

Delft University of Technology, Netherlands

This study is part-2 of the project:
"Numerical Modelling of Regional-Scale Wind Farm Flow Dynamics"

Response to Reviewer 1

Exec: M.A.Khan - February 5, 2026

Revised: Simon Watson - February 5, 2026

Revised: Matthew Churchfield - February 5, 2026

The authors thank the reviewer for the time dedicated to revising the article and suggesting valuable feedback to improve this research and the article. We proceed with answering and clarifying their comments, where possible.

For ease of tracking, our response to each comment is denoted in black, while the reviewer's comments are denoted in blue. The detailed changes made to the manuscript are included in the "track changes" section at the end of this document.

Line 117. Wave theory should not be the cause of wind-farm-induced AGWs.

We agree to the reviewer that wave theory is not a cause of AGWs, and the sentence is modified to "The description of wind-farm-induced atmospheric gravity waves involve mass conservation, buoyancy, and wave theory".

Lines 137-138. Is there any theory or reference to support this statement?

We suppose the reviewer is suggesting to provide a reference to how the capping inversion and free atmosphere support different wavelengths. This is investigated by (Khan et al., 2024a) by relating effective wavelength of internal waves with the Froude number that is function of geostrophic wind speed and lapse rate in the free atmosphere. These findings were then extended by (Khan et al., 2024b) and the current study to investigate the dependency of interfacial waves on capping inversion Froude number, which is a function of capping inversion strength and height. We have now cited both papers as references.

Lines 168-169. Why the periodic boundary conditions can be used in the transverse direction? Note that there are only 5 turbines in the lateral direction with a spacing of three rotor diameters, while the domain size is 6 km in the y-direction.

Using periodic boundary conditions is effective in preventing spurious gravity waves at the lateral boundaries. Spurious gravity waves are induced when the boundary conditions like slip wall, inlet-outlet, or symmetry are used. Periodic boundary conditions eliminate spurious gravity waves excluding any concerns related to their impact on the wind farm aerodynamics, which is yet to know. When using periodic boundary conditions, sufficient spacing between the wind farm and lateral boundary is critical to avoid hydrodynamic effects (i.e, flow accelerating in between the wind farms as its blocked by the wind farm) due to periodization the wind farms. The effect of lateral spacing between the wind farm and the lateral boundaries while using periodic boundary conditions is investigated by (Lanzilao and Meyers, 2024). It is shown in this study that domain widths greater than three times wind farm width reduce the hydrodynamic effects to less than 2% for deeper boundary layers. For shallow boundary layers, the domain has to be much wider, which is why the shallowest boundary layer simulate in this study is 18 km wide instead of just 6 km.

The explanation on the domain width requirements while using periodic boundary conditions at the lateral boundaries is now included on lines 188-193 of the modified manuscript that read: "The values of domain width are inferred from (Lanzilao and Meyers, 2024), which shows that the ratio of domain width to wind farm width should be roughly greater than 3 in deeper CNBLs (i.e. $H_i \geq 500$ m). This is required to avoid adverse hydrodynamic effects when periodic boundary conditions are used at the lateral boundaries. For shallow boundary layers the domain should be much wider, which is why the shallowest boundary layer (i.e, $H_i = 250$ m) simulated in this study is 11 times wider than wind farm width. This domain width is chosen because the gravity waves mostly disperse before reaching the lateral boundaries, preventing their recycling into the wind farm zone."

Figure 2. Plot ten turbines in the figure is more consistent with the text.

This suggestion is implemented as can be seen in Figure 2 of the modified manuscript.

Table 1. The ranges of some non-dimensional parameters are not consistent with that in Table 2 and Figure 6.

This is well spotted by the reviewer and is fixed.

Title of Section 3.2. The symbol Cr should not appear first in the title and its definition should be specified.

This suggestion is implemented as the definition of Cr is included that reads: " Cr is the ratio of kinetic energy of the downward propagating waves, reflected waves, to that of the real upwards propagating waves. The reader is directed to Khan et al. (2024a) for details about the estimation of Cr ."

Line 245. It should be explained in the text about how to estimate the reflection coefficient.

The Cr metric and its estimation is the same as it is explained in Khan et al. (2024a). In the view of a concise manuscript, we think it is fine to refer the reader to the reference that thoroughly explains the metric. Also, Cr is used here only to validate the criterion defined by Khan et al. (2024a), which is a brief detail of this study. Though, we have included upwards and downwards propagating waves extracted from the vertical velocity plots in Figure. 7 to assist the readers with qualitatively interpreting reflections.

