Response to Referee #3: James Bleeg

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.

Paper title: "Investigating the physical mechanism that modify wind plant blockage in stable boundary layers"

The authors simulated an idealized wind farm on flat terrain operating in two different stable boundary layers. The simulations were run back-to-back with a turbine operating in isolation. The results of these simulations were then analyzed to quantify the impact of blockage on the flow upstream of the wind farm and also on the upstream row of turbines. Further, the authors interrogated the solutions to understand the physical mechanisms behind these blockage effects.

If accepted for publication, I think this paper would be a strong addition to the growing literature on wind farm blockage effects. The analysis of the simulated flow is lucid and compelling. The main findings, in my view, are important and mostly new.

The main area where I feel the paper could be improved is putting the research into context. The next section of this review focuses on this aspect. I include a number of miscellaneous comments after the context section.

Putting the findings into context:

Gravity waves – Gravity waves are referred to a number of times in the manuscript, mostly in the vein of we-are-not-dealing-with-gravity-waves-in-this-study. The introduction puts this explicitly: "Here, we investigate how atmospheric stability modifies upstream blockage in the absence of gravity waves." There is even a section in the appendix substantiating the claim that there are no gravity waves in the flow solutions. I do not think this approach to setting the no-gravity-wave stage is necessary and may even be counterproductive, potentially leading readers to believe that the findings are limited to gravity-wave free conditions.

There are good reasons to believe wind-farm-induced gravity waves are very common. The manuscript implies the findings in the two Bleeg papers are not affected by gravity waves (see line 50), but this is not the case. All our wind farm simulations have gravity waves, at least to some degree. Gravity waves, as you know, are the primary means by which disturbances are transmitted in density-stratified flow. Such a disturbance might be an obstacle like a wind farm causing flow to rise. The resulting gravity waves—or speaking more generally, inviscid effects related to disturbed stratified flow—and their effect on the wind farm will depend upon the characteristics of the stratification and the wind farm itself. The authors appear to be simulating a set of conditions where the effects of gravity waves on flow upstream of the wind farm is not as pronounced as in the first Allaerts and Meyers JFM paper. I think it is sufficient to say just that.

There is strong numerical evidence at this point that inviscid effects related to stratification above the boundary layer—and the horizontal pressure gradients associated with them—can have a significant impact on blockage effects. This paper helps bring to light another important influence on the production of leading row turbines relative to what they would produce in isolation: the vertical advection of u-momentum. The finding not only improves our physical understanding of blockage effects, but also has significant implications for the modelling of these effects.

We appreciate and share your opinion on the prominent discussion on gravity waves. However, other reviewers have strong opinions about spurious gravity waves in the domain and upstream propagation of gravity waves, which can amplify blockage. Therefore, we decide to leave this discussion in our manuscript to proactively address such concerns. However, we do soften our language throughout the entire manuscript. An example is as follows:

Line 68: “Here, we investigate how atmospheric stability modifies upstream blockage with minimal upstream propagation of gravity waves”

Engineering models for blockage – The engineering models designed to predict blockage effects are inviscid, potential flow-type models, which do not account for shear and therefore are not able to reliably account for what this paper suggests is one of the most important contributors to front row blockage loss: the vertical advection of u-momentum. It is up to the authors, but I think it might be worth emphasizing this in the paper.

The manuscript states that blockage is not currently accounted for in EYA’s. This was true in 2018, but it is not true anymore. Almost all EYA’s account for blockage in one way or another. And in many cases, a potential flow model is used to account for the blockage effect. Potential flow models, as discussed, will miss a significant contributor to the impact of blockage on turbine production. I feel that the wind industry community would benefit from having this point highlighted.

Thank you for your suggestion, this is indeed worth highlighting. We include the following in our discussion:

Line 375: “Potential flow models often used to include the power loss from blockage in energy assessments do not account for vertical shear of the horizontal velocity (e.g., Forsting et al., 2021). Vertical advection of u-momentum is the primary amplifier for blockage in our simulations, driven by shear. As a result, energy yield estimates might underestimate losses from blockage.”

Has anyone looked into this before? According to the manuscript, the dominant mechanism causing front row wind turbines to produce less than they would in isolation is the vertical advection of u- momentum (at least in these simulations). To me, this is the most important finding in the paper. The Discussion and conclusion sections highlights others who have focused on adverse pressure gradients as a key driver behind blockage, and then explains that vertical advection of u-momentum amplifies the impact on the front row turbines. I’m a bit biased here, but it is worth mentioning that Bleeg and Montavon included a full section on this subject? The section makes the point that the combination of shear and flow rising as it approaches the wind farm, due to the presence of the wind farm and the ground, “appears to be an important factor in determine the magnitude of the blockage loss.” Immediately following, the paper reads, “in case 9, for example, the streamline passing through the hub in the wind farm configuration originates approximately 4.5 m below the hub-intersecting streamline in the isolated case. In turn, the wind speed on the streamline far upstream is approximately 2% lower in the wind farm configuration compared to isolated operation. This significant wind speed difference is the result of the vertical flow deflection combining with the increased shear that prevails in stable conditions."

