Response to Referee #1: Dries Allaerts

Authors' responses to reviewer comments appear in blue text. Line numbers referenced in the authors' responses refer to the revised document. Figures with Arabic numerals (e.g., Figure 10) correspond to the revised manuscript, Figures with roman numerals (e.g., Figure iv) only appear on the response to reviewer's comments.

The paper uses two WRF-LES simulations of a large, generic wind farm to investigate how and why wind-farm blockage varies with surface layer stability. The underlying physical mechanisms are explored based on a detailed analysis of the streamwise momentum budget components. Interestingly, the paper shows that the adverse pressure gradient upstream of a front-row turbine is nearly identical to the pressure gradient upstream of a standalone turbine, and the difference between single-turbine induction and wind-farm blockage stems from the vertical momentum advection. The paper is well-written and has a clear structure. I appreciate that the paper has one clear research objective, and accordingly the analysis of the wind farm simulations focuses solely on the upstream flow behaviour in order to address the research question. I am a bit puzzled by the claim that this paper investigates blockage in the absence of gravity waves, and I do have some related questions about the numerical setup. Please find below a list of comments and suggestions.

We thank the reviewer for providing thoughtful comments that helped improve our manuscript.

Main comments

1. One line 66, the authors claim that they investigate wind-farm blockage in the absence of gravity waves. How exactly do you ensure that there are no gravity waves in your simulation? I think this is quite a significant assumption and should therefore be discussed in more detail.

Thank you for highlighting this because it is an area of active research. We clarify throughout the entire manuscript that we investigate blockage with minimal upstream propagation of gravity waves (this was also requested by another reviewer) and direct the reader to the appendix as follows:

Line 68: "Here, we investigate how atmospheric stability modifies upstream blockage with minimal upstream propagation of gravity waves (see Appendix C for a discussion on gravity waves in our domain)"

We also clarify the discussion on gravity waves (see Main Comment #8 below) and include the energy associated with wave reflection from the model top (see Main Comment #4 below) as follows:

Line 483: "Spurious waves can sometimes modify the correlation between atmospheric variables in upstream-propagating gravity waves (Lanzilao and Meyers, 2022). Because the only potential source of gravity waves in our simulations is in the boundary layer (i.e., the wind plant), then waves with a downward group velocity (positive phase speed) and outside the boundary layer must be due to spurious reflections (Taylor and Sarkar, 2007). We quantify wave reflection following the methodology outlined in Taylor and Sarkar (2007). We find that 7.1% and

5.8% of the total vertical kinetic energy $0.5w'^2$ is associated with downward energy propagation for the weak and moderate stability cases, respectively, which is comparable to the wave reflection reported in other studies (Taylor and Sarkar, 2007; Allaerts and Meyers, 2018, 2017)."

2. The LES uses a two-domain configuration with one-way nesting. Can you give more details about the domain nesting? How large is the outer domain compared to the inner domain? What boundary conditions are imposed on the inner domain? If this is an inflow-outflow type domain, is there a transitional period to impose (blend) the inflow wind speed? What is the outlet boundary condition (simple outlet condition or again blending towards the parent solution)?

We add clarification about the size of the parent domain and one-way nesting in the manuscript as follows:

Line 88: "We use a two-domain configuration with flat terrain to evaluate the blockage effect from wind plants. A periodic LES domain provides the boundary conditions for a nested LES domain via one-way nesting (i.e., atmospheric conditions for the outermost grid cells in the nested domain are specified from the parent domain)."

Line 96: "The parent domain is 10 grid points larger than the nest in the horizontal directions."

We investigate the effect from prescribed boundary conditions by considering the velocity field close to the domain boundaries. We find that boundary conditions have minimal effects on the flow close to the wind plant (Figure i). The velocity field in the induction region asymptotes to the velocity at the upstream end of the domain. The velocity at x = -30D in the nested domain is virtually the same to the velocity of the parent domain (vertical black line in Figure i). At the downstream end of the domain, the wake recovery only appears to be influenced by the boundary conditions very close to the domain boundary.