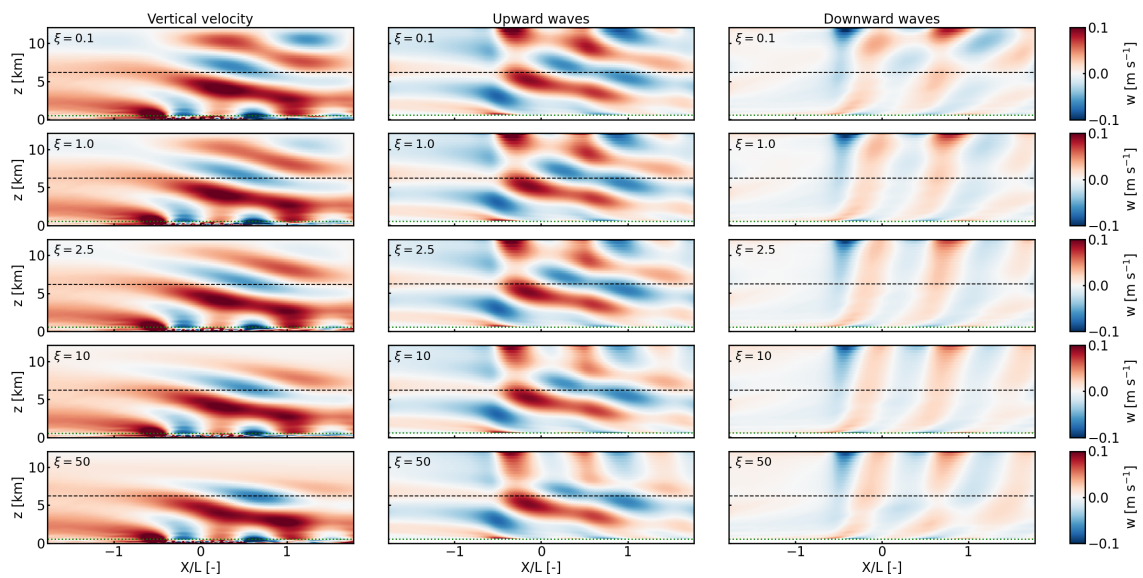


Figure 1. Contours of temporally averaged vertical velocity, upwards, and downwards propagating waves in a streamwise-oriented vertical plane for varying Rayleigh damping strength (ξ) of the T-RDL. The flow conditions are defined by $Fr = 0.15$, $Fr_i = 1.3$, $\tilde{H}_i = 0.087$, and $S_h = 0.0255$. The green and black dashed lines show the capping inversion mid-height and start of T-RDL, respectively.

Table 2. Please explain more on how to realize the NFA-ABL case in simulation.

We suppose the reviewer is suggesting to give further details of the simulation setup of the NFA-ABL case in the view of reproducibility. Thus, we include a better description of this case, which is: "NFA-ABL, where the setup is the same as the reference CNBL case, but the free atmosphere is neutrally stratified. This is done by setting the lapse rate in the free atmosphere to zero, while keeping everything else the same as in the reference CNBL case."

Figure 4. How can we judge that the CNBL with $L_z=12\text{km}$ is the most realistic result if the result depends on simulation configuration?

This is a valid point raised by the reviewer that requires clarification.

The configuration fully resolving AGWs most realistic in terms of capturing most of the physics among the investigated configurations. Which means that it fully captures the AGWs besides other physics captured by other configurations. This claim becomes valid from the discussion we have given on the comparison of various simulation configurations. For instance, "leaving out buoyancy or simulating TNBLs excludes the hydrostatic or global blockage effect and the impacts of AGWs on the wind farm performance (Lanzilao and Meyers, 2024; Stipa et al., 2024). Including a capping inversion and the free atmosphere but partially-resolving the AGWs, is the same as rigid-lid simulations or cases simulating just the ABL. In other words, the channeling effect caused by the top boundary is completely dominant over the stratification effects. Therefore, a more physically realistic simulation, which will also fully resolve AGWs, includes the capping inversion and the free atmosphere, and places the top boundary sufficiently high above the surface."

Initially, we didn't explicitly give the above reasoning in the discussion that would leave the readers to infer it on their own.

Section 4.2. Similarly, if the result depends on the value of ξ , how to judge the correctness of the result? Or how to choose the value of ξ in practical simulations?

The reviewer has rightly spotted the missing justification for the values of ξ simulated in this study. The choice of ξ for practical simulations is shown by (Khan et al., 2024a) to be in the range of 1 – 10. Thus, we validated this recommendation in comparison to values outside this range (i.e, 0.1 and 50). This is now explicitly mentioned as: "The chosen range of ξ follows Khan et al. (2024a), who demonstrated that reflections are minimal for $\xi \in [1, 10]$ for practical values of N ."

The correctness of solution is judged by inter-comparison of the cases presented here and in relation to the amount of reflections specified by the value of Cr that are reported in the text. Reflections are higher for $\xi = 0.1$ and $\xi = 50$ than that of ξ in the range of 1 – 10. We were missing the values of Cr for the two cases with higher reflections, which probably wasn't clear enough.

Moreover, the description of the newly include plots showing upward and downward waves also clarifies the bases to judge the results. This description reads: "As shown on the temporally averaged vertical velocity plots in Fig. 3, the AGWs reflect the most from the top boundary when the damping strength is the weakest or too strong. Damping strength is described by the non-dimensional value $\xi = 1/(\tau N)$. For the range of ξ from 1.0– 10.0, the waves are damped significantly, and the AGW amplitude decreases towards the top boundary. Using $\xi > 10$ makes the T-RDL damping too strong and the AGWs reflect off it, as seen in Fig. 3 for $\xi = 50$. This can be seen on the plots for the downward waves where there is a phase shift in the T-RDL when $\xi = 0.1$ as the waves reflect from the top boundary and inside the non-damped layer when $\xi = 50$ as the waves reflect off the T-RDL itself. For $\xi = 1.0– 10.0$, the wave fronts show no phase shift. Note that the strength of downward waves is roughly the same for all values of ξ , as the reflected wave energy is in range of 8 to 11.5% of the upward wave energy. See (Khan et al., 2024a) for more details about extracting the upward and downward waves from the vertical velocity."

Figure 11. It is strange that the authors called the LES results as measured values and the deep-water approximation as calculated ones. In addition, how to get the results from the deep-water approximation?

We agree that the use of terms is misleading, thus we now call LES results as "calculated" and those of deep-water approximation as "theoretical".

We believe that the procedure to calculate the interfacial wavelengths from the deep-water approximation is sufficiently explained on lines 510-518. We didn't redefine the variables in Equation.2 as they are already defined in manuscript before this equation. We guess this confused the reviewer.