In my opinion, the physical explanation provided in the manuscript under review is more clear, complete, and convincing that what we provide in Bleeg and Montavon. Much credit is due to the authors for this important finding. That said, I think it is fair to say that the Bleeg and Montavon paper did highlight the important influence of shear in combination with the upstream vertical deflection of flow as it relates to the impact of blockage on the production of leading row turbines relative to a turbine in isolation.

I am not an academic and am not familiar with what is required in a situation like this (also, I suppose I am not without bias in this regard), so I leave it to the authors and the editor to decide whether this should be acknowledged in the paper. For what it is worth, I think referencing the earlier result could further strengthen the credibility of the current finding. A sentence or two would be sufficient. And if it is followed by something indicating that your analysis more complete, I would not object, because it is.

Thank you for pointing out that we do not include this finding in our discussion. We agree that we should include previous work highlighting the importance of shear. We update the manuscript as follows:

Line 372: "Bleeg and Montavon (2022) also highlight the importance of vertical shear of the horizontal velocity and the vertical deflection of the flow. They suggest that, due to shear, the hub-height flow at the turbine location is slower than far upstream because the flow is being deflected upwards (Bleeg and Montavon, 2022)."

Miscellaneous:

The following questions, comments, and suggestions are in roughly order of priority:

A. I can't quite tell where the overall control volume in Figure 13 ends in the x-direction. The caption says it is bounded at the first turbine row (x = 5670 m). Clear enough, but where is that location in Figure 12? Is it at 0 D or just upstream? The reason why I ask is that that thrust body forces from the GAD are being applied to cells that include locations just upstream of 0 D, resulting in a rapid drop in pressure. If the end of the control volume in Figure 13 does correspond to 0 D, how would the results change if the end of the control volume were moved to just in front of the GAD?

Thanks for highlighting this inaccuracy. As you point out, the GAD applies forces to the flow upstream of its location due to its numerical implementation, resulting in a very rapid drop in pressure. We considered this in our initial analysis and shifted the control volume slightly upstream. We clarify as follows:

Line 235: "The thrust force imparted by the turbine to the flow is a fundamental driver for blockage (Ebenhoch et al., 2017). In the numerical implementation of the GAD model, the aerodynamic forces are spread across multiple grid cells along the streamwise direction to avoid numerical instabilities (Mirocha et al., 2014). A pressure gradient upstream of the turbine forms in response to the thrust force that the turbine imparts on the flow ($\Delta\overline{p}_{pert}/\Delta x > 0$ upstream of the turbine in Figure 12). Because the thrust force is spread across multiple grid cells in the streamwise direction, the maximum in pressure in front of the turbines is located slightly upstream of the actual location of the GAD in the numerical domain (Figure 12). As a result, we restrict the control volume V in Figure 11 to extend up to x=5647 m, the location of the maximum in pressure perturbation upstream of the turbine array (vertical dotted line in Figure 12)."

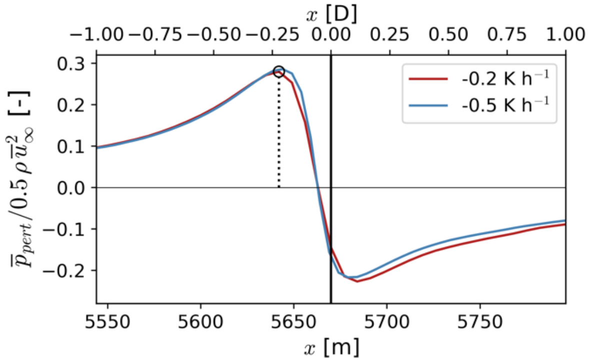

Figure 12: Hub-height pressure perturbation of a front-row turbine in the wind plant for each stability case. The pressure perturbation is normalized over the corresponding dynamic pressure for each stability condition. The solid black vertical line illustrates the location of the GAD in the numerical domain. The dotted vertical line illustrates the local maximum in pressure perturbation upstream of a front-row turbine in the wind plant. The secondary x-axis is scaled to locate x=0D at the location of the front-row turbine.