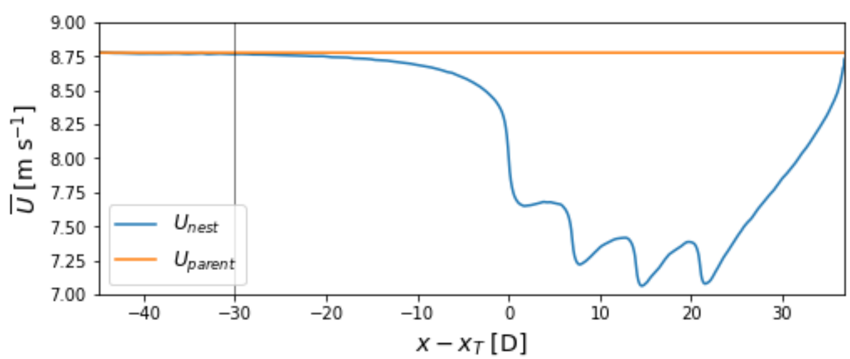

Figure i: Time-averaged velocity field for the parent and nested domain. The x-axis is re-scaled to locate 0 at the first turbine row.

Wake recovery varies for each turbine row (Figure ii). Wake recovery is faster for the rows in the trailing edge of the wind plant. The change in wake recovery between third and fourth rows is comparable to the change in wake recovery between the second and third rows. Furthermore, the velocity field only displays a sudden change very close to the domain outflow (Row 4). The horizontal velocity rapidly increases 15D downstream of the last row of the wind farm and the domain boundary is 15.7D downstream of the last turbine row.

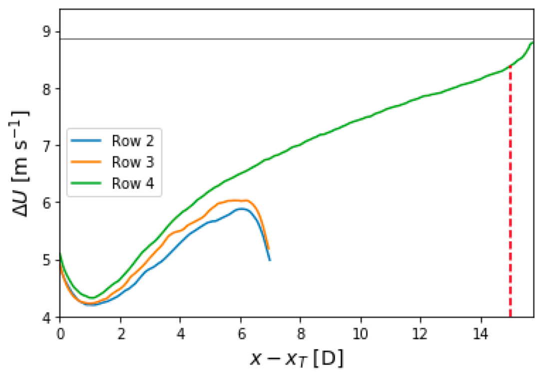

Figure ii: Wake recovery downstream of each turbine row of the wind plant. The dashed red line illustrates the distance downstream of the last turbine row where the velocity is likely influenced by the boundary conditions.

Because one-way nesting is regularly used for boundary-layer simulations of wind turbines (Mirocha et al., 2014; Aitken et al., 2014; Arthur et al., 2020; Sanchez Gomez et al., 2022; Wise et al., 2022) and because the velocity field is only minimally affected by boundary conditions close to the domain boundaries, we decide not to include this detailed information in the manuscript. We rather add clarification to the text as follows:

Line 107: “As will be shown later in the manuscript, the velocity deceleration in the induction region is virtually zero 30D upstream of the wind plant. Therefore, 45D of fetch upstream of the wind plant is deemed sufficient to investigate the induction region of the turbine array.”

Furthermore, we adjust the x-axis in Figure 8 to show the velocity deceleration far upstream as follows:

Line 183: “The velocity deceleration asymptotes to zero far upstream (x<-30D) for both atmospheric conditions (Figure 8).”

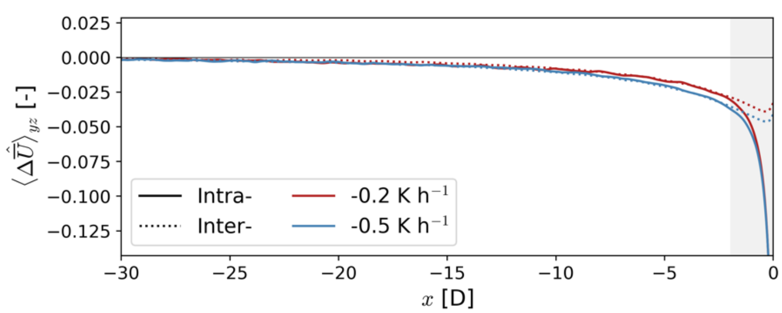

Figure 8: Normalized velocity deficit $\left(\Delta U = \frac{\overline{U} - \overline{U}_\infty}{\overline{U}_\infty}\right)$ for the inter- and intra-turbine regions upstream of the wind plant for each atmospheric condition…

3. How is the grid resolution chosen? The authors mention that a finer grid is used in the more stable case, but how did you determine that the employed grid resolution is sufficient?

