University of British Columbia

UBCO-UL NSERC Alliance Grant “Reduced-Order Models of Wind Farm Induction and Far-Field Wake Recovery”

Response to Reviewer 1

Exec. S. Stipa - May 7, 2024

We would like to thank the reviewer for the time dedicated to revising the paper. We proceed with answering and clarifying, where possible, their comments.

Our response, denoted in black, is shown below, while the reviewer's comments are denoted in blue. Please refer to the track changes document for a detailed overview of the changes made to the manuscript.

Line 280: the authors mention that "The cells in between will be displaced between zero and η depending on their distance from the wall". However, to my understanding, the vertical cells are not only displaced but also stretched to account for the changes in the vertical domain height (which depend on η). Is this correct?

The reviewer is correct in pointing out that this sentence is still not clear enough. The sentence has been corrected, highlighting that indeed is not the mesh cells which are displaced, but rather the mesh points. As a consequence, cells are stretched vertically with respect to the starting configuration where the mesh is initially cartesian.

Line 444-461: I would suggest expressing the differences in pressure between the various cases in percentage since stating "small pressure deviation" or "large differences" can be subjective. I would suggest to apply this change throughout the manuscript, where possible.

We implemented this comment throughout the manuscript, removing the instances of small/smaller and large/larger where possible. Regarding the specific line pointed out by the reviewer, we emphasize that providing the difference in percent of the pressure calculated from the AGW-resolved simulations is not a good metric, as deltas would go to infinity when the pressure perturbation values cross the zero perturbation line (due to a division by zero). Instead, we added the pressure delta in percent of ρu_{ref}^2, which is also consistent with the plots.

Line 470: note that a difference of 5% in terms of velocity causes a difference in power output of 15%, which is substantial.

We agree with the reviewer. We also highlight that our percentages are calculated relative to the local velocity, instead of u_{ref}. This means that a constant difference in velocity between the two plots would see an increase in relative error inside the wind farm, where the local velocity is lower. We are comfortable with keeping this metric in the manuscript, but we are also open to change it if the reviewer thinks that dividing by u_{ref} instead of $u(x)$ would make the plots more clear.
On a different note, differences in velocity around 5% (and more) can be also due to the different inflow used between the AGW-resolved and AGW-modeled simulations, as shown in Appendix B of the manuscript, so this does not mean that our approach implies a difference in power of 15%. Specifically, while the exact absolute value of velocity — and hence power — depends largely on the specific inflow used, our objective here is to show that our approach can capture the effect of AGW on the velocity gradients around the wind farm and on the row-by-row thrust and power distributions. These can be observed from Fig. 5 and 6, respectively, where we believe that the validity of the proposed approach can be appreciated.

Figure 6: it could be useful to also include the relative error, as done for Figure 5.

As mentioned above, we believe that only focusing on the relative error between the AGW-modeled and AGW-resolved approaches can be misleading, as differences also contain the effect of using a different inflow. For this reason, we do not agree with the reviewer that adding the relative difference would add clarity to our results. Perhaps the relative difference in Fig. 5 caused some confusion. However, we include the relative power difference in Fig. 1 of this response, calculated as $(P^t_{AGWM} - P^t_{AGWR})/P^t_{AGWR}$ (where t is the row ID), for the subcritical and supercritical cases.

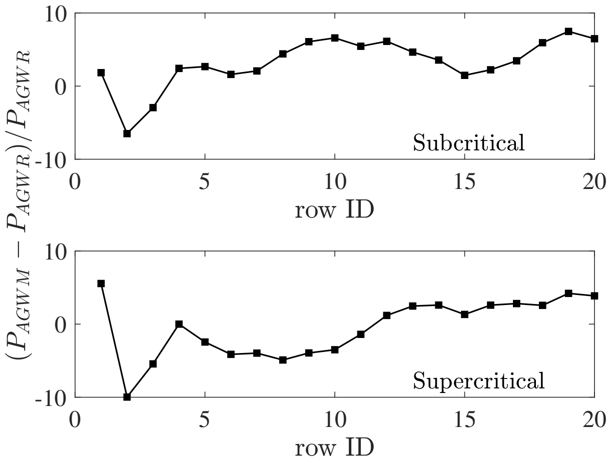

Figure 1. Comparison of row-averaged error on power for the subcritical (top) and supercritical (bottom) cases. Time averaging is performed as described in Appendix B of the manuscript.

