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 - November 14, 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 section at the end of this document for a detailed overview of the changes made to the manuscript.

Excellent work describing the MSC wind farm engineering model framework to simulate array interaction effects through a systematic approach that separates mesoscale and microscale scales. The authors made a great job breaking down the model in its different components and analyzing the differences with respect to simpler (industry standard) wake models and verifying against benchmark results from a high-fidelity LES model. The methodology is sound and the results speak for themselves as to the significant improvements from the earlier 3LM model.

I only have a few editorial suggestions and a couple of discussion points around the tuning and the practical application of the model.

In particular, is the re-tuning of the TI model not a contradiction with the modularity principle discussed in section 2.2 where one can "build upon already-existing sub-models, so that additional tuning parameters or individual sub-model re-tuning are not required". I wonder if the tuning coefficients for a stand-alone wake model are equivalent to those used in the MSC framework. Is it appropriate to tune the TI model against LES simulations that account for global blockage effects while the wake model doesn't? Is the separation of scales not happening in TI for now but something worth exploring in the future?

We were surprised to notice how big of an impact the TI model – which basically sets the wake expansion coefficient in the wake model – has on the individual power of each wind turbine in the farm. We believe – as mentioned in the paper – that such strong impact is mainly related to our choice of simulating an aligned layout, combined with the fact that we are at the TI model validity bounds. Moreover, besides the large variation in turbine power obtained when adjusting turbulence intensity, we also noticed that the TI model itself was underestimating turbulent intensity at the turbine locations when compared against LES data.

Our reasoning is that, without TI model tuning, wake model results are affected by an intrinsic error that is not actually due to the wake model itself, but rather to an erroneous wake expansion produced by an incorrect estimation of the TI at the turbine locations. This holds in general and not only when the wake model is used within the MSC framework. Conversely, re-tuning the TI model allows the wake model to operate with a more accurate value of hub-height TI throughout the farm. Fig. 1 shows the row-averaged TI predicted by the original and re-tuned TI model, as well as the one calculated from LES simulations, where N1 and N2 refer to the subcritical and supercritical conditions, respectively.

Regarding the dependency of TI on free atmosphere stability, we argue that lapse rate and inversion strength do not seem to appreciably affect fluctuations below the CNBL height. Conversely, as background velocity is affected by what happens aloft, we expect the TI to change throughout the wind farm as it is related to the ratio of fluctuations over mean. Despite these considerations, the Niayifar and Porté-Agel (2016) TI model does not include stability dependency and it only depends on pre-existing hub-height TI, turbine characteristics and wind farm layout. Nevertheless, when the model predicts TI values that are closer to the LES, it allows the wake model to accurately capture the mean power distributions. This means that row-to-row variations in turbulence intensity due to free atmosphere stability effects may not be a critical aspect to capture. Notably, we do not consider internal ABL stability in our paper, which is expected to produce a substantial difference on the TI experienced at the waked turbines. However, we expect ABL stability to produce more of a global shift in TI throughout the waked turbines, rather than a relative variation as is the case for free atmosphere stability.