Because another reviewer also asked about grid resolution, we now include an additional appendix in our manuscript:

Line 493: Appendix D

"Grid resolution in our simulations is sufficient to resolve most turbulence kinetic energy across the turbine rotor layer (Figure D1). For the non-linear backscatter and anisotropy subgrid-scale turbulence model (Kosović, 1997), the total turbulence kinetic energy $\overline{k}_{tot}$ is given as $\overline{k}_{tot} = \frac{1}{2}\left(\overline{u_i'u_i'} + m_{ii}\right) + \overline{k}_{SGS}$, where $\overline{u_i'u_i'}$ represents the resolved TKE, m_{ii} are the normal subgrid-scale stress components, and $\overline{k}_{SGS}$, is the subgrid-scale TKE. Nearly 80% of TKE in the turbine rotor layer is resolved by the numerical grid for both simulations (Figure D1). Because less than 80% of TKE in the lower rotor layer is resolved in the weak stability case ($\overline{k}_{res}/\overline{k}_{tot}$= 0.78 at z = 30 m), a finer grid is used for the simulation of moderately stably stratified flow."

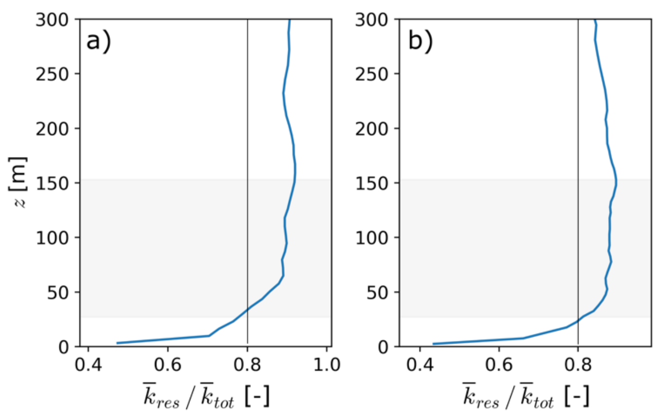

Figure D1: Fraction of resolved TKE in the surface layer for the weak (a) and moderate (b) stability cases. The solid, black line corresponds to 80% of resolved TKE. The grey shaded area corresponds to the turbine rotor layer.

4. An implicit Rayleigh damping layer of 1000 m is used to avoid wave reflection. How do you know this leads to sufficient damping? Did you check for wave reflections? Other LES studies typically use Rayleigh damping layers of 10 km or more (see, e.g., work by Allaerts and Meyers, or Lanzilao and Meyers), so 1000 m seems quite small to me.

We quantify the energy reflected from the domain top following the methodology from Taylor and Sarkar (2007). Because the only potential source of gravity waves is in the boundary layer, then waves with a downward group velocity (positive phase speed) and outside the boundary layer must be due to spurious reflections (Taylor and Sarkar, 2007).

We quantify wave reflection by observing the w' field in a frame moving with the geostrophic wind, as in Taylor and Sarkar (2007). Then, we transform w' into the frequency and wavenumber domain. We decompose the spectrum into upward and downward propagating waves for vertical levels in between the top of the inversion layer and the bottom of the damping layer. Finally, an inverse Fourier transform yields the variable w' in physical space for internal waves with downward energy propagation. We find that 7.1% and 5.8% of the total vertical kinetic energy $\left(\frac{1}{2}w'^2\right)$ is associated with downward energy propagation for the -0.2 K/h and -0.5 K/h simulations, which is comparable to the wave reflection seen in other studies (Taylor and Sarkar, 2007; Allaerts and Meyers, 2017, 2018).

We include the relevant information in Appendix C, as reported in Main Comment #1.

5. How does the power of the entire wind farm vary with stability? I imagine this will also affect the amount of blockage.