I would suggest changing the title of Section 4.2. Here, the authors do not show results about the rigid lid approximation but rather compare these results to the ones obtained under different types of thermal stratification.

We agree with the reviewer, the title has been changed to "Implications of the Rigid Lid Approximation".

Figure 9: it is shown that the rigid lid case has a higher wind farm efficiency than the truly neutral case. Which mechanism is responsible for this behavior? Could it be that the flow speed-up over the farm (if present) in the rigid lid case enhances vertical mixing and therefore wake recovery?.

While we highlight that we did not run any truly neutral LES and so the data to precisely answer the question is not available to us, we hypothesize that the higher wind farm efficiency in the rigid lid case with respect to the fully neutral case is primarily due to the lid-induced pressure gradient around the wind farm, where the favorable gradient inside overcomes the unfavorable gradient upstream. Moreover, as momentum fluxes due to vertical mixing are mainly produced by the $\partial \overline{u'w'}/\partial z$ term in the momentum equation, where the production of $\overline{u'w'}$ indeed increases with increasing shear, the hypothesis of the reviewer might also be a factor. Nevertheless, in our experience the acceleration above the wind farm in a capped boundary layer, which arises due to continuity arguments, does not necessarily translate in more efficient vertical mixing. An example is a stably stratified boundary layer, where $\overline{u'w'}$ are quickly suppressed by buoyancy after the wind farm, causing the flow to remain accelerated in the upper layer as little to no momentum is removed by turbulence.
However, we emphasize that the exact dynamics of the shear stress evolution inside a finite wind farm under a truly neutral boundary layer has never been compared to a capped CNBL to date, representing an important topic to investigate in the future. For instance, although this question was not answered in their paper, the data from the parametric study performed by Lanzilao and Meyers (2024) can be used to address this topic.

A somewhat similar analysis to the one presented in Figure 9/10/11 has been performed by Allaerts and Meyers (2019) and Lanzilao and Meyers (2024). It would be interesting if the authors could relate their findings to the ones described in the articles mentioned above, when possible.

We expanded a bit this section with some comments that relate our findings to those proposed by the reviewer. There is one fundamental difference between the parametric study conducted by Allaerts and

Meyers (2019) and ours. Since the coupling between the wake model and the atmospheric perturbation model in the MSC model is local, it can account for both adverse and favorable AGW-induced pressure gradient effects. Conversely, the 3LM used by Allaerts and Meyers (2019) only accounts for blockage effects. As a consequence, these authors found that those conditions characterized by a high blockage are also those where the wind farm produces less. This is different from our findings, where more blockage also means more beneficial pressure gradient, leading to a higher wake recovery and a higher overall power production by the wind farm. This is true for the range of H/H_1 where the MSC is applicable, but it should be pointed out that, in general, the dominance of adverse over favorable pressure gradient also depends on the ABL height, as shown by Lanzilao and Meyers (2024). In particular, these authors show that, for very low ABL heights ($H \approx H_1$), η_{nnl} correlates well with η_{tot}, even though η_{w} increases. In this case, because wind turbines span the entire ABL height, the momentum loss cannot be replenished by vertical turbulent fluxes, leaving only the favorable pressure gradient to promote wake recovery.

Line 45: error in reference style (\cite -> \citep).

Rephrased and corrected.

Line 304: untested -> has not been tested.

Corrected.

Line 314: remove "used to compute η".

Rephrased.

Line 656: it could be more intuitive to express the time in hours instead of seconds.

We prefer to leave the time in seconds, as this is the standard SI and it is the unit of measure used in simulation codes.

Line 665: replace with "the geostrophic wind is not know a priori and it has to be retrieved by.."

It is not clear to us where the modification should be implemented.

Table 3: I would suggest replacing N1 and N2 with subcritical and supercritical, respectively.

Corrected.

Table A1: in the caption, change u\ast to u_\ast

Corrected all instances of $u*$ to u^* throughout the paper for consistency.

References

Allaerts, D. and Meyers, J.: Sensitivity and feedback of wind-farm-induced gravity waves, Journal of Fluid Mechanics, 862, 990–1028, https://doi.org/10.1017/jfm.2018.969, 2019.

Lanzilao, L. and Meyers, J.: A parametric large-eddy simulation study of wind-farm blockage and gravity waves in conventionally neutral boundary layers, Journal of Fluid Mechanics, 979, A54, https://doi.org/10.1017/jfm.2023.1088, 2024.