Figure 15: Momentum balance over the entire induction region of the wind plant. The integral momentum equation is evaluated on the control volume V shown in Figure 11a. The control volume V is bounded in the x-direction by the inflow of the domain and the maximum in pressure perturbation upstream of the turbines (x = 5647 m). The mean momentum fluxes and the pressure gradient force are normalized using the u-momentum flux at the inflow of the control volume far upstream ($\rho\overline{u}_{\infty}\overline{u}_{\infty}S_x$) for the respective stability case.

We also modify the relevant figures in the manuscript (Figures 13, 14, 16, 17, 18, 19, 20) to highlight this pressure drop that happens across the grid cells that intersect the GAD. An example is Figure 13 in the manuscript:

Line 246: "Immediately upstream of the turbine (cross-hatched area in Figure 13), the pressure gradient force becomes negative because the GAD produces a pressure drop in the flow and the pressure perturbation field reaches a local maximum slightly upstream of the turbine (Figure 12). In the numerical implementation of the GAD model, the aerodynamic forces are spread across multiple grid cells to avoid numerical instabilities (Mirocha et al., 2014), which causes the pressure field to decrease over multiple grid cells (Figure 12)."

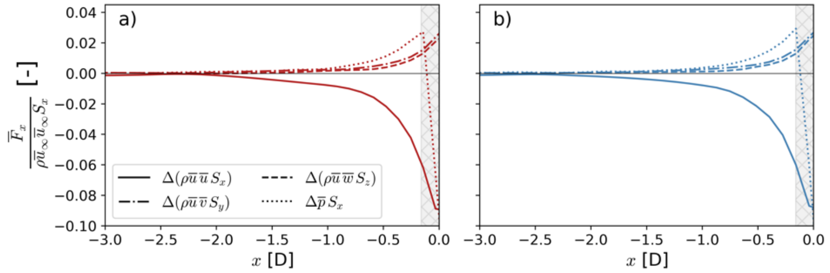

Figure 13: Streamwise evolution of the u-momentum equation (Eq. 2) for a stand-alone turbine in the weak (a) and moderate (b) stability cases. The integral momentum equation is evaluated on differential control volumes δV along the x-direction, as shown in Figure 11. The x-axis is scaled to locate x=0D at the location of the turbine. The mean momentum fluxes and the pressure gradient force are normalized using the u-momentum flux at the inflow of the control volume far upstream ($\rho\overline{u}_\infty\overline{u}_\infty S_x$) for the respective stability case. The cross-hatched area in each panel illustrates the grid cells influenced by the thrust force from the GAD.

Line 255: “Note that the momentum balance immediately upstream of the turbine (cross-hatched area in Figure 13) is not equal to zero because the thrust force from the GAD is not included in our calculations.”

B. Again, I found the analysis of the results in sections 3 and 4 to be clear and convincing. That said I wonder if you could go just a little further to provide more physical insight and help the reader connect the dots towards your key finding. I refer specifically to the significant influence of the vertical advection of u-momentum. Perhaps you could break this down a bit. It could help drive home the point of the importance of shear. The x-component of velocity is clearly higher at the top of the control volume than the bottom. I’m not sure how the vertical component of velocity varies streamwise upstream of the wind farm, but I suspect it is positive and generally increases as flow approaches the wind farm. Of course, the trend may differ between the top and bottom of the control volume (in fact, the vertical component of velocity may be negative close to the rotors). I think it would be nice to have these things related to the vertical advection of u-momentum broken down, though I concede that what you already have in the report is sufficient to make your point.

We appreciate your suggestion. We now include a new figure of the vertical velocity in the manuscript and extend the discussion on shear. Note that we only show the vertical velocity for the upper half of the turbine rotor layer because, as you point out, the vertical velocity can be negative at the bottom of the turbine rotor layer close to the turbines (Figure i).

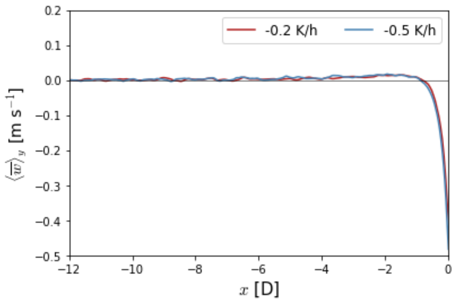

Figure i: Time-averaged vertical velocity at the bottom (z = 27 m) of the turbine rotor layer for each atmospheric condition. The vertical velocity is averaged in the y-direction over the span of the wind plant.

We extend our discussion on shear 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."