Thank you for your suggestion, we expand our analysis on turbine power as follows:

Line 195: "Even though the wind speed slowdown from blockage is small, front-row turbines in the wind plant produce on average 5.2% less power than a stand-alone turbine (Figure 10). Because winds are slightly faster in the moderate stability case compared to the weak stability case, turbine power is also expected to differ. As a result, we evaluate the difference in power production between the turbines in the wind plant and a stand-alone turbine for the same atmospheric conditions. Just as the velocity deceleration is modified with atmospheric stability, turbine underperformance is more severe in the moderate stability case compared to the weak stability case. Whereas turbines in the first row produce on average 4% less power than a stand-alone turbine for the weak stability condition, front-row turbines produce on average 6.5% less power than a stand-alone in the moderate stability case. Downstream of the first row of the wind plant, turbine power is primarily dominated by the evolution of the wake. Turbine wakes persist longer in stable boundary layers because of reduced turbulence mixing (Dörenkämper et al., 2015; Lee and Lundquist, 2017), so we expect downstream turbines to produce less power in the moderate stability case compared to the weak stability case."

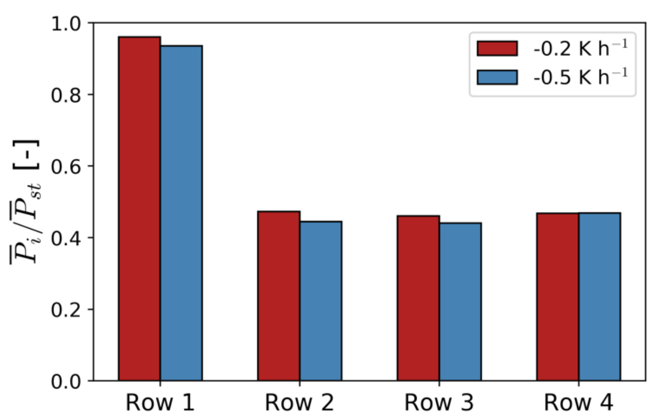

Figure 10: Normalized turbine power for each row of the wind plant and each atmospheric condition. The mean turbine power for the i-th row of the wind plant $\overline{P_i}$ is normalized over the mean turbine power of a stand-alone turbine $\overline{P}_{st}$.

We also want to emphasize that the momentum fluxes presented in our manuscript are normalized to account for discrepancies in inflow conditions. We update the manuscript as follows:

Line 230: “The u-momentum flux at the inflow of the control volume $\mathcal{V}$ in Figure 11 is larger in the moderate stability case compared to the weak stability case due to slightly faster hub-height winds. Consequently, the magnitude of the momentum fluxes and turbine power is expected to be larger in the moderate stability case as well. To contrast the momentum balance between different atmospheric stability conditions and turbine array sizes, we normalize the forcing terms in Eq. 2 using the momentum flux at the inflow of the control volume far upstream $(\rho\overline{u}_{\infty}\overline{u}_{\infty}S_x)$ for each stability case.”

6. Why do you call the net-upward vertical motion in the region upstream of the wind farm a secondary circulation? I don’t fully understand why you see this as a circulation (any similarities with other flow scenarios?). Note that this upward flow displacement has been noted by others in the past (see, e.g., Allaerts and Meyers 2017).

We replace “secondary circulation” with “secondary flow feature” over the entire manuscript, as suggested by the reviewer. As an example, line 293 now reads as: “Mass balance indicates the slowdown of the u-velocity in the turbine rotor layer $(\Delta(\rho\overline{u}S_x) < 0)$ is balanced by the development of a secondary flow feature in the form of net-upwards vertical motion $(\Delta(\rho\overline{w}S_z) > 0)$ for both stability conditions (i.e., $\Delta(\rho\overline{u}S_x) + \Delta(\rho\overline{w}S_z) \approx 0$).”

We also comment on findings from other studies than mention the vertical deflection of the flow:

Line 345: “Other simulation studies have also noted this vertical deflection of the flow (e.g., Wu and Porté-Agel, 2017; Allaerts and Meyers, 2017).”

