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 - March 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.

Abstract: the authors mention that this new technique demands less than 15% of the computational resources than traditional methods, which is an impressive achievement. However, this technique also reduces the accuracy of an LES to the one of the MSC model. I think this is quite an important limitation, which should be mentioned.

This aspect has been added to the abstract (see line 15 of the track changes document).

Line 34: the authors mention that AGWs have "extremely large spatial scales". I think the reader would benefit if a number in terms of km would be provided, as the word "extremely" can be subjective. I suggest trying to limit the use of this word throughout the manuscript (e.g. see lines 35 and 51).

The word "extremely" has been omitted and more specific details have been added (see lines 40-42 and 171-172 of the track changes manuscript).

Line 37: I would also refer to some earlier works about AGW excited by hills/mountains (e.g. Klemp and Lilly 1977, Teixeira 2014).

The reference Teixeira (2014) has been added at line 37 of the track changes manuscript, while the work from Klemp and Lilly (1978) has been cited later at line 40 to introduce the problem of wave reflection.

Lines 54-67: the authors describe the Rayleigh damping layer and fringe region technique in this paragraph. However, the descriptions are mixed so that it is easy to confuse the role of the two techniques. I would propose to re-write this paragraph explaining first the Rayleigh damping technique, for instance, and afterwards the working principle and use of the fringe region technique.

The paragraph has been almost completely rewritten following the reviewer's comment to ensure that the Rayleigh damping layer and fringe layer are distinctly described. Please refer to the track changes document.

Line 78: the authors define the rigid-lid approximation as a case with strong free atmosphere stability. However, this could also be seen as a case with a very strong (practically infinite) capping-inversion strength. To avoid confusion, I would define the rigid-lid case as an approximation for a case with very strong thermal stratification above the atmospheric boundary layer.

The comment has been implemented in the revised manuscript. Please see lines 24 and 122 of the track changes document.

Introduction: I suggest to include the general aim of the article in the first paragraph of the introduction. This will give the reader a hint about why AGW numerical models and boundary conditions are described in this section.

The comment has been implemented in the revised manuscript. Please see lines 45-47 of the track changes document.

Line 107: I would mention this in the introduction, i.e. that the article only deals with CNBLs.

The comment has been implemented in the revised manuscript. Please see lines 143-144 of the track changes document.

Section 2.2: the proposed technique uses the MSC model to predict the capping-inversion vertical displacement (η) and only simulate the flow within the ABL with LES. Hence, it is implicitly assumed that $H > H_1$, with $H_1 = 2z_{\mathrm{hub}}$. However, H_1 values are getting closer to heights at which the capping inversion is typically located (for instance, the Vestas V172-7.2MW turbine has a hub height of 199 m, a figure that may increase in the coming years) so that cases where the ratio $H/H_1 \approx 1$ are realistic. In the current work, the authors fix H/H_1 to 2.77 and do not discuss this further. However, I believe that the performance of the new technique is sensitive to the H/H_1 ratio. Would it be possible to add a few comments and/or simulations on what would happen when $H/H_1 \approx 1$? Also, how would this technique deal with cases where $H/H_1 < 1$? If this is a limitation, it should be reported in the text.

This comment has been now addressed in the manuscript as follows, between lines 349 and 363 of the track changes document). The case where $H/H_1 \approx 1$ "corresponds to a situation where the turbine top tip almost pierces the inversion layer, with consequent disappearance of the upper layer. Devesse et al. (2023) developed an alternative strategy to the one used in the MSC model to couple the 3LM of Allaerts and Meyers (2019) and the Bastankhah and Porté-Agel (2014) wake model, which also uses the 3LM to address AGW effects. When validating this new model against wind farm LES characterized by $H = 150, 300, 500$ and 1000 m and $h_{hub} = 119$ (Lanzilao and Meyers, 2023), the authors excluded those LES cases with $H/H_1 = 0.63$ ($H = 150$ m). Among the remaining cases, the model showed the highest deviation from the LES when $H/H_1 = 1.26$ ($H = 300$ m). As also the MSC model uses the 3LM to model AGW effects, these results suggest that the MSC model will loose accuracy when $H/H_1 \lesssim 1.5$. In the present manuscript, the dependency of the proposed technique to the ratio H/H_1 is not investigated and this number is fixed to 2.78. We highlight that this is a limitation of the MSC model used to compute η. If η could be evaluated by different means (e.g. with a coarser AGW-resolved LES employing a simple canopy model) at a height located above the inversion layer, the AGW modeling approach could be used for small H/H_1 ratios by placing the upper boundary a few hundreds meters into the free atmosphere and by including the potential temperature transport equation."