Figure 16. The authors stated that wind-farm performance is most sensitive to the parameter H_i . However, this figure seems to indicate that wind-farm performance is most sensitive to the parameter S_h . This needs to be explained.

It is true that variation in power against S_h is most prominent and needed to be addressed to draw clearer conclusions. Thus, we did further analysis to distinguish power variations caused by higher velocity for putting the turbines at higher hub heights from those caused by stratification effects. This analysis is included in Sections 4.4.1 and the newly added section 4.4.2 on wind farm efficiencies that is as following.

0.0.1 Wind Farm Efficiencies as a Function of Physical Non-Dimensional Parameters

Although wind farm performance is thoroughly discussed in the previous section, it is essential to quantify wind farm blockage effect and wake losses relative to the inflow characteristics of each case. This is important because varying atmospheric conditions leads to different inflow characteristics, such as different shear profiles and veers. (Allaerts and Meyers, 2017) proposed wind farm efficiency (η_t) as the product of non-local and wake efficiencies, the former being an estimate of wind farm blockage effect and the latter to quantify wake losses. These efficiencies are defined as following.

$$\eta_t = \eta_{nl} \cdot \eta_w, \quad \eta_{nl} = \frac{P_1}{P_\infty}, \quad \eta_w = \frac{P_{wf}}{N_t \cdot P_1}$$

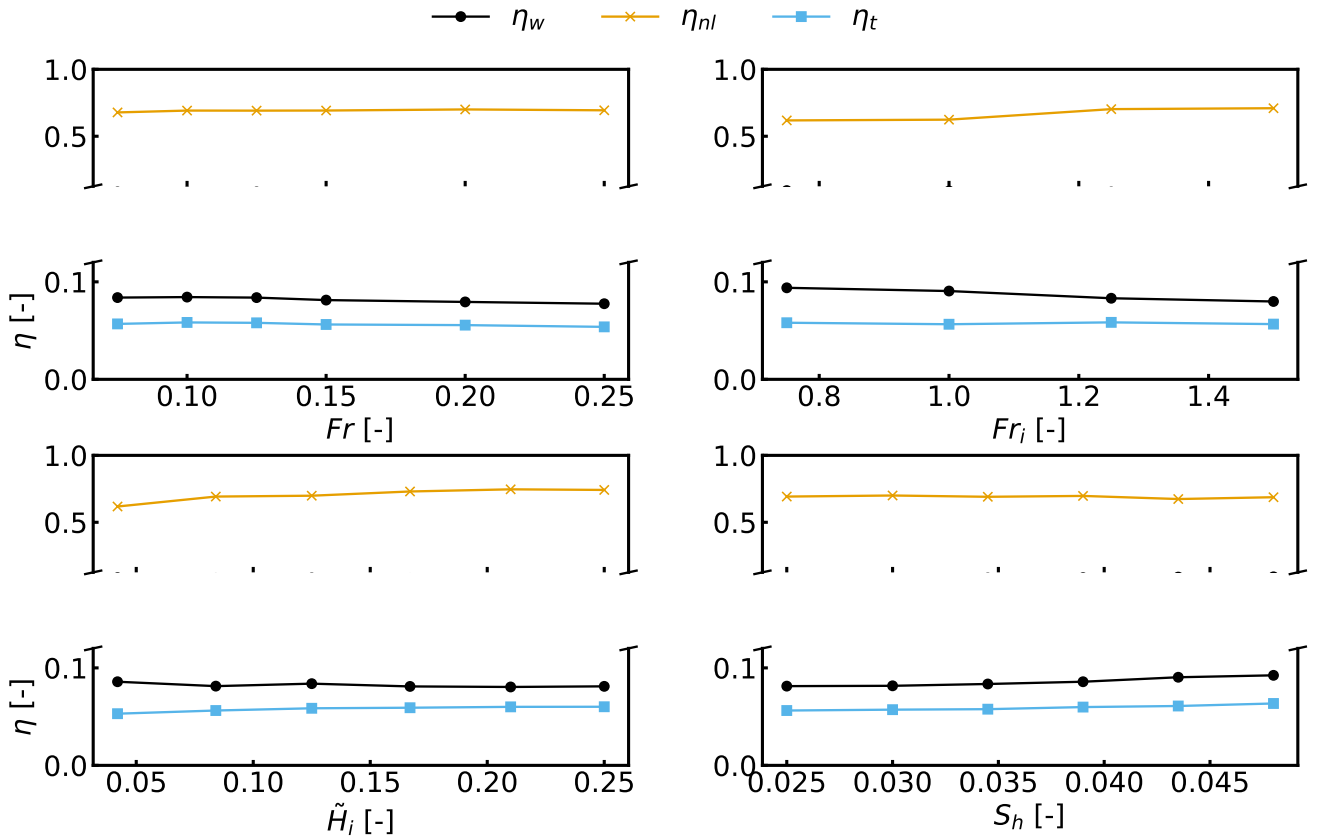


Figure 2. Wake, non-local, and total wind farm efficiencies as a function of Fr , Fr_i , H_i , and S_h .