7. Line 265 ”Larger vertical shear of the horizontal velocity in the moderate stability case contributes to the increased vertical momentum advection compared to the weak stability case.” I think this is the most important finding of the paper, but it is not entirely clear to me how vertical shear affects vertical momentum advection. Can you please elaborate?

Thank you for this comment. Indeed, this is one of the most important findings. We added clarification in the manuscript as follows:

Line 300: “The vertical velocity advects horizontal momentum out of the turbine rotor layer (Figure 19). Vertical advection of horizontal momentum is 20% larger in the moderate stability case compared to the weak stability upstream of the first turbine row (Figure 19b). Larger vertical shear of the horizontal velocity in the moderate stability case compared to the weak stability case is the primary cause for the increased vertical advection of horizontal momentum. Shear $\left(\frac{\Delta\overline{u}}{\Delta z} = \frac{\overline{u}_t - \overline{u}_b}{D}\right)$ between the bottom $\overline{u}_b$ and top $\overline{u}_t$ of the turbine rotor layer is 43.6% larger in the -0.5 K/h simulation compared to the -0.2 K/h simulation. Similarly, the vertical velocity in the turbine rotor layer is 20% larger in the moderate stability case than in the weak stability case between x = -6D and x = 0D. The vertical velocity is expected to be larger in the moderate stability case because, as shown in Figure 18, the streamwise slowdown of the flow is transformed almost entirely into vertical motions. As a result, advection of horizontal momentum by the vertical velocity $\Delta(\rho\overline{u}\,\overline{w}\,S_z) = \rho S_z(\overline{u}_t\overline{w}_t - \overline{u}_b\overline{w}_b)$ is amplified.”

8. Appendix C: It is an interesting approach to assess the presence of gravity waves by means of the phase shift between pressure and vertical velocity signals (note that it is not clear at which height the signals are obtained, or are they averaged over heights?).

However, I'm not entirely sure whether these phase relations still hold when you have wave reflections. When there are wave reflections, these lead to standing wave patterns and I can imagine that for those cases the phase relationships change. Did you look at vertical cross-sections of pressure and vertical velocity throughout the entire numerical domain?

You bring up an interesting point about spurious gravity waves, which have been shown to distort the pressure and vertical velocity fields in (Lanzilao and Meyers, 2022). However, as we point out in Main Comment #4, we have marginal energy being reflected from the model top and therefore we do not expect the phase shift to be driven by spurious waves. Furthermore, as shown by Lanzilao and Meyers (2022), spurious gravity waves also cause artificial oscillations to the pressure and vertical velocity, which are not present in our simulations.

We examined the vertical cross-sections of the pressure and vertical velocity over the entire domain (Figure iii), and we observe the same trend: the pressure and vertical velocity are out of phase. For example, the maximum in vertical velocity occurs at the location of the first turbine row (x = 0D), whereas the perturbation pressure at x=0D is zero. However, we decided to include the line plot because it distinctively shows the maxima/minima for each variable and where each variable intersects $y = 0$. By clearly showing this behavior, the line plots visibly depict the phase shift between the atmospheric variables. The line plot also illustrates that we do not have spurious waves, which can cause artificial oscillations in the pressure field at the height of the inversion layer (Lanzilao and Meyers, 2022).

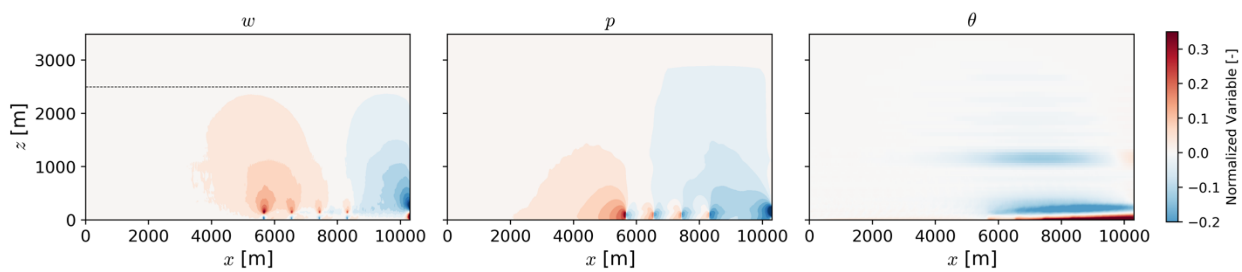

Figure iii. Vertical cross section of the time-averaged vertical velocity (left), pressure perturbation (middle) and potential temperature (right) fields for the -0.5 K/h simulation. Each variable is normalized and therefore non-dimensional. The dashed horizontal line in the left panel shows the height of the damping layer.