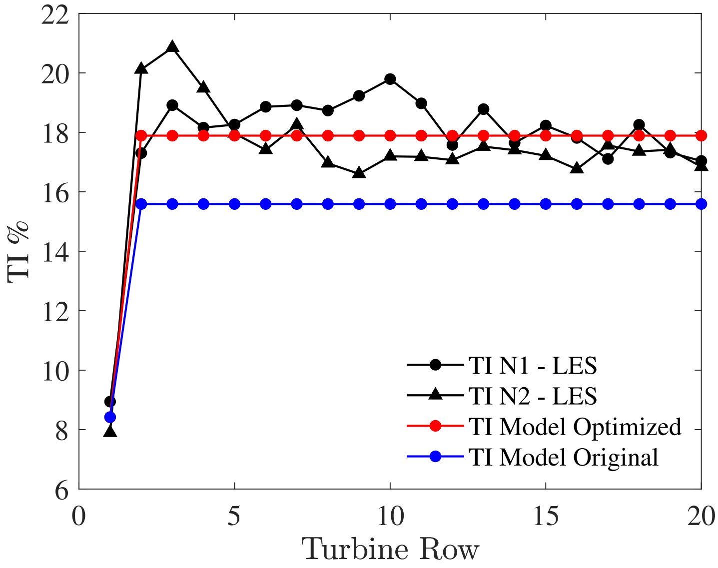


Figure 1

To conclude, the tuning of the TI model has been performed in order to remove TI-related error from the wake model results. This is something that would have affected all engineering model runs presented in the paper. The message that we want to convey with the sentence *"build upon already-existing sub-models, so that additional tuning parameters or individual sub-model re-tuning are not required"* is that (1) the MSC model operates each individual model in a way that is consistent with that model's design, and (2) the coupling strategy can not be tweaked, i.e. there are no tuning parameters related to model coupling. This means that if a different sub-model is used instead of the ones proposed (we mainly refer to local-induction, wake and TI models), it does not require re-tuning just because it is now operating in a multiscale framework. Unfortunately, this does not mean that the overall MSC framework is a parameter-free model in its entirety, as each of the sub-model parameters still remain. In this regard, the TI model tuning does not conflict with what is stated above, i.e. the model has been re-tuned just because it was performing poorly for this application, but the adopted value of d_s is the same throughout the paper regardless of the wake model being operated alone or coupled with a mesoscale model.

The authors use LES as "ground truth" to tune the TI model and quantify the correctness of the model in very specific neutral conditions. However, from an application point of view, the coefficients of an engineering model are also means to correct the mean bias when the model is integrated over the annual wind climate in a wide range of layout and siting conditions. I wonder if the authors could provide some insights about the foreseen calibration strategy using (observational) validation data, where separation of scales may not be possible. I guess LES offers a high-fidelity benchmark model to tune engineering models but you still need to train the models with measured data to mitigate outstanding biases. Would this training focus on some of the engineering model coefficients or additional ones to preserve the separation-of-scales principle?

The reviewer points to a very important question related to our paper. The proposed separation of scales principle is used when setting up the foundation of our coupling strategy, i.e. distinguishing between microscale and mesoscale effects. In this regard, individual sub-models should be in principle designed and tuned by only looking at their scale, i.e. without allowing different scales to introduce a bias. Just to give an example, individual wake models should be tuned using isolated turbines.

On the other hand, as the author states, "the coefficients of an engineering model are also means to correct the mean bias when the model is integrated over the annual wind climate in a wide range of layout and siting conditions", which is true and allows to indirectly include physical processes and effects that models do not directly capture in their formulation. However, depending on the bias that one wishes to correct, such practice may lead to erroneous power results if e.g. tuning is performed on velocity or vice versa. This is expected to hold for any engineering model, being it a simple wake model or the MSC framework, with decreasing error on those variables not used for tuning with an increase in the amount of modeled physics. Regarding the MSC framework, we highlight that the proposed mesoscale sub-model does not feature tuning parameters, and it is just a result of the input background state. For this reason, provided that such state is evaluated with the highest possible degree of accuracy, model tuning can be performed at the microscale, i.e. the scale that most directly influences wind turbines. This practice obviously violates the separation of scale principle, as the quantity used for tuning inevitably contains effects from other scales. In particular, while more accurate predictions on the tuning variable might be obtained, correlation could be lost on some of those variables that are not object of tuning.

On an operational standpoint, tuning should be performed first on those models that are characterized by a single output (when data is available). For example, the TI model can only be tuned on TI, hence it should be tuned first, as the overall model can only benefit from this. Possible decorrelation on secondary outputs arising from this operation should be seen as a structural deficiency of dependent models (e.g. the wake models), and can be corrected with further tuning. For example, correcting TI predictions in our case revealed that the wake model was actually performing fairly well and it did not require any tuning. Regarding local induction, because self-induction is excluded when performing power and thrust interpolation from turbine curves, we demonstrated in our paper that it has a small effect on wind farm power and its internal trends. For this reason, we do not recommend tuning the induction model. Finally, the wake model can be tuned by changing the coefficients of Eqs. 10 and 12 to better match power or velocity but, in either case, it is expected to become less accurate at capturing the wake of an isolated wind turbine.

