University of British Columbia

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

Response to Reviewer 2

Exec. S. Stipa - December 21, 2023

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

Our response, denoted in black, is shown below. Modified text of the paper is shown between quotes in *italic*, while the reviewers' comments are denoted in blue. Please refer to the track changes document for a detailed overview of the additions/deletions made in the revised manuscript.

When looking at the new Figure 9 and revisions associated with it, I am a bit uncomfortable about the justification of running the wake model with mirroring of the turbines when using it within the MSC, yet needing to run it without mirroring when used outside of the MSC framework. I feel that this raises some fundamental questions and demands some justification based on the physics, and what the mirroring is supposed to represent, rather than, just based on the agreement each sub-option gets wrt the LES. Did you question this? Do you have some potential explanation?

We did not include mirroring in the wake model, wake model + VC, and original 3LM model because we wanted to maintain the same wake model configuration as presented in Niayifar and Porté-Agel (2016). Conversely, in the MSC model we added turbine mirroring because we aimed to include the highest amount of physics possible, i.e. we added both the background pressure gradient effect and a more realistic representation of velocity close to the ground through turbine mirroring. The result was that the MSC achieved very good agreement with respect to LES, not only on relative-to-first row, but also on an absolute basis. Regarding the prior models, including turbine mirroring yielded poorer predictions, as they were not defined with this effects included. Nevertheless, it is possible that they might perform better after having re-tuned them to account for turbine mirroring effects. These considerations highlight a confounding artefact of engineering models: when a tuned model is supplemented with additional physics, it may achieve poorer predictive accuracy and it would need to be re-tuned. Specifically, once a model configuration has been selected (i.e. number of rotor quadrature points, superposition method, wake model, mirroring or no mirroring, turbulence intensity model) and tuning is performed, it is very possible that two different configurations may achieve similar results with distinct sets of tuning coefficients.

In our study, we tried to circumvent this artefact by comparing the MSC model to the prior engineering models as they were initially proposed. As we stated in our paper, we do not perform any tuning of the prior engineering models apart from the turbulence intensity model, whose tuning is not aimed at converging turbine power, but rather at providing the correct TI for the TI-wake expansion relation in the wake model (the same coefficients are used for all models). Hence, we can say that in our study the wake model is operated as is, using the same wake superposition technique and TI model used by Niayifar and Porté-Agel (2016) (who also used the Bastankhah and Porté-Agel (2014) model) for the Horns-Rev wind farm. Moreover, Niayifar and Porté-Agel (2016) show the huge difference produced by different superposition strategies or distinct TI models. By looking at Fig. 6 in their paper, it can be seen how energy superposition overestimates power compared to linear. For instance, adding turbine mirroring may change this situation and actually make results better for the energy superposition. Nevertheless, since the set-up used by Niayifar and Porté-Agel (2016) has been proved to work, we kept it the same for our wake model runs. We apply similar reasoning towards the original 3LM and wake model + VC; the first only corrects a first-row bias, while the influence of the second is extremely local, as shown in Fig. 15 of our paper. Basically these model configurations behave like a wake model alone, with some correction, as no meso-scale effects are present. Conversely, as the MSC model brings more physics into the picture, we used mirroring, which is more physical as slower wake growth occurs at the ground. In this case, the longer persistence of the wake is balanced by a favorable pressure gradient inside the wind farm, which is closer to what happens in reality, and we feel makes the MSC model innately more general and less in need of model tuning to cover some missing physics. In fact, no additional tuning was required when the LES featured the same physics that was modeled within the MSC through engineering parametrizations.

Also looking at Figure 15 again, I realize that the VC model used to model blockage hardly does anything to the result. And I think this is to be thought about in conjunction with the earlier question.

Regarding this aspect, we would like to give some clarification. While it is true that power and thrust are not much affected by the presence or absence of the VC model (Fig. 15 in our paper), the VC model substantially improves the accuracy of the velocity field (Fig. 13 in our paper). In fact, comparing the first two panels, it can be seen how the velocity depicts a discontinuity at every turbine location without the VC model, while the use of the VC model adds what we refer to as the local blockage, i.e. the induction effect due to the pressure increase in front of each rotor. As explained, since the induction produced by an individual turbine yields a negligible wind speed reduction already five to six diameters upstream of its rotor, local blockage hardly has an effect on power and/or thrust distributions within a wind farm, but its inclusion in the model is important when making velocity estimates in the neighborhood of wind turbines.