Thank you for pointing out that we do not include the height for the data shown in the line plots. We added clarification in the caption of Figure C1 in the manuscript and updated the Appendix C as follows:

Lines 467: "Figure C1 shows the streamwise evolution of the deviation in vertical velocity, pressure and potential temperature from the inflow of the domain at the capping inversion."

Figure C1: "Streamwise evolution of the vertical velocity, pressure and potential temperature deviation from inflow conditions for the weak (a) and moderate (b) stability cases at z = 1200 m. Each variable a_i is normalized as $\hat{a}_i = \frac{a_i}{\max(a_i) - \min(a_i)}$ and averaged along the y-direction (from y = 1953 m to y = 5922 m). The gray shaded area in each panel represents the region covered by the wind plant."

Line 481: “Note that we show the phase shift between the pressure, vertical velocity and potential temperature above the capping inversion; however, this phase shift is also observed in the boundary layer.”

Other scientific comments

1. Line 139-140 ”The balance between turbulence production via shear below the LLJ’s nose and temperature stratification result in a shallow stable boundary layer” and line 145-146 ”The balance between turbulence production via shear below the capping inversion and temperature stratification result in a deep stable boundary layer.” You seem to suggest two different physical mechanisms. Are you saying the stable boundary layer is shallow or deep depending on whether buoyant destruction is balanced by shear production below the LLJ or below the capping inversion? I don’t think this is a proper description of what happens physically.

We restructure and shorten this paragraph because it was confusing. This paragraph now reads as:

Line 146: “Boundary layer evolution varies with surface forcing (Figure 4). A fast cooling rate (i.e, -0.5 K/h) produces increasing temperature stratification below 400 m and quasi-neutral stratification up to the capping inversion (Figure 4c). The rapid development of a stable layer close to the surface reduces the vertical transport of momentum, suppressing turbulence aloft. A broad low-level jet (LLJ) develops after 4 hr as turbulence aloft decreases (Figure 4a,b). Boundary-layer evolution for the slower cooling rate is slightly different. A -0.2 K/h produces increasing temperature stratification up to the capping inversion (Figure 4h). The slow cooling rate initially produces nearly uniform cooling of the entire turbulent layer. After 3 hr, temperature stratification close to the surface is large enough to reduce the vertical transport of momentum and a LLJ starts forming close to the capping inversion (Figure 4f,g). Because of a slower cooling rate, the gradual reduction in vertical turbulent mixing results in a deeper boundary layer in the weak stability case compared to the moderate stability case.”

2. Line 143-144 ”After 3 hr, temperature stratification close to the surface reduces the vertical transport of momentum above 400 m, and a LLJ starts forming (Figure 5f,g).” This goes a bit fast, please explain. How does the stratification close to the surface affect transport above 400m and lead to a LLJ?

Please see comment above.

3. Line 220-221 ”Whereas the v-velocity transports momentum to both sides of the turbine, the w-velocity primarily transports momentum upwards.” How do you know that w-velocity primarily transports momentum upwards? Did you check this? You can’t make this conclusion only based on Figure 11.

We appreciate the reviewer’s attention to detail. Figure iv shows that the vertical velocity immediately upstream of the turbine can be negative at the bottom of the turbine rotor layer. We decide not to include this figure in the text because it is a minor detail that deviates from the main objective of the manuscript.