Line 184: the authors mention that "Then, the vertical displacement is linearly distributed to the underlying cells, deforming the mesh before starting the simulation". This passage is not clear to me. Would it be possible to explain in more detail?

This aspect has been clarified in the manuscript. Please see lines 322 to 324 of the track changes document. Specifically, the upper boundary initially located at H is vertically displaced according to η before starting the simulation, following which it remains fixed, as the applied η corresponds to the steady state solution obtained with the MSC. Then, the vertical displacement applied to the top boundary is linearly distributed to the underlying cells. This means that, at each horizontal location, the first cell away from the bottom wall is not displaced at all, while the top cell moves vertically by η. In between, the cells are vertically displaced by a distance Δd that is calculated based on their distance from the wall as $\Delta d(\mathbf{x}) = z/L_z \cdot \eta(x, y)$, where L_z is the local vertical domain size and Δd is the vertical displacement at $\mathbf{x}$.

Line 193: in the LES framework, the vertical displacement of the capping inversion generates a cold anomaly, which in turn results in pressure perturbation. However, the authors mention that in CNBLs, the potential-temperature equation can be left out in the AGW-modelled simulations. Hence, are the pressure perturbations solely caused by the flow convergence/divergence caused by the irregular top edge of the main domain? How is the buoyancy term computed in the vertical momentum equation? Or is it neglected? I would appreciate a more detailed explanation.

On lines 140-144 and 341-342 of the track changes document, we state that under CNBLs there is no need to solve the potential temperature advection equation, as the flow is neutral everywhere except close to the top boundary. For those conditions where $H/H_1 \approx 1$, the upper boundary should be moved a few hundreds meters into the free atmosphere and so potential temperature must be solved and η cannot be calculated with the MSC model anymore (though a coarse LES using a canopy model might be used). This aspect is addressed at the end of Section 2.2.

Moreover, a more detailed explanation on the relation between flow convergence/divergence and AGWs is provided between lines 328-333 of the track changes manuscript, when talking about the rigid lid. To summarize, buoyancy is not required to capture AGW effects inside the ABL, as these are given by flow divergence/convergence of the ABL top. In fact, the pressure disturbance produced within the ABL by AGWs in the free atmosphere has to coincide with the pressure produced by flow convergence/divergence, otherwise the governing equations (Equations 9 and 10 of the revised manuscript) are not satisfied. In particular, there is a unique η that satisfies this condition, which is the one that we impose using the MSC model. This whole reasoning is the backbone of Section 2.2 and it is shown using the simple model derived by simplifying the 3LM model of Allaerts and Meyers (2019).

Line 194: the authors mention that "This condition is only violated very close to the top boundary, where discrepancies in turbulent fluctuations produced by the absence of stability and by the physical boundary are deemed acceptable as they happen away from the wind farm". I would note that this affirmation does not hold for low H/H_1 ratios (as mentioned in comment 8).

We agree with the reviewer, but in this case the main problem would be not being able to use the MSC model to compute η. To extend our approach to low H/H_1 ratios, we would advocate using a coarse LES that employs a canopy model to run a computationally cheaper AGW-resolved simulation. Under those conditions, the domain in the AGW-modeling method can be truncated a few hundred meters into the free atmosphere, instead of at H, as the streamline displacement is available here from the AGW-resolved LES. Of course, potential temperature transport has to be retained in this case even if a CNBL is simulated. This suggestion is given at lines 359-363 of the track changes document.

Section 3.2: if I understand correctly, the inflow data used for the AGW resolved and modelled simulations are computed with two different precursor techniques. Hence, differences in the wind-farm simulation results between the two techniques could be also attributed to this difference in inflow conditions. Why the authors do not drive the AGW-modelled simulations with inflow sections taken from the precursor simulations used for the AGW-resolved simulations?

We agree with the reviewer that this would have been the best approach. However, the inflow data used for the AGW-resolved cases is not available as it was generated at runtime during the simulations (these employed a concurrent precursor method) and not saved to slices. Hence, the approach followed in the manuscript is arguably the best alternative. A supporting rationale has been added to the paper (lines 431-434 of the track changes document).

Line 264: the authors mention that "we used the velocity inflow data of the subcritical case to prescribe an inlet for the rigid lid". Is the velocity profile in the precursor domain for sub- and supercritical cases equal? Showing some vertical profiles of the precursor simulation would be beneficial.