I believe that the VC model for blockage, as implemented, does not satisfy mass conservation at the wind farm level. Is that right? I expect that if it did (like for example the RHB with wake expansion does), the model would produce a larger magnitude of blockage and create some background acceleration through the wind farm. The results from e.g. wake + RHB-W would be more different from wake results than your wake + VC currently are. If this were the case, you probably would not mind using it with a wake model also implementing the turbine mirroring, as without it, the combined wake + blockage would overpredict the power at the back of the wind farm. You would perhaps think that it's an issue if the wake model on its own no longer captures the power down the line of turbines when operating standalone. But if the wake model is meant to be used in conjunction with a blockage model, it should be validated against pattern of production when used in conjunction with the blockage model anyway, so this is not necessarily an issue*. Any thoughts?

The reviewer is correct that the VC model does not conserve mass. We coupled the wake model and the VC model as explained in Branlard and Meyer Forsting (2020), which does not conserve mass at the wind farm level. This is because wake models are defined on velocity, while the VC model is defined on vorticity. We are not aware of the details of the RHB with wake expansion. To our knowledge, the RHB model has been only published in Gribben and Hawkes (2019), but Nygaard et al. (2020) state, "*Away from the rotor the flow patterns of this potential flow model* [the RHB] *are nearly identical to those of the vortex cylinder*." In general, we do not believe that the cumulative induction from single rotors equates to the large-scale pressure perturbation around the wind farm, as this effect is very local. We agree with the reviewer's considerations about using the RHB model with turbine mirroring, but we emphasize that the local blockage model should only capture local blockage in order to lie within the scales separation principle mentioned in our study. If this is not the case, there might be some bias due to double counting between the local induction model and the mesoscale global blockage model. If the reviewer is referring to the upgraded RHB model mentioned in Nygaard et al. (2022) and developed by the same authors, it is properly a global blockage model, so care should be paid in this case to avoid double counting global blockage effects.

In general, it is extremely difficult to validate each sub-model with observations, as these would contain all the ongoing physics. One alternative is to use computer simulations that incrementally add more physics to the problem. For example, to validate the wake model + VC, one could run LES simulations of a truly neutral (i.e. fully non-stratified) boundary layer, with the domain top placed very far from the wind farm in order to avoid any flow confinement (which would produce large-scale pressure gradients). In this case, as there are no gravity wave induced pressure perturbation, the mesoscale model can be disregarded and the accuracy of the turbine-scale parametrization can be assessed. A simulation of this type has been performed in Lanzilao and Meyers (2023) and it shows that, without thermal stratification, pressure perturbations only exist very close to the wind turbines (Fig. 15 in Lanzilao and Meyers, 2023). Moreover, looking at Fig. 21 in their paper, it can be seen how the pattern of production for the NBL (neutral boundary layer) case is very similar to the wake model with turbine mirroring (Fig. 9 in our study). In our paper, the wake model without mirroring matches better with our simulations, but these include stratification. So basically it is as if not including mirroring reduces the error on the pattern of production produced by stratification effects, where the favorable pressure gradient within the farm produces a faster wake recovery. Finally, blockage in truly neutral conditions is only produced by the superposition of individual turbine inductions (i.e. local blockage effects), while boundary layer displacement does not produce any pressure disturbance in this case due to the absence of stability.

To conclude, as proved by Fig. 9 in our paper, the fact that the wake model in the same configuration as the one used in the MSC model (wake model alone with mirroring in our case) does not capture the pattern of production is not an issue as long as this pattern is captured by the overall MSC model. Moreover, our LES simulations contains stratification, while the wake model alone does not. To be strict, the wake model alone is trying to model a wind farm immersed in a fully neutral boundary layer. The fact that it captured the pattern of production is simply a consequence of having used the wake model in a configuration that was shown by Niayifar and Porté-Agel (2016) to be the best for standalone use.

Am I right in thinking that the pressure perturbation derived from the 3LM model is not just the feedback from gravity waves, but indeed also accounts for mass conservation at the wind farm level? After all the ABL displacement which comes in equation 5 is very much the result of mass conservation across the layers. If so, does this raise the question as to whether the MSC framework requires a model for wind farm blockage at all, superposing individual turbine inductions? Would you not be double accounting if you were using the 3LM with a blockage model that does a better job at enforcing mass conservation? In fact, how do the MSC results compare with the LES if you don't used the VC model? Based on the little effect the VC model has when used with the wake model, I suspect that MSC results without it would not change much.

