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 - 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.

This is an excellent, highly relevant contribution to the field of wind farm atmosphere interaction, presenting a multiscale coupled model framework, coupling models that resolve effects from turbine scale to meso-scale. The paper is well written, and the authors provide a very clear description of the models and how they are coupled. The novelty of the contribution is the three-layer model reconstruction (3LMR), the background velocity reconstruction and the new wake superposition.

The MSC model is verified against results from LES simulations for two cases with neutral conditions in the boundary layer and stable conditions above, one supercritical and one subcritical.

The ability of the MSC to better capture the LES results than the earlier 3 layer model is evident. The MSC (and the LES it is verified against) clearly demonstrate the significance of the stability conditions above the boundary layer for the magnitude of the wind farm blockage.

While the verification against a LES simulation is a very valuable exercise, I would recommend as further work (not for the current paper, but as a follow up project to the current submission) some investigations comparing the model against field data (SCADA data as well as dual scanning lidar data measuring blockage upstream of wind farms). Once validating against field data, binning measurements for finite sectors, and averaging the model results over a range of directions, it is possible that some of the conclusions might change slightly, since derived from simulations for a single direction aligned with the wind farm layout.

We sincerely appreciate the reviewer's comments on the paper. We fully agree with the reviewer's recommendation to compare the MSC model against field measurements. Currently, we implemented the MSC model in OpenWind® (this research is partially enabled by UL Renewables) and an effort is underway to validate the model against observations gathered in the GloBE project (Adams et al., 2023). Moreover, we changed the word validation to verification throughout the paper, as we are dealing with comparison against LES rather than observations.

For the current paper, I would also recommend to clarify that the magnitude of the effects reported, which imply large reduction in wind farm output, are only applicable for the conditions simulated, rather than over the typical overall conditions that will be experienced by a wind farm.

The reviewer suggestion has been implemented in the paper.

The MSC model can potentially deliver increased fidelity compared to standard engineering models for wakes and blockage, at the fraction of the cost of more expensive high-fidelity simulations such as RANS CFD or LES. How useful the model will end up being for the wind industry will depend on the ability for the users to source the many input parameters that the model requires. Inputs such as the potential temperature profile, the eddy viscosity profiles, the background wind angle profile, etc.. are typically not measured up to the heights of interest (i.e. through the boundary layer height). Have the authors already considered if the outputs from meso-scale models or re-analysis data sets could provide the required information?

Yes, both our research version of the model and the OpenWind implementation can now use reanalysis data to set the model inputs for the potential temperature profile. Using ERA5 renalysis data has already been done by Allaerts et al. (2018) to assess blockage effects on annual energy production for the 3LM. Mesoscale models are another viable option. Text explaining this has been added in the paper.

Abstract: it would be a good idea to clarify that the 'overestimation of the wind farm power by 13% to 20% is seen for the conditions modelled (9 m/s at hub height, therefore high Ct, and also for very specific stability conditions), rather than over the whole range of site conditions.

The abstract has been rephrased according to the reviewer's suggestions.

28: 'cannot be described by a simple combination of individual wake deficits' . Should this be softened a bit? Empirical wake models are doing reasonably ok on the wind farm sizes onto which they have been calibrated. May be say that models combining individual wake deficits have their limitations, and explain what these are (e.g. need for a wake superposition model, response to local changes in wind speed/turbulence intensity/shear/veer when these fall outside of the range within which the models have been typically validated).

The literature review section mentioned by the reviewer has been rephrased.

33: when you write 'Regarding blockage, or induction (Bleeg et al., 2018)', this kind of suggests that Bleeg et al imply that blockage is synonym with induction. I don't think this is the case. Might be better to add the reference to Bleeg et al at the end of the sentence. Because in the Bleeg et al results, the interaction between the wind farm and the thermally stratified atmosphere is very much playing a role in the magnitude of the wind farm blockage.