In conclusion, the separation of scales can be enforced from the input in order to generate the model outputs, but there is no current means of splitting observations (characterized by outputs + unmodeled physics) in their individual scale contributions. For this reason, tuning against observations will likely make individual sub-models less accurate at the scale in which they are defined, but capable of masking their lacked physical modeling when coupled. We have the opinion that tuning should begin with the TI model – if TI data is available – then act on the wake model. However, the beneficial effect gained with tuning is expected to be site and layout specific.

Compared to traditional wake models, the MSC model requires significantly more input quantities, some of which are not accessible in wind resource measurement campaigns. While this may fall outside the scope of the paper, I would recommend the authors to discuss how the model could be used in connection to wind farm design. While the model shows significantly higher physical insight compared to traditional wake models it remains to be seen if the additional complexity does not bring additional uncertainties. Further investigation should focus on evaluating the added value of each module in the framework by quantifying accuracy in relevant quantities like AEP and array efficiency over a wide range of layout/siting conditions. These validation datasets would be useful to investigate training methodologies that provide a good trade-off between physical/numerical complexity and accuracy.

We fully agree with the reviewer. The model requires substantial more inputs due to the broader range of physical processes being modeled. The most important piece of information currently missing in site assessment studies is the vertical profile of potential temperature. This provides plenty of important information such as ABL height, ABL and free atmosphere stability. Reanalysis data, such as ERA5, have already been used to address this shortcoming (Allaerts et al., 2018), but it remains to be seen how accurate these annualized energy production (AEP) predictions are when compared with real observations.

We are currently making efforts in this direction. The MSC model has been implemented into OpenWind®

and is currently being tested against observations gathered from the GloBE project (Adams et al., 2023). This will be a step in the direction of better understanding each sub-model's contribution under different conditions.

Regarding wind farm design, thermal stratification and ABL height statistics for the site of interest are essential to produce a representative picture of what the beneficial/detrimental effects of blockage and gravity waves will be. If one does not possess such information, blockage and gains/losses associated with gravity wave effects cannot be predicted. If the amount of required additional statistics is reduced to solely the ABL height (i.e. no info on stratification), global blockage can still be predicted to a certain degree by using the rigid-lid approximation (Smith, 2023). This means that only the flow confinement under the inversion layer is considered, while its deformation is assumed zero for all conditions. While this approach is better than modeling local blockage only, as global blockage associated with flow confinement is included, it completely neglects gains/losses associated to gravity wave effects.

53: duplicated "the".

corrected.

109: For brevity, I would avoid the outline of the section "The present section is organized…" (likewise in subsequent sections).

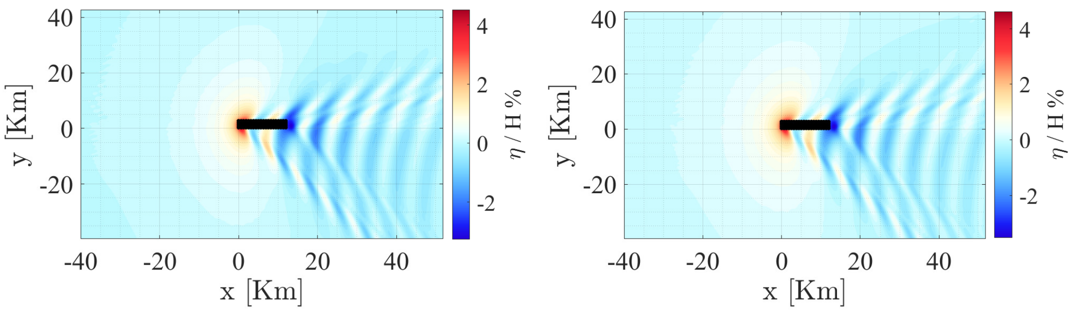
Thanks for this suggestion. Nonetheless, given the consistent length of the paper, we would like to maintain those outlines to help provide a structured layout of the paper.

185: what about Ct? which wind speed is it based upon?