The non-local efficiency (η_{nl}) is the ratio of average power of the 1st row turbine (P_1) to the power produced by a turbine in the free stream or isolation (P_∞). Since the LES power predictions are based on rotor-average velocities, we calculate P_∞ as a function of rotor equivalent wind speed in the free stream. To this

end, we sample velocity over the rotor annulus for each case from the respective precursors and temporally average it over the last flow through time of the successor simulations as this interval is used to calculate the actual power outputs. This takes into account the unique shear and veer inflow velocity profiles of each CNBL condition. In other words, the variations in the power outputs due to variations in the inflow are excluded from the analysis of blockage effect. Note that η_{nl} can also be estimated based on the power output of an isolated wind turbine simulated in the same conditions. This approach is computationally taxing in this study as each of at least the \tilde{H}_i and S_h cases would require a separate simulation. The wake efficiency (η_{wf}) is the ratio of total wind farm power output to the power output of the total number of wind turbines in the farm (N_t) operating in the 1st row; meaning a scenario where no turbine is waked.

Figure4 shows these efficiencies as a function of the non-dimensional parameters. It can be seen that η_{nl} improves slightly opposed to slightly decreasing trend for increasing values of Fr and Fr_i . Therefore, η_t remains roughly constant. There is a clear increase in the η_{nl} for increasing values of \tilde{H}_i , while η_w decreases slightly, resulting in an overall improved farm efficiency in deeper boundary layers. Non-local efficiency is roughly constant for varying S_h , though improved wake efficiency causes the total wind farm efficiency to improve too. This confirms that variations seen on row-averaged power plots against S_h are mainly due to the turbines operating in higher wind speeds."

Response to Reviewer 2

The authors thank the reviewer for the time dedicated to revising the article and suggesting valuable feedback to improve this research and the article. We proceed with answering and clarifying their comments, where possible.

For ease of tracking, our response to each comment is denoted in black, while the reviewer's comments are denoted in blue. The detailed changes made to the manuscript are included in the "track changes" section at the end of this document.

Line 56: There should be a space between “turbines” and “(Smith, 2023)”.

This technical issue is fixed.

Lines 87-88: I struggled a bit with the “dimensional” vs “non-dimensional” classification of various papers. First a minor point: the “and” in “either dimensional and non-dimensional quantities” should probably be an “or”. Plotting the results against non-dimensional quantities is an important part of Lanzilao and Meyers (2024), though I think it is fair to say that dimensional quantities were the quantities specifically varied in the parametric study. Finally, for examples of papers focusing on non-dimensional quantities, should Allaerts and Meyers (2019) be included? Allaerts and Meyers. Sensitivity and feedback of wind-farm-induced gravity waves. *JFM*, 2019.

We can see the confusion caused by the way we phrased this paragraph. As the reviewer has indicated, the bases of this classification is clearly rooted in whether the cited studies varied dimensional or non-dimensional parameters. The reviewer also rightly highlighted (Allaerts and Meyers, 2019) to be listed as study that took the non-dimensional approach in their investigation of AGWs using a three layer model. Similarly, Stipa et al. (2024) used the Multi-Scale Coupled Model to investigate AGW impacts on wind farms in relation to non-dimensional quantities. Thus, the rephrased paragraph with these two studies cited, now reads as follow.

"Past studies of AGWs focused on their impacts on wind farms by varying either dimensional or non-dimensional quantities. Allaerts and Meyers (2018) and Lanzilao and Meyers (2024) varied dimensional quantities of interest including ABL depth and thermal stratification strength of the capping inversion and free atmosphere. In contrast, Smith (2010), Allaerts and Meyers (2019), and Stipa et al. (2024) focus on AGW impacts on wind farms by varying non-dimensional quantities. Similarly, Khan et al. (2024a,b) examined various non-dimensional quantities used to establish an optimal large-eddy simulation (LES) setup for capturing realistic AGW behavior. In this work, we adopt this non-dimensional approach to investigate AGWs and their impact on wind farm performance in a CNBL."

Line 155: “. . . with a thrust coefficient, with normalizing velocity taken at the disk, C_t' of 1.3.” This just doesn't read well. Consider re-arranging the sentence.

This sentence is rephrased as follows: "This study uses the uniform actuator disc model with a disc-based thrust coefficient (i.e, velocity sampled at the disc), C_t' of 1.3."

Line 194: “Hi is capping inversion mid height”. Two things here. Firstly, there should probably be a “the” between “is” and “capping”. Secondly, could you comment in your response on using the mid-height for the Froude number (a choice I support) relative to line 110, which reports on two papers that “describe interfacial waves as two-dimensional waves somehow bound to the top of the capping inversion”?

The semantic error is fixed and following is our interpretation—implied from Section 2: Different Kinds of Wind Farm-Induced AGWs—of interfacial waves and their location in relation to the capping inversion. The capping inversion is an interface between the ABL and the free atmosphere, especially that its depth is considered irrelevant in wind farm studies and often fixed at 100 m. Therefore, studies on wind farm-induced interfacial waves consider them analogous to the capping inversion shape, hence their shape in streamwise and vertical directions is most important to the wind farm inducing them. We also think that interfacial waves are linked to the capping inversion but they aren't 2D. To our understanding, interfacial waves occur along the capping inversion, both below and above, as they are consequence of response of the adjacent flow to its displacement. This means they occur in the free atmosphere and the ABL and there is no reason why they cannot be 3D. It is just that their lateral shape is mostly the same over the wind farm width, thus a cross-section is sufficient to characterize them, as can be seen in Figure 1 (b).

Line 225: The “s” in “Ls” should be a subscript.

The semantic error is fixed.

Line 310: “AGWS” should probably be “AGWs”

The semantic error is fixed.

Line 336: To my ear, “constraining” would probably be better than “to constrain”.

This suggestion is implemented.

Line 376: For clarity, “turbine” should probably be replaced with “row” or “row of turbines”. Same is true for line 377.

This suggestion is implemented, we now use "row of turbines", as the plots show row-averaged power.