The reviewer is right. The meaning of induction is more related to momentum theory rather than thermal stratification, hence this word has been removed from the sentence.

52: 'reducing the free stream velocity'. Always reducing? I'd expect mesoscale effects for offshore wind farms operating downstream of a coastal transition to see acceleration of the freestream flow towards the back of the wind farm. Or possibly expect lateral gradients in the background flow if the fetch to the coast varies for different parts of the wind farm. Wouldn't you?

May be this is just because the 'meso-scale effects' accounted for by the 3LM are only those representing the feedback from the wind farm onto the background flow. If so , might it be worth clarifying?

The reviewer is right in general, but in this specific case we are referring to the correction provided by the 3LM to the freestream velocity used by the wake model. In the original coupling between the 3LM and the wake model, the 3LM solution is sampled upstream of the wind farm cluster, for this reason the wind farm power predicted by the model is always reduced w.r.t to the wakes-only formulation. This is exactly the reason why such coupling is limited, i.e. [it] "fails to adequately transfer the influence of the large-scale physical processes to the flow at the turbine scale". This includes background flow acceleration towards the end of the wind farm or – not treated in our paper – wind speed up due to coastal gradients. Although we rephrased the section mentioned by the reviewer to highlight the first aspect, we feel that the discussion about coast proximity falls outside of this specific discussion about the limitation of the original 3LM coupling.

54: 'the coupling between the turbine-scale wake effects and the meso-scale global effects is weak…': is this documented somewhere? If so, please provide a reference.

This is explained more in detail when we go over the original coupling technique in Sec. 2.1.5, but no specific reference addressed this aspect. Nevertheless, this can be easily seen based on how the coupling is defined in Allaerts and Meyers (2019). Specifically, after the 2D spatial solution is obtained at the mesoscale, only one point of this solution (10 diameters upstream the wind farm) is used to update the freestream velocity for the wake model. This is clearly limited as the new velocity is just shifted uniformly. Conversely, in our approach, a different freestream velocity is chosen at each turbine location, interpolating the mesoscale field. This allows to transfer large scale velocity gradient at the microscale, capturing the flow deceleration upstream, but also the beneficial/detrimental effects inside the wind farm.

74-77: 'In the subcritical regime … due to internal waves'. Does this belong to the discussion/conclusion section?

We rephrased the sentence without drawing any conclusions, but just explaining the differences between the two regimes from a physical standpoint.

118: Questions about the three layers structure: Wind turbines are getting taller... assume a 15MW Vestas turbine, rotor diameter of 240m with a hub height of 2/3 the RD, has a tip height of 280m. Offshore boundary layer heights can be quite low. How high do you assume the geostrophic height to be above the turbine layer in the model? Could you please comment if this is compatible with realistic offshore conditions?

The turbine top tip piercing the inversion layer is a situation in contrast with the perturbation analysis used to develop the mesoscale model. This is an extreme case that has been studied through LES in Lanzilao and Meyers (2023), and would probably require ad-hoc corrections to the mesoscale model (we did not investigate such condition). However, we analyzed the statistics of the inversion height e.g. in the North See using ERA5 reanalysis data. For instance, Lanzilao and Meyers, 2023 did the same and found a value of around 200 m, but they fitted ERA5 data with a temperature model that could only provide neutral conditions below the ABL height, which in many cases provides fits that are very far from the actual profile. In our fit, we used a temperature model that could distinguish between convective, neutral and stable conditions (the most frequent), and we found that on average the inversion height is located around 500 m (Fig. 1).