We include the vertical velocity upstream of a front-row turbine in the middle of the wind plant and of a stand-alone turbine as follows:

Line 309: "Therefore, differences in vertical transport of horizontal momentum between a stand-alone turbine and a turbine in the wind plant are entirely due to the vertical velocity that forms upstream of the turbine array (Figure 21). For the wind plant, the secondary flow (i.e., net upwards w-velocity) extends farther upstream than for a stand-alone turbine (Figure 21)."

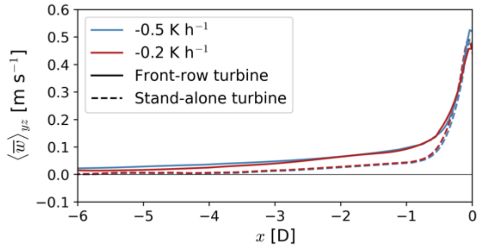

Figure 21: Streamwise evolution of the vertical velocity upstream of a stand-alone and front-row turbine in middle of the wind plant. The vertical velocity is averaged in the y-direction over the rotor diameter and in the z-direction over the top half of the rotor layer.

c. This is a big statement in the paper: “Given that the normalized pressure gradient force remains unchanged with atmoshperic stability and turbine array size, differences in blockage are caused by momentum redistribution in the induction region.” Firstly, you’ll want to correct the misspelling of atmospheric. Secondly, I just want to put this into context. In practice, when evaluating the impact of blockage or wakes on a wind farm, what we care about is the power production of the wind farm turbines relative to what they each would produce in isolation. In other words, we care about the wind conditions that each turbine experiences relative to the conditions it would experience in isolation. My interpretation of your analysis is that, at least for the simulated conditions, the vertical advection of u- momentum is *the dominant factor* affecting the production of leading row turbines relative to what they would produce in isolation. It is by far the main physical mechanism behind blockage loss for these turbines—again, for the simulated conditions. Am I interpreting your work correctly?

Thank you for highlighting the spelling mistake, we corrected the manuscript accordingly. Your interpretation is correct. We also extend our analysis on the power production of the turbines compared to a stand-alone turbine 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.”

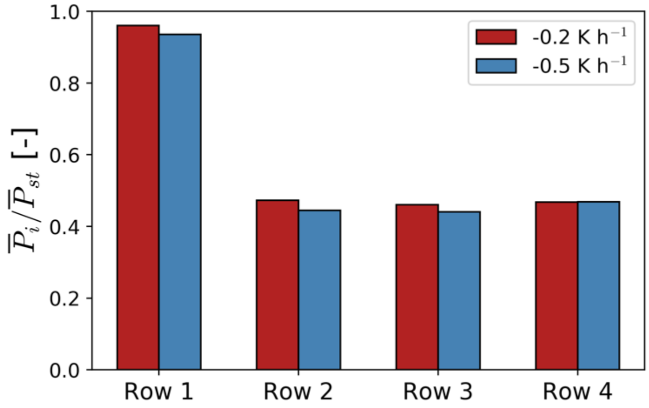

Figure 10: Normalized turbine power for each row of the wind plant and each atmospheric condition. The mean turbine power for the i-th row of the wind plant $\overline{P}_i$ is normalized over the mean turbine power of a stand-alone turbine $\overline{P}_{st}$.

D. I’m not sure your simulation setup can reliably capture gravity waves. With our own steady- state RANS model, a domain height of 3500 m (too low) and a damping layer thickness of 1000 m (too thin) would yield significantly different results with respect to gravity waves than our standard domain (top boundary at 17,000 m and much thick damping layer). That said, if you were to re-run your analysis with a much larger domain, I doubt your main findings would be much different. Thus, in my view, it is not required to run this sensitivity check, though it would be a nice-to-have. If future studies focus on gravity waves—and more broadly the influence of the stably stratified atmosphere above the boundary layer—such a sensitivity study would be needed.

Thank you for your suggestion, we agree that this analysis would be very helpful. However, the computational resources required for this sensitivity analysis are not available to us at this moment so we cannot include this in our manuscript.

E. As suggested above, I would consider just dropping Appendix C. However, if you keep it, could you please clarify the height at which the values in Figure C1 are being plotted?

Thank you for highlighting missing information. We decided to keep the Appendix on gravity waves because the other two reviewers consider this information important for our analysis and discussion.

We update the caption of Figure C1 to include the height for the data as follows:

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 $\hat{\bar{a}}_i = \frac{a_i}{\max(a_i) - \min(a_i)}$ 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.”

F. If you pursue this research further, it would be interesting to know what you find when simulating a neutral boundary layer and/or an unstable boundary layer.

Thank you for your recommendations. We agree that this would be an interesting and useful path for future research.