Line 384: When I hear “power curve”, my mind goes immediately to the power vs freestream wind speed curve commonly used to define the power performance of an individual turbine. However, in the context of this paragraph, a power curve clearly represents something else (row-by-row variation in temporal and row-averaged power). I suspect that the “power curve” labels could also make other readers pause. You may wish to consider writing something like “row-by-row power output” instead.

This is well spotted by the reviewer and we change all instances of "power curve" used in the context of row wise power variations to "row-wise power output".

Lines 406-409: This text focuses on Figure 7 and whether and to what degree there are spurious AGW reflections. In a couple of places, the text highlights that these reflections can be seen in Figure 7. Is there anything the authors can do to help the reader understand the features of these plots that signal a reflection? I do not doubt that the text is correct about reflections occurring in these two cases (top and bottom plots in Figure 7). I just do not trust that I am able to positively identify the telltale signs of reflections in these plots.

This is a fair point noticed by the reviewer and we addressed it by including contour plots showing upward propagating (i.e, natural waves) and downward propagating (i.e, reflected waves) for each ξ in the same figure, which is given below. Please note that we follow ξ values recommended by Khan et al. (2024a), who have thoroughly shown higher reflections for very low and high values of ξ than that of ξ in the range of 1-10.

"As shown on the temporally averaged vertical velocity plots in Fig. 3, the AGWs reflect the most from the top boundary when the damping strength is the weakest or too strong. Damping strength is described by the

non-dimensional value $\xi = 1/(\tau N)$. For the range of ξ from 1.0– 10.0, the waves are damped significantly, and the AGW amplitude decreases towards the top boundary. Using $\xi > 10$ makes the T-RDL damping too strong and the AGWs reflect off it, as seen in Fig. 3 for $\xi = 50$. This can be seen on the plots for the downward waves where there is a phase shift in the T-RDL when $\xi = 0.1$ as the waves reflect from the top boundary and inside the non-damped layer when $\xi = 50$ as the waves reflect off the T-RDL itself. For $\xi = 1.0$ – 10.0, the wave fronts show no phase shift. Note that the strength of downward waves is roughly the same for all values of ξ , as the reflected wave energy is in range of 8 to 11.5% of the upward wave energy. See (Khan et al., 2024a) for more details about extracting the upward and downward waves from the vertical velocity.

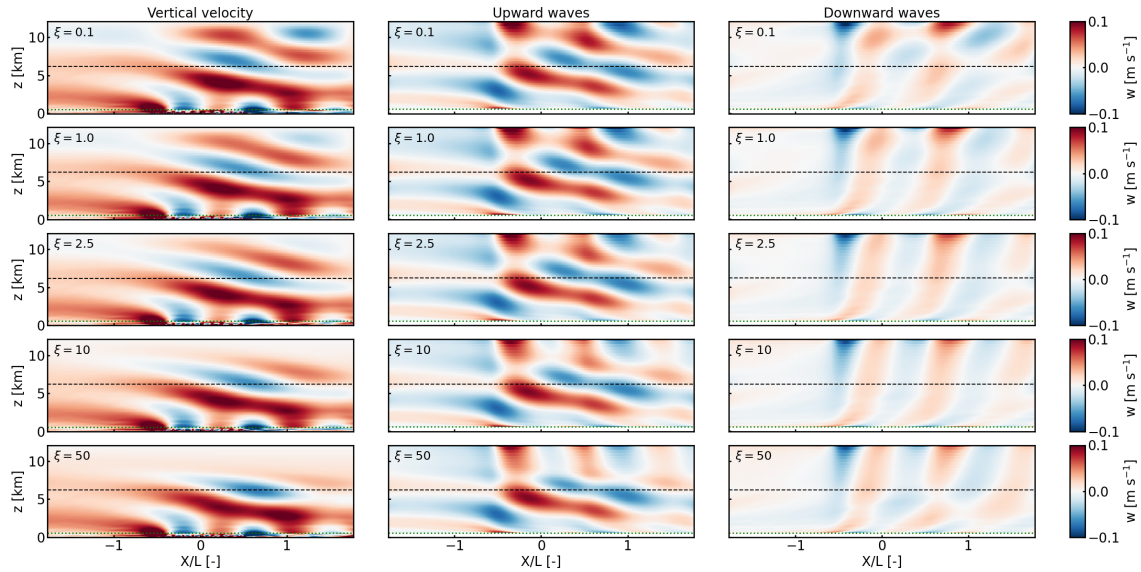


Figure 3. Contours of temporally averaged vertical velocity, upwards, and downwards propagating waves in a streamwise-oriented vertical plane for varying Rayleigh damping strength (ξ) of the T-RDL. The flow conditions are defined by $Fr = 0.15$, $Fr_i = 1.3$, $\tilde{H}_i = 0.087$, and $S_h = 0.0255$. The green and black dashed lines show the capping inversion mid-height and start of T-RDL, respectively.

"

Figure 6b. The variation in power production between the five cases is small. (The case-to-case power variation appears to be much less than in Figure 16a and Figure 16b, where the authors see little variation in the wind farm efficiency.) It is almost as if practically speaking the damping strength doesn't matter in this case. Do you think this is generally true or is there something about this case that makes the turbine power predictions generally insensitive to the damping strength.

It is generally true that LES cases are contaminated by spurious waves triggered at the inlet, which are contained in the advection damping layer. This, leaves behind reflections mainly from the top boundary, that are dependent on the damping thickness and coefficient. Since the damping thickness is fixed to greater than a vertical wavelength, as recommended by Khan et al. (2024a), damping coefficient has little impact on the flow around the wind farm. In other words, these simulations already use setups that are well tuned for damping parameters except the damping coefficient. From experience we know that damping thickness makes a bigger impact than damping coefficient.