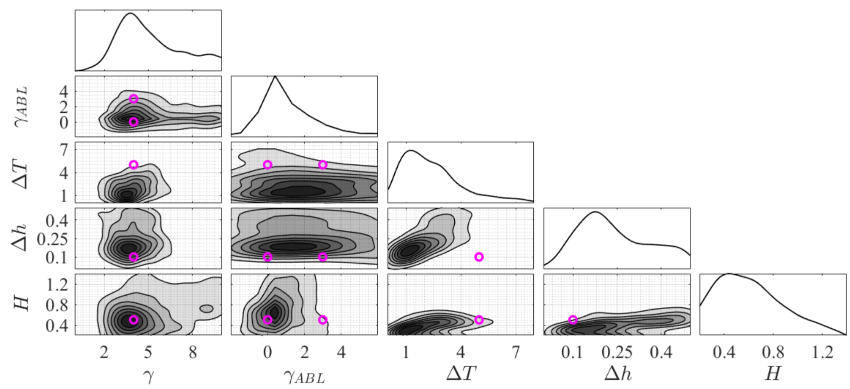

Figure 1. Statistics of parameters defining the potential temperature profile at the location of the N4 cluster in the North Sea, extracted from hourly-averaged ERA5 reanalysis data of years 2021-2022. Variables are as follows: γ [K/km] is the lapse rate, γ_{ABL} [K/km] is the lapse rate below the inversion layer, ΔT [K] is the potential temperature jump across the inversion layer, Δh [km] is the inversion width, H [km] is the inversion height. The profile used to fit the data is continuous, piecewise linear with three sections, i.e. the region below the inversion layer (unstable, neutral or stable), the inversion layer and the free atmosphere (always stable, at most neutral)

.

In these conditions, as demonstrated in our paper, results are pretty accurate. When the ABL height decreases, the main problem is that the second layer starts to disappear, as the height of the first layer is fixed to twice the hub height. In this case – not included in the present study – we analyzed a modification to the model where we relax the definition of H_1 below a certain value of H so that it is defined as half of the ABL height (this leads to $H_2 = H_1$), and we distribute the thrust force also to the second layer based on the rotor disk area fraction that is above H_1). Although still under investigation, this does not seem to show any numerical discontinuities when transitioning to the layer height definitions presented in the paper. In this situation, the ABL is shallow enough to remove the need of a distinction between the upper layer and the wind farm layer.

How are H1 and H2 set/defined?

In the paper, this quantities are defined in Sec. 2.3 "Background State".

141: where is the lapse rate defined? Lapse rate above the geostrophic level?

Yes, this is the free atmosphere lapse rate, it has now been specified. The definition of all input parameters is explained in 2.3 and their numeric values are summarized in Sec. 4 (Table 6).

142: 'dtetha is the potential temperature jump across the inversion layer': how thick is the inversion layer? Is the thickness affecting the model results?

The inversion layer is modeled as a discontinuity in potential temperature, as we use shallow water wave theory to model interface waves. This approximation is consistent with previous studies such as Allaerts and Meyers (2019); Smith (2010). In reality and in our LES simulations, interface waves are in reality trapped waves within the inversion layer. This approximation is sufficiently accurate for reasonably thin inversion layer, such that it does not become part of the ABL. As mentioned, in the LES simulations we used an inversion thickness of 100 m and the vertical profile of potential temperature given by Rampanelli and Zardi (2004), and the gravity waves patterns agree well.

151: 'zero vertical pressure gradient inside the ABL': is this limiting the applicability of the model to neutral conditions in the boundary layer?
In principle, yes. However, we are planning to extend the 3LM to formally account for internal ABL stability other than neutral by modifying the parametrization of the tensors C_{ij} and D_{ij}. Moreover, the mesoscale model can already partially account for some effects produced by internal ABL stability, as they would change the background input. In particular, the layer-averaged velocity would change ($\mathbf{U}_1$, $\mathbf{U}_2$ and $\mathbf{U}_g$), together with the shear stress at H_1, which would be reduced, in turn changing C_{ij} and D_{ij}. For the coupling strategy, stability should be accounted for in the velocity reconstruction as mentioned in the dedicated section. However, the capability that the MSC model already possesses to capture stability effects has not been yet assessed.