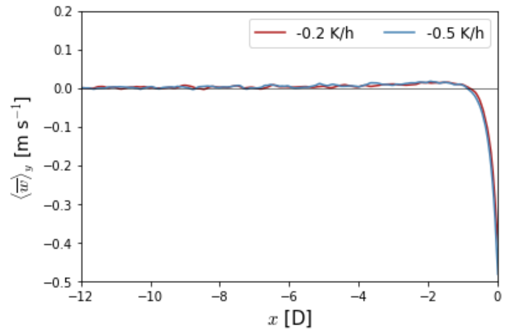

Figure iv: Time-averaged vertical velocity at the bottom (z = 27 m) of the turbine rotor layer for each atmospheric condition. The vertical velocity is averaged in the y-direction over the span of the wind plant.

We update the manuscript as follows:

Line 251: “Whereas the v-velocity transports momentum to both sides of the turbine, the w-velocity primarily transports momentum upwards. Immediately upstream of the turbines (-1D<x<0D), the vertical velocity is negative at the bottom of the turbine rotor layer (not shown), transporting momentum downwards. Nonetheless, the vertical velocity is positive over the rest of the induction region, transporting momentum upwards.”

4. Line 237 “Vertical momentum advection is the primary forcing mechanism ... .” Figure 13 shows that the pressure contribution is also significant. I agree that only the vertical momentum advection is affected by stability, but you make it sound like the pressure gradient force is insignificant. See also lines 297-298 “The pressure gradient force is also present upstream of the wind plant; however, the primary mechanism decelerating the flow is the vertical advection of horizontal momentum.” I don’t think this is true. I’d say the flow deceleration is due to the combined effect of vertical advection and pressure gradient force.

We agree with the reviewer that the pressure gradient has a strong contribution to wind plant blockage. We soften the language in our manuscript as follows:

Line 272: “The vertical advection of streamwise momentum and adverse pressure gradient are the primary forcing mechanisms influencing wind plant blockage in our simulations.”

Line 338: “The pressure gradient force is also present upstream of the wind plant; however, the vertical advection of horizontal momentum contributes more to the deceleration of the flow.”

Minor/technical comments

1. Figure 3: Please mention how the effective grid resolution is determined. Is this from visual observation, or is there a certain method to calculate the effective grid resolution?

We did not calculate the effective grid resolution of our simulations explicitly. Rather, we cite the expected effective grid resolution based on the reduced advection scheme. Our main goal is to illustrate the scales at which we expect to resolve turbulence in our simulations. We update the caption of Figure 2 as follows:

Caption Figure 2: "Compensated turbulence spectra of the w-velocity for the $\Delta x = 7$ m neutral LES at z=90 m (a), z=300 m (b), and z=800 m (c). Spectra are color coded in 10-minute time increments since initialization. The dotted, black vertical line in each plot represents the effective grid resolution (4-5Δx) expected from the reduced advection scheme in our simulations (Kosović et al., 2016). The theoretical −2/3 Kolmogorov slope for the inertial range is indicated by the solid black line in each plot."

2. Line 122-126: "Large, localized gradients of the horizontal velocity instigate large-scale turbulence early in the simulation, which then cascades into small-scale turbulence (Figure 3)" and "Localized shear instabilities instigate turbulence throughout the boundary layer within the first hour of the simulation. These structures break up rapidly into smaller eddies, reducing shear until a quasi-steady state is reached. Turbulence structures form rapidly close to the surface and propagate upwards (Figure 3)." It sounds to me as if you are saying the same twice. Is this intentional or should one of the two phrases be removed? Furthermore, it is not clear how turbulence is initialized. Do you apply random perturbations to the velocity field?

Thank you for pointing out duplicate information. We remove the first sentence in our manuscript. In addition, we include the following information in the Methodology section:

Line 128: "Furthermore, we speed up turbulence development by adding ± 0.5 K perturbations to the potential temperature field below the capping inversion at initialization."

3. Line 144 "Vertical turbulence redistributes ... ." Should this be vertical turbulent mixing? Not sure what you mean by "vertical turbulence".

We replaced vertical turbulence with vertical turbulence mixing in the manuscript.

4. Figure 5: It is hard to appreciate the formation of a LLJ from the evolution of the wind speed components. Wouldn't it be more informative to show the wind speed magnitude and the wind direction with height?

Thank you for your suggestion, we modify the figure to include the magnitude of the horizontal velocity and the wind direction rather than the u- and v-wind components. We also modify Figure 6 to include the horizontal wind speed and wind direction rather than the u- and v-wind components.