Line 464: "In a physical sense, Fr_i approaching 0 would mimic a rigid lid that would barely move due to the disturbance from a wind farm." I think this line might be OK, but I'm not sure. In this study, Fr_i is varied by adjusting $\Delta\theta$ (change in potential temperature across the inversion). If we drive Fr_i in the

direction of zero by strengthening the inversion in any sort of realistic way, aren't we creating a situation that is more like free surface with a fluid of uniform density below?

We think Fr_i approaching zero for a constant capping inversion height means the temperature jump across it is infinity or enormously high. That would mean an extremely stable layer that would mask any perturbations from the ABL irrespective of the source of perturbation. This means vertical motions are suppressed and the flow has to accelerate in the horizontal direction to ensure continuity. In this sense, a case with $Fr_i = 0$ is similar to a rigid-lid that causes the channeling effect.

Lines 449-500: Would replacing "calculated" with "theoretical" and "measured" with "simulated" help avoid confusion?

This is indeed confusing and is implemented as suggested.

Line 507: It is not immediately obvious what is meant by the capping inversion becoming "more sensitive" for sub-critical values of Fr_i . I believe the authors are saying that the displacement of the inversion layer due to the presence of the wind farm is more pronounced. Instead of "the capping inversion becomes more sensitive", it might be better to write something more specific and less open to interpretation.

It is right that using phrase "more sensitive" is vague. We explicitly make our point now, which is phrased as: "We recall from Section 4.3.1 that the capping inversion strongly suppresses the perturbations which is indicated by stronger trapped waves for sub-critical values of Fr_i . Also, it should be recalled that the interfacial waves are smaller for sub-critical than for super-critical values."

Line 509: "... showed that lambda approaches the streamwise turbine distancing as if each turbine row is triggering an interfacial wave." I *think* I know what the authors are trying to say here, but I'm not sure. Perhaps with some small tweaks readers may be more likely to get your point quicker. For example, "... showed that lambda is on the order of the distance between turbine rows – as if each row is triggering an interfacial wave."

Yes, that is what we were trying to convey here, and we rephrased the sentence as suggested.

Lines 526-527: I had to go back to Lanzilao and Meyers (2024) to decipher what this sentence is referring to. I believe the sentence is referring to the orientation of the waves along the inversion north and south of the wind farm when viewed from above. Lanzilao and Meyers call it a V-shaped pattern. Now after checking the Lanzilao and Meyers (2024) the meaning of this sentence seems clear, but for the reader who does not have a picture in their head of what this refers to, more description may help. Perhaps the word "inclination" set me down the wrong mental path, as I typically think of an inclination being in a vertical plane.

The reviewer has rightly indicated to better describe the interfacial wave's V-pattern. We give the following description now.

"Moreover, the orientation of interfacial waves with respect to lateral axis (i.e. spanwise direction) are dependent only on Fr_i and have already been investigated by Lanzilao and Meyers (2024). When looking from the top, interfacial waves make a V-pattern with the vertex at the induction zone of the wind farm, and the wave fronts extending downstream. The angle on the interior of the V-pattern at the vertex is inversely proportional to Fr_i , such that the V-pattern becomes increasingly acute for super-critical values. For sub-critical to critical values of Fr_i , the interfacial waves are blunt at the vertex (i.e. parallel to the lateral axis)."

Lines 556-557: "For instance, the lamda/L plot against Fr_i resembles the decreasing inclination angle trend against Fr_i shown in Fig. 13." I believe the lamda/L plot against Fr_i refers to the top right plot in Figure

14. It is not clear to me how this plot resembles the top right plot in Figure 13. Am I missing something here?

Again, the reviewer has rightly spotted lack of description. Firstly, this is an inference, which wasn't mentioned. Secondly, the basis of this inference wasn't explained at all. We have made it clear now and these sentences now read:

"The values reported for $Fr_i = 0.25$ and 0.5 are estimated with less confidence, as negligible internal waves were observed for these idealized cases, as can be seen in Fig. 12. Small variations about the average value (i.e. $1.0L$), which is roughly the value for $Fr = 0.15$, for varying Fr_i , \tilde{H}_i , and S_h are the impacts of the interference of the two wave trains that depend on the inclination angles. For instance, the λ_{igw}/L plot against Fr_i for its values ≥ 0.75 resembles the decreasing inclination angle trend against Fr_i shown in Fig. 13. Both plots asymptote at $Fr_i = 1.0$ after a sudden drop. Note that we infer this resemblance for data points excluding the less certain values for 0.25 and 0.5 , for reasons mentioned earlier."

Line 596: To avoid confusion, I recommend replacing "a higher capping inversion" with "an upwards displacement of the capping inversion."

This suggestion is implemented.

Line 619-620: "However, the power profile for the shallowest ABL ($H_i = 0.042$) is most affected by localized capping inversion displacement, which can be revisited in Fig. 10." Figure 10 shows maximum capping inversion displacement, but the sentence is talking about localized capping inversion displacement. Should the reader then be directed to look at Figure 9?

The referencing error is well spotted, and we have fixed it.

Lines 654-655: "Whereas the amplitudes grow stronger for shallower inversions." This is a dependent clause. It should not stand alone as a complete sentence. That said, I think it is OK to break grammatical rules under certain circumstances. The text is clear enough. I just wanted to point this out in case this was done accidentally rather than on purpose. There is another issue with this line: the term "shallower inversions". I am pretty sure this refers to inversions that are closer to ground. Up until this point in the manuscript, this situation was described as a shallower ABL. When I first read this, I thought "shallower inversion" might refer to the thickness of the inversion, but of course, inversion thickness is not investigated in this study, so it almost certainly refers to ABL thickness/depth. "Shallow inversions" come up again in the next paragraph. I recommend switching back to shallow boundary layers or shallow ABLs for consistency and to avoid confusion.