152: 'p can only change in response to a vertical displacement of the ABL' : Shouldn't p also respond to the presence of the wind farm, in that the horizontal p gradient is usually linked to the Coriolis term. If the wind farm removes momentum it also changes the balance between Coriolis and pressure gradient. If the pressure in the model is not responding to this change, does it mean that the model might produce some flow acceleration/deceleration which are not correct?

When deriving the 3LM perturbation equations (Allaerts and Meyers, 2019), the fact that geostrophic wind (i.e. the mean pressure gradient), vertical momentum transport and Coriolis force in the unperturbed state are in balance is exploited to eliminate these terms from the momentum equations, leaving only their perturbations. However, the mesoscale driving pressure gradient should not feel the presence of the wind farm, as it is on a much larger scale, hence it is not perturbed (this is equivalent to saying that the wind farm does not perturb the geostrophic wind). This is consistent with the LES simulations, where the background pressure gradient that is applied in the successor simulation is the same as the precursor. Instead, velocity variations produce an imbalance in the geostrophic equilibrium by varying Coriolis force and vertical momentum transport, and this is modeled in the mesoscale model through the Coriolis and C_{ij}/D_{ij} tensors.

Or does this statement only apply to the pressure boundary condition that is the third layer?

The pressure is the same throughout the ABL, while the third layer is in reality just a boundary condition that links the inversion displacement to the pressure perturbations sent back into the ABL from the free atmosphere response.

175: 'at wind turbine locations'. Should this be 'at downstream wind turbine locations'?

No, this is to distinguish between using the wake model to extract power/thrust (i.e. computing velocity only at turbine locations) or to evaluate velocity on a uniform grid, i.e. at every location on a plane. Here, some query points are located in the undetermined region as shown in Fig. 12 and 13, where we look at the entire velocity field. Conversely, wake models are usually employed to extract power/thrust (e.g. Fig. 15), which is faster as they only require evaluation at the turbine location or on a few disk locations if using quadrature points, and it does not require their evaluation in the near wake.

312: 'they do not account for turbine-ABL interaction'. I don't know that this statement is completely true... a lot of wake models, initially tuned on capturing single wakes, were re-tuned based on e.g. capturing PoP at wind farms such as Horns Rev/Nysted/Rodsand... rather than tuned against LES results... so, while they don't explicitly account for the turbine = ABL interaction, they implicitly account for some of it through their calibration.

The reviewer correctly points out that engineering model tuning allows to indirectly include physical processes and effects not directly captured in their formulation. The sentence has been rephrased to clarify this concept.

Equations 21 & 22: how is mass conservation between layer 1 and 2 satisfied in the reconstruction step?

The background velocity cannot satisfy mass conservation. In fact, this velocity is the result of the perturbation pressure field obtained when assuming the same boundary layer displacement as if the wind farm was present. The mass conservation equations contain an imbalance due to the velocity deficit directly produced by the removal of momentum from the wind farm. Specifically, the 3LM solution can be decomposed into background velocity + wind farm wake which, together, satisfy mass conservation across layers.

343: given that you include Coriolis in your model equations, how valid is your assumed inflow profile? (thinking about tall turbines that might reach into the Ekman layer).

The inflow profile is decomposed in magnitude and direction. For the magnitude, we use the classic similarity laws in the surface layer, while the angle is calculated from the LES precursor simulations. However, in absence of that, we propose other methods in Sec. 2.3 to compute the inflow angle and/or magnitude, such as ERA5 reanalysis data or the Nieuwstadt (1983) model, both featuring wind veer (in particular both input modes are currently implemented in both our research and OpenWind® versions of the MSC model). We found the Nieuwstadt (1983) model to be pretty accurate regarding the wind angle if compared with LES results. Conversely, velocity magnitude can differ sightly, and this may have a significant impact especially close to $F_r \approx 1$, where a small change in velocity could make the flow either subcritical or supercritical. In the context of the present study, where we validate the model against LES, we wanted to remove any potential error associated with the input background state (hence not strictly related to the model accuracy itself). For this reason we decided to use the LES data to define the background inputs of Tab. 6.