We rephrased this paragraph, specifying how C_T is calculated for an isolated wind turbine. For waked conditions, we cross-reference Sec. 2.2.4, where we explain how the "freestream" velocity is evaluated in this case.

207: the re-tuning of d_s introduces quite a significant change in the turbulence intensity parameterization. This is indicative of the potentially large dependencies on the layout characteristics. Wouldn't this imply that we need to recalibrate the model with additional LES simulations in new layouts? Otherwise the potential benefits from adding a better description of the mesoscale conditions would be compromised by the underlaying uncertainty of the microscale wake model. The additional cost of this recalibration would also penalize the use of the model as an engineering model.

The turbulence intensity at turbine locations, as explained in the first answer, has a huge effect on the wake model results. However, as shown in the paper, its lack of dependency on free atmosphere stratification does not impair the accuracy of power predictions. For this reason, while the re-calibration of the TI model certainly improves turbine results from an absolute standpoint, this is something mainly related to the microscale and has little effect on the mesoscale. Regarding the latter aspect, i.e. the effect of TI on the mesoscale solution and thereby its indirect influence on the wake model results, the following has to be considered. While the actual magnitude of the Gaussian-filtered wind farm thrust distribution is calculated with the wake model, hence it depends on TI, its shape is mainly influenced by wind turbine layout and potential temperature structure, which do not depend on TI.



(a) un-tuned d_s **(b)** tuned d_s

Figure 2. Differences in inversion layer displacement between tuned and un-tuned d_s coefficient.

In this regard, it can be shown that the difference in magnitude produced using the tuned or un-tuned TI model leads to negligible changes in the mesoscale perturbation solution (illustrated in Fig. 2). As stated above, this is because the TI model does not affect the shape of the forcing, while its value only changes by a small amount w.r.t its order of magnitude. Conversely, at the microscale level, this fraction is already considered important from a power production perspective, as it is around 8% of the total wind farm power. The effect of tuning the TI model is shown in Fig. 3, which highlights how using an un-tuned TI model yields results that are comparably compromised in both wakes-only + induction and coupled modes of operation. Note that, as the error due to blockage is not relevant in this context, results are normalized with the first row-power. Moreover, regarding the coupled model, we only performed one iteration, meaning that turbine thrust distribution is only updated once based on mesoscale model results.

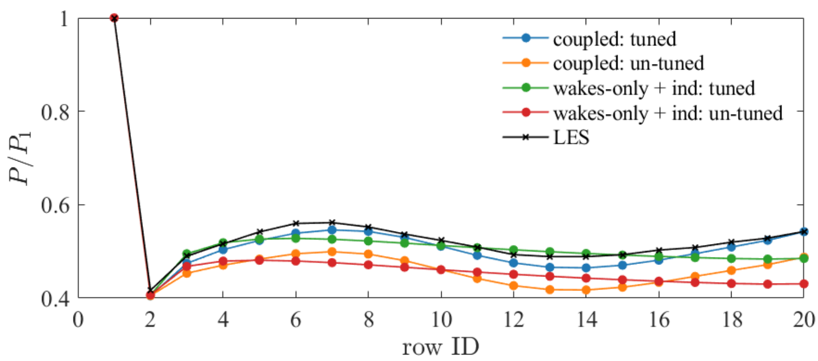


Figure 3. Row-averaged turbine power for the tuned and un-tuned TI model cases. Computations are shown for the coupled and wakes-only + induction modes of operation, where the former is run in direct mode, i.e. with only one coupling iteration.

In summary, it can be stated that the higher degree of physical description provided by the MSC model is weakly dependent on TI model tuning. Conversely, independent of the adopted modeling technique – coupled or wakes-only approaches – TI model tuning is strongly recommended as it allows to substantially improve predictions of relative trends in wind farm thrust and power throughout the wind farm.

311: "wake models are usually tuned on velocity for an isolated wind turbine wake". Is this statement referring to equation (7) on the definition of the wake velocity deficit with respect to U_∞ (without blockage)? If so, I would provide the cross-reference. Still, I would say the models are "defined", not "tuned", in terms of U_∞ to be consistent with the same definition used in theoretical power/Ct curves for an isolated turbine.