The analysis required by the reviewer has been added to the revised manuscript in Appendix A.

Figure 2: I suggest to center the colorbar around the zero value (so that the background color is white).

The reviewer's comment has been implemented in the revised manuscript.

Line 274: the authors mention that "the AGW-modelling technique requires a domain that is more than 85% smaller compared to the AGW-resolving approach". In which terms? Number of cells? Does this number also consider the precursor domain (for instance, the AGW-modeled simulations use a precursor domain 7 times bigger in the y-direction)?

This comment has been addressed in more detail in the revised manuscript (see lines 479-482 of the track changes document).

Figure 3: How would you explain the differences between AGW-modelled simulations and the MSC model? Are those due to the simplifications made in the MSC model (for instance, linearity, absence of resolved turbulence, etc..)?

We explain the differences as follows. The AGW-modeled and MSC model feature the exact same η, but a different level of fidelity inside the boundary layer. Hence, the same η does not lead to identical pressure and velocity perturbations. Conversely, the AGW-modeled and AGW-resolved cases use the same model inside the ABL, but η is slightly different, as it comes from the MSC model in the former and it is resolved in the latter. As a consequence, mass and momentum conservation show some differences in the perturbation velocity and pressure. This explanation has been added to the revised manuscript (see lines 518-529 of the track changes document).

Figure 3: I suggest extending the caption of this figure, mentioning for instance that the profiles shown are averaged in height between H1 and H and along the wind-farm width. This comment extends to the whole manuscript since I feel that the combination of figure and caption is often not self-explanatory.

The reviewer's comment has been addressed in the revised manuscript and further extended to all figure captions. Please refer to the track changes document.

Line 304: the domain in Figure 3 is too small to appreciate this behavior.

We have rephrased by pointing at Figure 3 in the revised manuscript, which corresponds to Figure 2 of the original manuscript (i.e. the entire η fields for the subcritical and supercritical conditions).

Line 313: at which height are the profiles taken? I suggest mentioning it in the manuscript.

They are taken at the hub height. The reviewer's comment has been implemented in the revised manuscript (see line 537 of the track changes document).

Figure 4: the match in terms of velocity is really good. This makes it hard to spot differences between AGW-modelled and resolved simulations in Figure 4, which obviously is a good sign. Could be an idea to also plot the relative error for both cases? This will allow the reader to easily understand where the two methods differ the most.

The reviewer's suggestion has been implemented in the revised manuscript.

Table 3: Would it be possible to include the non-local, wake and farm efficiency values in this table (as defined in Lanzilao and Meyers (2024), for instance)? The total farm power (and farm efficiency) in the supercritical case is almost identical in the AGW-M and AGW-R cases. However, I expect some differences in non-local and wake efficiencies.

The reviewer's comment has been addressed. However, since the inflow data relative to the AGW-resolved simulations is not available, it is impossible to conduct isolated with turbine simulations to compute P_∞, as done in Lanzilao and Meyers (2023). For this reason, in order to compute the efficiency, we used the data from Appendix B of Stipa et al. (2023), which are evaluated with uniform inflow and in absence of turbulence. We agree that this would lead to values of η_{nnl} and η_{tot} that are different from the actual figures, but since the same procedure to compute P_∞ has been used for all the entries of Table 4 of the revised manuscript, comparisons can still be drawn (lines 561-586 of the track changes document). In particular, the ability of the model to capture the underlying physics is confirmed by noticing that the AGW-modeled and AGW-resolved simulations lead to the same conclusions regarding which case is characterized by the highest wake efficiency, blockage effect and total wind far power.

Section 4.2: I'm assuming that the rigid lid is located at $H = 500$ m. Is this correct? I suggest to explicitly mention the vertical extent of the domain in the text.

Correct. This information has been added (see line 595 of the track changes document).

Section 4.2: the pressure build-up in the rigid-lid case is solely attributed to flow confinement. However, even in neutral conditions, the cumulative turbine induction generates a pressure build-up and consequently, a flow slow down (typically much lower in values than the one observed in the presence of thermal stratification). Therefore, I would rephrase this sentence and/or find an alternative method to evaluate the flow blockage solely induced by the flow confinement.