Table 1 p18:
Is the background shear stress magnitude at the wall related to the friction velocity and the density? (i.e. if you have two of these as inputs, is the third one an input parameter too?)
In the context of our paper, following Allaerts and Meyers (2019) we identify $\tau_{xz} = \overline{u'w'}$ and $\tau_{yz} = \overline{v'w'}$, while $|\tau| = \sqrt{\tau_{xz}^2 + \tau_{yz}^2}$, so this is not the shear stress that the reviewer has in mind, but rather its ratio with the constant density ρ_0 (this can be seen from the specified unit of measure in Tab. 1). Apart from this yes, $|\tau| = u*^2$ at the wall, but the two inputs have been left in Tab. 1 for completeness. To highlight this aspect we added a sentence that specifies the above condition.

What height is TI_∞ taken (hub height?)

Yes, the hub height. We have clarified this.

Section 3.1: the description of the LES methodology, with the use of precursor, successor simulations and the role of the fringe region could be more clearly explained.
The reviewer raises a fair point, but the paper is already very lengthy. We covered these aspect with extreme detail in our previous paper Stipa et al. (2023), that is accessible for the interested reader (the subcritical case is the same described there, while the supercritical case is identical except for a different value of the inversion strength). There, TOSCA's methodology is explained (fringe region, controller, hybrid off-line/concurrent precursor and geostrophic damping), together with a step-by-step description on how the simulation has been carried out. We believe that it would be both lengthy and repetitive to include these detailed aspects in the present paper.

473: 'we use both periodic boundary conditions and a fringe region located at the domain inlet': Do you mean a fringe region at the inflow has periodic BCs, and the resulting profile is applied at an inflow BC to the simulation domain downstream of the fringe region (as opposed to there being periodic BCs at the inflow and outflow downstream of the windfarm?

Maybe a schematic showing the respective domains would help, illustrating the quantities listed in Table 2 and 3.

The sentence literally means that the successor simulation uses periodic boundary conditions at the inlet and outlet, combined with a fringe region located at the inlet. This is nothing more than a Rayleigh damping layer located at the inlet that extends for some distance into the domain, where instead of forcing the flow to a reference velocity imposed by the user (in the case of Rayleigh damping the vertical velocity component is forced to zero at the domain top, where the low is laminar) the flow is forced using a time and spatially resolved field, available at each time step, obtained from a precursor simulation that runs simultaneously with the successor, i.e. the concurrent precursor. This simulation in turn is initialized with inflow-outflow boundary conditions using a mapped inflow boundary from an off-line precursor run on a smaller domain. Once the turbulent solution has filled the concurrent precursor domain, we switch also its boundary conditions to streamwise periodic, and the simulation is now self-sustained, i.e. it does not require any external turbulence mapping. An entire section of our previous paper (Stipa et al., 2023, where also a schematic is shown) details this procedure, and explains the reason why we employ such method, namely to prescribe an unperturbed turbulent inflow to the successor (the wind farm wake re-advected into the domain by periodic boundary conditions is canceled out within the fringe region length) at the same time damping gravity waves reflections from the inlet and outlet boundaries that would otherwise pollute the LES solution. Other studies use a similar set-up (see for example Allaerts and Meyers (2017, 2018); Lanzilao and Meyers (2022, 2023)).

493: ‘conducted on a domain smaller than the fringe region’: how large was this domain? Large enough to avoid artificially increasing the correlation between developing turbulence structure (from what I presume are periodic BCs).

This part of the simulation (i.e. the off-line precursor run) and its results are described in Sec. 3.2. In particular, "The domain size of the two CNBL simulations is of 6 km × 3 km × 1 km in the streamwise, spanwise, and vertical directions respectively. The mesh has a horizontal resolution of 15 m, while in the vertical direction it is graded equally as the concurrent precursor and successor simulations." This domain is larger or in-line with previous literature (Calaf et al., 2010; Churchfield et al., 2012).