5. Line 179-180 "Horizontal wind speed is on average 31 % slower ... ." Do you see a 31% difference in the wind speed or in the wind speed deficit? This is a big difference, so please clarify.

We added clarification to mention the differences in velocity deficit as follows:

Line 192: "The horizontal wind speed deficit is 31% slower …"

6. Line 196-198: Where are the order of magnitudes of the various forces coming from? Did you get these values from the results or are they a simple order of magnitude estimate? Please specify.

We estimate the order of magnitude of the forces from data in our simulations. We added clarification in the text to stress that the order of magnitudes for each term is for our simulations specifically as follows:

Line 212: "We evaluate the balance between momentum advection by the mean flow and a pressure gradient. Even though the Coriolis force in our simulation domain is not negligible, Coriolis forcing in the induction region is small. The Coriolis parameter scales as $f_c \sim 10^{-4}\ s^{-1}$ and the v-velocity in the turbine rotor layer for both stability cases is on the order of $\overline{v} \sim 0.1\ m/s$, thus Coriolis forcing is of the order $f_c \overline{v} \sim 10^{-5}\ m/s^2$. Turbulence momentum redistribution is also small in the induction region of the wind plant for our simulations $\nabla \cdot \left(\overline{u' \boldsymbol{u}'} \right) \sim 10^{-4}\ m/s^{-2}$. In comparison, momentum advection by the mean flow in the induction region is of the order $10^{-1}\ m/s^{-2}$ in our simulations."

7. Line 221-222 "The streamwise velocity advects momentum back into the induction region of the turbine" and line 235-236 "The streamwise momentum advection replenishes momentum upstream of the first turbine row." I find the formulation of these sentences a bit weird and therefore confusing. Streamwise momentum advection simply acts as a source of energy because the flow is decelerating. Please reformulate to make it more clear what you mean.

We reformulate both sentences to make them clearer:

Line 255: "The streamwise velocity replenishes momentum in the induction region as the flow decelerates."

Line 271: "The streamwise velocity replenishes momentum in the region upstream of the first turbine row $\Delta(\rho \overline{u}\, \overline{u}\, S_x)$."

8. Line 240 "Momentum advection by the v-velocity is 10.1 % as large as the ... ." This confusing, I guess you are saying that the momentum advection by the v-velocity is only 10.1 % of the vertical momentum advection?

We reformulate this sentence as follows:

Line 275: "Momentum advection by the v-velocity is only 10.1% (12.8%) of the vertical advection of u-momentum for the moderate (weak) stability case."

9. Discussion of Figure B1 is a bit hard to follow. What does a negative value for the pressure gradient force mean? Does a negative value correspond to an upward or downward pressure force?

Thank you for pointing this out. We include additional information in this section as follows:

Line 451: "Figure B1 shows the streamwise balance of vertical momentum. Because of the convention adopted throughout the manuscript ($\Delta X = X_{out} - X_{in}$ for an arbitrary variable X on the control volume $\mathcal{V}$ shown in Figure 11), negative terms in Figure B1 correspond to upward forces. As such, $\Delta\overline{p}S_z < 0$ is forcing the flow upwards and $\rho g' V > 0$ is forcing the flow downwards."

We also add clarification as follows:

Line 454: "Momentum balance for the w-velocity indicates the secondary flow (i.e., w-velocity) in the induction region is driven by a pressure gradient far upstream, and horizontal transport of w-momentum close to the turbines (Figure B1). Immediately upstream of the first turbine row (-1D<x<0D), horizontal advection of w-momentum drives upward motions. The sharp deceleration of the u-velocity immediately upstream of each front-row turbine ($\Delta\overline{u} < 0$ is large as shown in Figure 9) results in momentum replenishment, which is balanced by a downward pressure gradient force ($\Delta\overline{p}\, S_z > 0$). Farther upstream (x<-1D), an upward pressure gradient force ($\Delta\overline{p}S_z < 0$) overcomes buoyancy and the streamwise advection of vertical momentum. Redistribution of vertical momentum by the v- and w-velocity components is marginal within the induction region of the wind plant."