of Section 4.3 is on its application (rather than its evaluation).

Fig. 14: The interpretation of these results it is not very compelling. Here we are comparing farms with the same layout, same capping inversion heights and free atmosphere lapse rate, but different capping inversion strengths (i.e. different blockages and momentum entrainment). These are my take aways:

•When using η_w, η_{nl}, results are not really meaningful because they are based on the assumption that the first row is representative of isolated turbine power, which breaks down in case of blockage.

•η_{TS} is capturing most of the energy losses due to blockage and also wakes (which are local effects), but in an average sense and thus not connected to the farm layout. In other words, $C_{P,Nishino}$ is the efficiency of the farm (including wakes!) but for all possible layouts. Calling this "farm-atmosphere interaction losses" is misleading. , $C_{P,Nishino}$ would be less than 1 even in a non-stratified, uniform inflow, just because of wakes. The fact that $C_{P,Nishino} \sim C_P / \ C_{P,Betz}$ simply means that the layout considered happens to have losses similar to the average layout adopted by Nishino.

•η_{TS} is only a small correction that accounts for local layout effects not considered in the global Nishino model. I don't agree that this means that the "turbines perform better than if they were isolated" It simply means to me that this particular layout has slightly lower losses than the average layout considered by Nishino.

A. We believe that our response to the 4 major points (provided at the beginning of this response letter) have sufficiently addressed all these points.

Fig 16.: I would make this figure bigger, as it is arguably the most important. It shows that the η_{TS} capture changes in the layout (which is evident) and should show that η_{FS} should track the changes in efficiency due to stability. The latter is not very clear since values are similar across different capping inversion heights. I suggest adding the number of not of each bar.

A. We thank the referee for this suggestion. We have made Fig. 16(b) bigger and also added the values of η_{FS} (above each orange bar) to show more clearly that η_{FS} does change with atmospheric conditions. We have also decided to remove Fig. 16(a) since this figure was not discussed in the main text.

L 326: The conclusion that flow confinement is causing the η_{TS}>1 are not supported by specific evidence here. The local-scale efficiency larger than 1 simply means that the turbines do better than those in an average layout. The average layout can be interpreted as an infinitely large fetch of rough elements exerting the same thrust as the turbines over a unit area. η_{TS} will be greater or lower than one for every departure form this idealized average layout. If it is flow confinement or other effects, it was not shown.

A. We disagree with the referee's interpretation of $\eta_{TS} > 1$ as we explained through the 4 major points described earlier. However, we agree with the referee that, in this paper, we were unable to provide a clear evidence of flow confinement effects causing $\eta_{TS} > 1$ (although the results for the "double spacing" case shown in Fig. 18, together with the LES results of Ouro and Nishino (2021) cited, suggest that such flow confinement effects are likely the cause of $\eta_{TS} > 1$). We have therefore made some minor changes of wording in our revised manuscript.

Section 4.4.: the error analysis of the analytical model could be made more comprehensive. A linear regression between all the farm efficiencies from LES and model with error metrics (e.g., $R2$) should be shown instead of only the overall error (Fig. 20b)

A. We thank the referee for this suggestion. Now we have added a new Fig. 20b to show the relationship between the LES results and analytical model predictions of η_{FS} for all 29 cases, together with the R^2 value. We have also removed the original Fig. 20a since this figure was only for 3 selected cases and it was not very informative.

We thank the referee again for all these comments, which have helped us improve the manuscript significantly.