495: ‘Inflow slices are then periodized along the spanwise direction and mapped at the concurrent precursor inlet, as it uses inflow-outflow boundary conditions in this initial phase’. Not sure what this means.
The concurrent precursor domain is larger than the off-line precursor domain. In particular, it is equivalent to the successor in the spanwise and vertical directions, which are mainly dictated by the ability to capture gravity waves. Hence, as the concurrent precursor domain is larger than what is required to achieve statistically steady turbulent statistics, we spin-up turbulence on an off-line precursor that runs alone on a 6 km × 3 km × 1 km domain for $120,000$ s. During this phase, inflow slices are saved and used to spin up the flow in the concurrent precursor domain, which requires at this point just a single flow through time since the inflow already contains fully developed turbulence. When the domain is filled by resolved turbulence (i.e. after a bit more than one flow through time) boundary conditions in the concurrent precursor are switched to periodic and there is no need for the inflow database anymore. In fact, the concurrent precursor would proceed like a conventional precursor. This procedure is detailed in Stipa et al. (2023).

Table 4: delta h not defined. I can see it in the text in the next paragraph, but I think it should be defined in the paragraph above, where the other parameters are described.

This has been added.

532: ‘wake models alone’: do you mean ‘engineering wake models alone’?
‘would predict very similar power production for each individual turbine in the two cases’: Should you show the resulting TI values too to be able to state this? I suspect that the resulting TI at HH is similar for both N1 and N2, but may be this should be stated/checked.

We included the reviewer suggestions. Indeed the hub-height TI is the same for the two cases and mention has been added.

Table 5: what is qmin?

This is mentioned in the table caption, it is the minimum heat flux within the boundary layer and it is a useful parameter if these cases will be compared against other codes or ABL conditions in the future.

Should the Froude numbers be added to this table for quick recollection of which case is which??

Good suggestion. We have added this to the caption.

594-607: It would be really interesting to develop this section a bit, by adding some plots illustrating what you describe. Since the way engineering models capture (or not) the pattern of production down a line of turbine, and how accounting for blockage might change the redistribution of power between the front and the back of the wind farm are both still hotly debated in the industry.

We expanded the section following the reviewer's suggestions, adding a plot that shows the effect of mirroring/no mirroring on row-averaged power distributions for the MSC and wake model.

The LES, by resolving the direction fluctuation, will naturally include some effect that would represent wake meandering, while the engineering models, when operating steady state, tend to ignore this. Did you account for direction uncertainty when processing the results from the engineering models? Something like the averaging discussed in Gaumond et al ,Wind Energ. 2014; 17:1169–1178?

Same question about the results from the MSC? Are the results for a single wind direction aligned with the wind farm layout, or are you also averaging results for a finite sector width, accounting for direction uncertainty?

No, all engineering models (MSC, original 3LM, wake model with and without local induction) are run on a single wind direction without accounting for direction uncertainty.

601: 'in agreement with many previous literature studies' . Please include references.

References added as per reviewer's suggestion.

609: 'As the NREL 5-MW thrust curve is not available in official literature..': this must have been an issue for the LES run too no? How did you deal with this then? Should this be mentioned earlier, when mentioning NREL turbine used in the LES? was the thrust and PC used in the LES consistent with that used in the MSC model?

The LES does not require C_T as an input. Instead, our actuator model implementation features blade information and wind turbine controllers. Each radius station is characterized by chord, twist and airfoil lift and drag coefficients, so that C_T is actually an output of our simulations. For this reason, we decided to run isolated turbine simulations extracting the thrusts and power curves with TOSCA. This aspect is highlighted in Appendix B.