The reviewer is correct that results would not change much by removing the VC model, but this only holds for the wind farm power production and thrust. The velocity would be incorrect by not using the VC model, as the background velocity calculated by the mesoscale model does not include local blockage effects. This can be proved mathematically. First, the 3LM evaluates the inversion layer displacement such that mass conservation is satisfied within each layer. Moreover, the pressure disturbance is the one that puts in agreement the momentum balance below H and the pressure response given by the resulting boundary layer displacement. If a fully neutral flow is considered, the only pressure disturbance in the boundary layer is due to the local induction, but in this case the 3LM model predicts a null pressure disturbance, as $\Phi \to 0$ in Eq. 5 (this is also shown in Smith, 2023, Tab. 2), hence the 3LM disregards local blockage. In this case, mass imbalance produced by velocity decrease is fully compensated by streamline divergence.

In conclusion, while local blockage does not affect power and thrust distribution, its parametrization using for example the VC or the RHB models should be retained for an accurate velocity field in the region immediately upstream of the wind farm. Moreover, if the local induction model only models local blockage effects, without attempting to cover a broader range of physical aspects, the separation of scales principle holds and global induction is not double counted. Conversely, local induction cannot be double counted as the 3LM neglects it by construction.

Can you please spare some thoughts about the above and amend the paper with what you conclude on this? The main question to address really is whether the MSC should actually be run with a blockage model at all.

The paper explicitly clarifies this aspect at line 151: "*It is important to highlight the significance of the pressure variable, as this concept will be essential in the formulation of the MSC framework later on. First of all, Allaerts and Meyers (2019) make the assumption of zero vertical pressure gradient inside the ABL, hence p only varies in the horizontal directions. Secondly [...] it is clear that p can only change in response* *to a vertical displacement of the ABL and only if the latter experiences a capping inversion jump $\Delta\theta$ and/or a stable lapse rate γ. For this reason, the pressure variable considered by Allaerts and Meyers (2019) only contains the effect of internal and interfacial waves, and does not account for the local pressure rise in front of each wind turbine, responsible for what we refer to as local blockage or turbine-level induction, which must be modeled separately.*"

We also added an additional reminder of this aspect in the revised paper at the beginning of Sec. 2.2.4, namely "*As explained at the end of Section 2.1.1, the pressure-induced background velocity produced by the atmospheric perturbation model at the mesoscale does not contain local blockage effects.*"

Finally, we significantly expanded our discussion regarding turbine mirroring at the beginning of Sec. 4. Please refer to the track changes document.

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.

Bastankhah, M. and Porté-Agel, F.: A new analytical model for wind-turbine wakes, Renewable Energy, 70, 116–123, https://doi.org/https://doi.org/10.1016/j.renene.2014.01.002, special issue on aerodynamics of offshore wind energy systems and wakes, 2014.

Branlard, E. and Meyer Forsting, A. R.: Assessing the blockage effect of wind turbines and wind farms using an analytical vortex model, Wind Energy, 23, 2068–2086, https://doi.org/https://doi.org/10.1002/we.2546, 2020.

Gribben, B. J. and Hawkes, G. S.: A potential flow model for wind turbine induction and wind farm blockage, Systems and Engineering Technology, 2019.

Lanzilao, L. and Meyers, J.: A parametric large-eddy simulation study of wind-farm blockage and gravity waves in conventionally neutral boundary layers, 2023.

Niayifar, A. and Porté-Agel, F.: Analytical Modeling of Wind Farms: A New Approach for Power Prediction, Energies, 9, 741, https://doi.org/10.3390/en9090741, 2016.

Nygaard, N. G., Steen, S., Poulsen, L., and Pedersen, J. G.: Modelling cluster wakes and wind farm blockage, Journal of Physics: Conference Series, 1618, 062 072, https://doi.org/10.1088/1742-6596/1618/6/062072, 2020.

Nygaard, N. G., Poulsen, L., Svensson, E., and Grønnegaard Pedersen, J.: Large-scale benchmarking of wake models for offshore wind farms, vol. 2265, p. 022008, https://doi.org/10.1088/1742-6596/2265/2/022008, 2022.

Smith, R.: The wind farm pressure field, Wind Energy Science Discussions, 2023, 1–14, https://doi.org/10.5194/wes-2023-56, 2023.