Well, the phrase is intentionally kept alone in the view of concise writing. We implemented the suggestion related to switching to a better phrase for "Shallow inversions" as we now use "shallow boundary layers".

The one major concern has to do with analyses and conclusions related to wind farm "performance" and "efficiency". Figure 16 shows time-and-row-averaged power vs. row for four different parametric studies. These plots seem to be used in support statements in the paper related the impact of the parameters on wind farm performance and efficiency. I think that "performance" and "efficiency" mean the same thing in this paper, but I am not 100% sure. These terms should be specifically defined.

Wind farm efficiency is traditionally defined as the power of the wind farm divided by the sum of the power the turbines would produce if they were operating in isolation. If turbines were run in isolation for each of the simulated conditions, I recommend normalizing the power values in figures 6 and 16 with the isolated power values. Simulating each wind farm turbine in isolation for each set of inflow conditions is not practical; the simulation of just one of the wind farm turbines in isolation may be enough if the time-mean flow is sufficiently horizontally homogeneous.

If isolated turbine simulation results are not available and producing the results deemed to be too taxing, an

alternative power normalization representing a relevant reference power of some sort for each case could still be useful. The authors acknowledge this general issue on Line 641: “Sh can affect the power output mainly because of placing the wind turbines at higher or lower wind speeds, which has little to do with gravity waves or blockage.” This is no doubt true. What about the other sensitivity studies? Would the power of an isolated turbine be the same across all the Hi cases, for example? When Hi varies, the shear and veer will probably vary, which could change the power across different cases for a given hub-height wind speed. (The inflow turbulence will probably vary as well, which may affect wake recovery.)

There are two key statements in the Conclusions section about the impact of these various non-dimensional parameters on wind farm efficiency (there are also some in the last paragraph of the abstract): Statement 1: “Wind farm performance was most sensitive to the ABL aspect ratio: shallow inversions increased wind farm blockage and reduced wind farm efficiency. . .” In addition to defining wind farm efficiency, the definition of wind farm blockage should be clarified. At the least, the reader needs to understand how the authors reached the conclusion that shallow boundary layers increased wind farm blockage. Is wind farm blockage the amount by which blockage reduces production in the leading row? If so, how is the reduction quantified? Relative to isolated operation? I suspect the wind farm blockage claim is based on the power variation across the cases in the first row. Since the freestream, hub-height wind speed is the same across these cases, one might attribute the power variation to wind farm blockage. I suspect this, in fact, is the dominant factor explaining the power variation in the first row, but based on the information in the paper, I cannot rule out another contribution: different inflow conditions could result in different powers for turbines simulated in isolation (power variation unrelated to wind farm blockage), as the power at the actuator disks is based on the velocity across the disk rather than the hub-height freestream wind speed. Statement 2: “. . . the total wind farm efficiency—which accounts for both wake losses and stratification effects—remained nearly constant with respect to Fr and Fri, but improved with increasing Hi, and Sh.” Again, how was wind farm efficiency in these cases determined? It is not clear from the text or the figures, especially for the Sh sensitivity plot in Figure 16. In fact, I think the only places efficiency is mentioned are in the abstract and in the conclusion.

On a related note, the abstract claims that deeper layers enhance wake recovery. I think that is true, but I didn’t see this specifically demonstrated and called out in the paper. How specifically is the amount of wake recovery determined?

To summarize this concern, it is not clear how the conclusions regarding wind farm performance, wind farm blockage, and wind farm efficiency are supported (or even how these metrics are defined). I suspect that the conclusions are based on Figure 16, but from these plots it is difficult for me to discern case-to-case power variation due to wind farm effects from case-to-case power variation due to differences in the wind conditions that an isolated turbine would experience (particularly for the sensitivity studies where Hi and Sh are varied). Lastly, if efficiency/performance trends remain a key point of discussion in the paper, I recommend quantifying the wind farm efficiency for each case (same for front-row blockage loss if it is discussed). Even if the row-by-row data in Figure 16 were normalized by isolated power, for example, it may not be straightforward for someone to mentally translate the plots into reliable efficiency/loss trends for the full wind farm.

We agree with the reviewer that the conclusions made require the reader to extract efficiencies from the power plots. This raises all the questions indicated by the reviewer. Thus, we have included a section on wind farm efficiencies that describes, wind farm blockage effect and the efficiency. Figure 17 provides the evidence of the conclusions drawn regarding efficiencies as a function of the non-dimensional parameters. Note that we didn’t normalize powers in Fig. 6 and 16, as we want to report the actual power values in case someone wants to reproduce or requires the results in advancing this study. Therefore, we added a section on wind farm efficiencies, given below, which addresses the comments and suggestions given by the reviewer.