610: 'with uniform, non-turbulent inflow': non-turbulent ? really ? thrust and PC curve typically depend on background conditions, such as TI, shear, ... is this the right approach? Or when saying 'non-turbulent', do you mean the turbine is operating in background turbulence only (i.e. no added turbulence from neighbouring turbines).

The turbine curves were extracted using an idealized setup, i.e. not including ground effects, turbulence and shear. We agree with the reviewer that this might not be the ideal method, but the inclusion of these effects would have been extremely computational intense, as a precursor simulation for each set-point would have been required. Moreover, according to Bardal and Saetran (2017), the effect of TI is minimal around the operating conditions chosen in our paper for the wind turbines, i.e. far from cut-in and rated wind speeds (where TI effects are more pronounced). Regarding shear, we did not include its effect in the power curve, but instead shear was considered in the velocity profile from which the background velocity at turbine quadrature points is evaluated. In fact, modeling each rotor as a group of quadrature points allows to account for non-uniform flow conditions (partial waking and shear) at turbine locations.

639: 'while depth averaged perturbation velocities are overestimated ': similar to earlier comment: It would be interesting to find out how your MSC results would change if you work out what is the typical wind direction standard deviation at one point in the precursor LES run, use this as a measured of the wind direction (WD) uncertainty and carry out additional MSC runs for directions 1-2 stdev away, then average the MSC results over a few directions. My expectations would be that the pressure signal is not changing much but the velocity might.

This is an interesting test to perform. In general, we believe that a slightly different wind direction has a greater impact at the microscale rather than on the mesoscale. Focusing on the latter, we agree with the reviewer that, while the 3LM pressure might not change much, the 3LM perturbation velocity could. However, only 3LM pressure is then used in the 3LMR (reconstruction step) to reconstruct the background perturbation velocity from the pressure field. As a consequence, we expect the overall sensitivity of the mesoscale sub-model on small changes in wind direction to be negligible compared to the one of the wake model, as this might enable partial waking, thus appreciable variation in power distributions.

644: 'While such limitations...' : Surely the pressure field is a function of the velocity deficit within the wind farm (which via mass conservation which conditions the vertical displacement at the inversion, which itself will feedback on the pressure). So, is the fact that different velocity distributions between the LES and MSC lead to similar pressure distributions a happy accident? i.e. does it relate to the integrated velocity deficit within the wind farm rather than the shape of the velocity deficit profile?

Looking at the 3LM equations, it can be seen that, once the pressure perturbation is known, the boundary layer displacement is automatically known from linear theory. Moreover, mass conservation can be rewritten for the variable $\eta = \eta_1 + \eta_2$, which depends on the perturbation velocity in both the wind farm and upper layer. Looking at Fig. 2, it can be seen that the error on perturbation velocity w.r.t the LES is opposite in the wind farm and upper layer, i.e. where deficit is overestimated in one it is underestimated in the second.

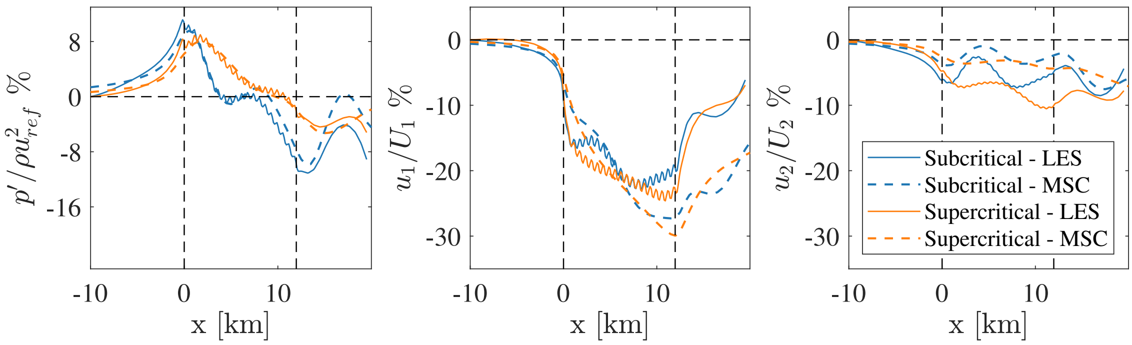

Figure 2. Perturbation pressure (left) and perturbation velocity in the wind farm (center) and upper layers (right). Both cases N1 (subcritical) and N2 (supercritical) are shown. Solid lines indicate LES results, while dashed lines indicate the 3LM results.