This section has been heavily modified (please see the track changes document). To specifically address the reviewer's comment, we would like to highlight the statement added at lines 605-609 of the track changes document. In particular, global blockage effects is always due to flow confinement, which is an alternative way of referring to the AGW-induced pressure gradient. In fact, the two are uniquely related, as explained in Section 2.2. Hence, in the rigid-lid global blockage is generated in the exact same manner as in the full AGW solution, with the only difference being that flow confinement is approximated to that produced when $\eta = 0$. This implies that the flow is horizontally divergence free in the rigid lid (i.e. on wall-parallel planes), while continuity is satisfied on pliant surfaces defined by η in the full AGW solution (i.e. surfaces coincident with the local vertical streamline displacement). Notably, both induce global blockage due to flow confinement or, alternatively, to stability effects above H, but the rigid lid corresponds to the limiting case where $\Delta\theta \to \infty$ and/or $\gamma \to \infty$.

Line 350: Which is the MSC setup used to generate Figure 8? In the text, only the capping-inversion strength and free lapse rate are mentioned. In general, I would appreciate more details on the simulation setup, so that it would get easier and more intuitive to reproduce the results.

The reviewer's request has been implemented throughout the revised paper (see for instance Table 3 of the revised manuscript).

Figure 8: it would be interesting to split this figure into three panels, reporting the sensitivity of the relative error based on non-local, wake and farm efficiency to the capping-inversion strength and free lapse rate. I suggest this because at times the total farm power of two simulations can be almost identical, even though the power trends are very different (two behaviors that cancel out).

The reviewer's request has been addressed in the revised manuscript. In particular, instead of computing the error between the different models, 1D parametric analyses have been conducted (Figures 9 and 10), where the different approaches are directly (and more visually) compared. Moreover, Figure 8 of the old manuscript has been removed and substituted with the sensitivity of η_{nnl}, η_w and η_{tot} to the parameters $\Delta\theta$ and γ. The error when these are estimated using the rigid lid approximation can still be well appreciated from Figures 9 and 10.

Table 4: the relative error remains positive for all cases when the MSC model is used. However, in the LES case, the error becomes negative for the subcritical case. Any idea about why this occurs?

The sensitivity study has been extended, and there are indeed conditions where the rigid lid approximation performs slightly worse than the full AGW solution. In fact, it appears that η_{tot} approaches the rigid lid solution from above instead of below. The cross-over point occurs, according to the MSC model, around $\Delta\theta = 10$ K. This is higher than the value used in the subcritical LES conditions, where η_{tot} for the subcritical case is already higher than the rigid lid at 7.312 K. Unfortunately, while the crossover of the full AGW solution over the rigid lid seems to be predicted by both the MSC model and the LES, we do not have a clear explanation regarding the difference in the value of $\Delta\theta$ at which such crossover is observed.

Line 372-374: the fringe region is adopted in pseudo-spectral (or fully spectral) flow solvers to impose the inflow conditions. The presence of gravity waves does not imply the use of a fringe region, as inflow-outflow boundary conditions can be adopted (although the implementation is not trivial).

This aspect has been addressed in Section 2.1. And the sentence mentioned by the reviewer has been corrected by specifically referring to finite volume codes (see line 713).

Line 385: As a future work, I would also suggest further validation of this technique, as it has been only applied to two idealized cases. For instance, Lanzilao and Meyers (2024) performed 40 LESs in different atmospheric conditions, for which the displacement of the capping inversion is computed. This comparison could offer further insights into the performance of the proposed method.

The reviewer's suggestion has been added to the revised manuscript (see lines 727-729 of the track changes document).

Appendix A: this construction looks quite artificial to me. From my point of view, it would be easier to drive the main domain using the same precursor simulation for both the AGW-modelled and AGW-resolved simulations. Is there a particular reason why the authors decided to not pursue this option? It would eliminate the need for the process described in this appendix together with ensuring equal inflow conditions in both cases.

We totally agree with the reviewer. Unfortunately, as previously mentioned, the inflow data for the AGW-resolved simulations (which have been conducted some time ago and already presented in Stipa et al. (2023)) have been generated at runtime and have not been saved to slices in the disk. In this regard, the approach followed in the paper was the only one that allowed us to avoid re-rerunning the AGW-resolved simulations, which we consider an unnecessary use of computational resources in light of the results presented in the paper.

Line 113: replace "If one wishes to resolve AGW within LES" with "When simulating AGWs in an LES framework" or similar.

Corrected (see line 168 of the track changes document).

line 205: used -> use.

Rephrased (see line 370 of the track changes document).

Line 368: conventionally neutral boundary layers -> CNBLs.

Corrected (see line 709 of the track changes document).

References

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