"

0.0.2 Wind Farm Efficiencies as a Function of Physical Non-Dimensional Parameters

Although wind farm performance is thoroughly discussed in the previous section, it is essential to quantify wind farm blockage effect and wake losses relative to the inflow characteristics of each case. This is important because varying atmospheric conditions leads to different inflow characteristics, such as different shear profiles and veers. (Allaerts and Meyers, 2017) proposed wind farm efficiency (η_t) as the product of non-local and wake efficiencies, the former being an estimate of wind farm blockage effect and the latter to quantify wake losses. These efficiencies are defined as following.

$$\eta_t = \eta_{nl} \cdot \eta_w, \quad \eta_{nl} = \frac{P_1}{P_\infty}, \quad \eta_w = \frac{P_{wf}}{N_t \cdot P_1}$$

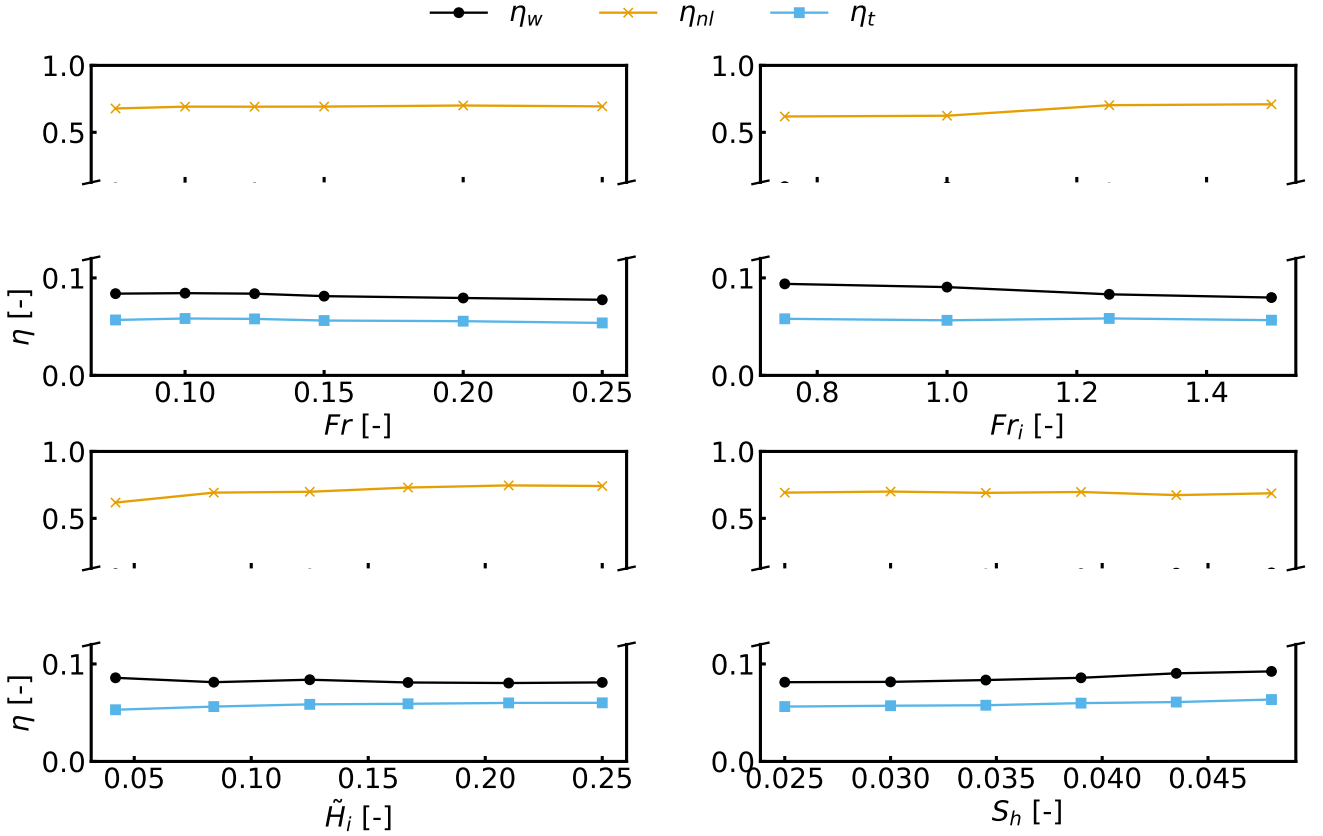


Figure 4. Wake, non-local, and total wind farm efficiencies as a function of Fr , Fr_i , \tilde{H}_i , and S_h .

The non-local efficiency (η_{nl}) is the ratio of average power of the 1st row turbine (P_1) to the power produced by a turbine in the free stream or isolation (P_∞). Since the LES power predictions are based on rotor-average velocities, we calculate P_∞ as a function of rotor equivalent wind speed in the free stream. To this end, we sample velocity over the rotor annulus for each case from the respective precursors and temporally average it over the last flow through time of the successor simulations as this interval is used to calculate the actual power outputs. This takes into account the unique shear and veer inflow velocity profiles of each CNBL condition. In other words, the variations in the power outputs due to variations in the inflow are excluded from the analysis of blockage effect. Note that η_{nl} can also be estimated based on the power output of an isolated wind turbine simulated in the same conditions. This approach is computationally taxing in this study as each of at least the \tilde{H}_i and S_h cases would require a separate simulation. The wake efficiency (η_{wf}) is the ratio of total wind farm power output to the power output of the total number of wind turbines in the farm (N_t) operating in the 1st row; meaning a scenario where no turbine is waked.

Figure4 shows these efficiencies as a function of the non-dimensional parameters. It can be seen that η_{nl} improves slightly opposed to slightly decreasing trend for increasing values of Fr and Fr_i . Therefore,

η_i remains roughly constant. There is a clear increase in the η_{nl} for increasing values of \tilde{H}_i , while η_w decreases slightly, resulting in an overall improved farm efficiency in deeper boundary layers. Non-local efficiency is roughly constant for varying S_h , though improved wake efficiency causes the total wind farm efficiency to improve too. This confirms that variations seen on row-averaged power plots against S_h are mainly due to the turbines operating in higher wind speeds."

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