This is exactly what we wanted to convey. We rephrased the sentence according to the reviewer's suggestions.

316: “turbine thrust distribution” add cross-referenced equation (18).

Added.

342: I wouldn’t call (23) an ABL profile. This is rather a generalization of Monin-Obukhov surface-layer model to account for stability. An ABL model would be valid across all three layers. Surface-layer context is reinforced when you assume the the friction velocity only varies horizontally. Since this is only applied to the first layer I think it is appropriate to call it a surface-layer model. However, when extending the model to simulate stable conditions it may be worth exploring the use of a single-column ABL model with mixing-length parameterization.

We rephrased the sentence according to the reviewer’s suggestions.

452: “Reference potential temperature, inversion jump and lapse rate can be prescribed based on observations”. These quantities are not measured in conventional wind resource campaigns. Are you suggesting that the model requires these additional measurements? Wouldn’t it be more practical to rely on mesoscale simulations to characterize these inputs?

Yes, in order to exploit the model capability those measurements should be gathered, which is not excluded for future campaigns. Mesoscale simulations, as suggested by the reviewer, may represent a more practical way to characterize these inputs. A third option is to use reanalysis data such as ERA5, which we started to do recently following the approach of Allaerts et al. (2018). The sentence has been expanded to clarify these concepts.

473: can you explain what the “fringe region” does?

The fringe region is basically a Rayleigh damping layer where the desired velocity that the flow tries to achieve is temporally and spatially varying. This means that such approach requires a time and spatially resolved inflow field that is equal or larger than the fringe region in order to have knowledge about the desired velocity to apply. This technique is the preferred way in which an user-defined inflow is prescribed in pseudo-spectral codes, as they can only use periodic boundary conditions in the horizontal directions. For TOSCA, which is a finite-volume code, inlet-outlet boundary conditions can be applied, but the fringe region was chosen as it has the added capability of damping the reflections of gravity waves that are generated by the wind farm. The actual fringe region method adopted in TOSCA is thoroughly explained in Stipa et al. (2023) (WES preprint). Other works that employ this technique are, among others, Allaerts and Meyers (2015, 2017); Lanzilao and Meyers (2022); Wu and Porté-Agel (2017).

580 (and elsewhere): Consider using the term “verification” instead of “validation” since you are doing code-to-code comparisons instead of comparing with measurements.

The suggestion has been implemented in the revised paper.

References

Adams, N., Rodaway, C., Gottschall, J., Hawkes, G., and Simon, E.: WESC 2023 Mini-Symposium - OWA GloBE: Building Industry Consensus on the Global Blockage Effect in Offshore Wind, https://doi.org/10.5281/zenodo.8085205, 2023.

Allaerts, D. and Meyers, J.: Large eddy simulation of a large wind-turbine array in a conventionally neutral atmospheric boundary layer, Physics of Fluids, 27, 065 108, https://doi.org/10.1063/1.4922339, 2015.

Allaerts, D. and Meyers, J.: Boundary-layer development and gravity waves in conventionally neutral wind farms, Journal of Fluid Mechanics, 814, 95–130, https://doi.org/10.1017/jfm.2017.11, 2017.

Allaerts, D., Broucke, S. V., van Lipzig, N., and Meyers, J.: Annual impact of wind-farm gravity waves on the Belgian-Dutch offshore wind-farm cluster, 2018.

Lanzilao, L. and Meyers, J.: An Improved Fringe-Region Technique for the Representation of Gravity Waves in Large Eddy Simulation with Application to Wind Farms, Boundary-Layer Meteorology, https://doi.org/10.1007/s10546-022-00772-z, 2022.

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.

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

Stipa, S., Ajay, A., Allaerts, D., and Brinkerhoff, J.: TOSCA - An Open-Source Finite-Volume LES Environment for Wind Farm Flows, Wind Energy Science, 2023.

Wu, K. L. and Porté-Agel, F.: Flow Adjustment Inside and Around Large Finite-Size Wind Farms, Energies, 10, 2164, https://doi.org/10.3390/en10122164, 2017.