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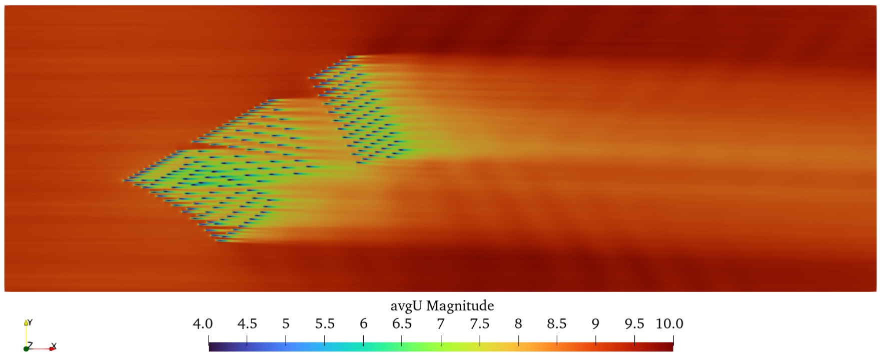
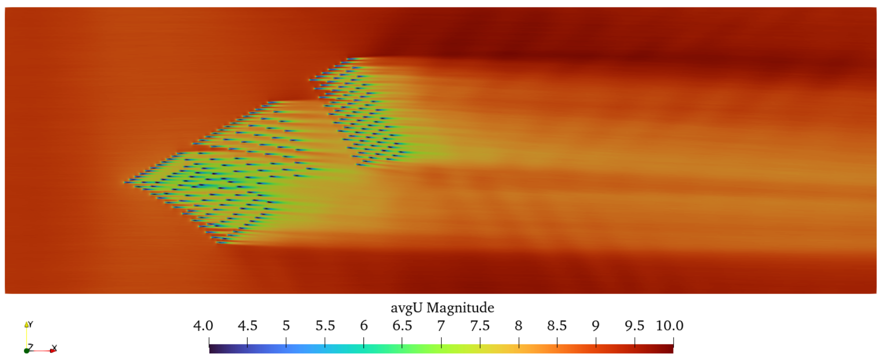
More than an happy accident, this is a structural problem of the 3LM, and arises because the layer thickness is constant throughout the wind farm. In reality, the wind farm layer grows until a fully developed state or the wind farm end are reached. Conversely, as this is constant in the model, wake effects are stronger towards the end (less height to which deficit is distributed) and smaller at the wind farm entrance (height over which deficit is distributed is larger than actual IBL height).

Figure 11: LES results are time averaged? over what time period? Surprised by the streakiness along the flow direction. What is causing this? not enough distance between the periodic inflow/outflow, leading to turbulence structures which have wrong spatial correlation properties?

Successor runs are carried out for 30,000 s, of which 5,000 s are of spin-up (slightly more than one successor flow turnover time) and 25,000 s are used to perform averaging (Sec. 3.1). The appearance of the streaks suggests that this may not be sufficient to obtain perfectly clean average fields, and the effect of turbulence may still be observed. As a reference, we report below a comparison between two hub-height averages obtained from two similar cases where one has been run for 40000 s, while the second has been run for 20000 s with spanwise shift of the inflow condition. This procedure is used for faster convergence of the average fields, as streaks do not remain locked in position at the hub height, where the flow is aligned with the x direction, but are slowly shifted along the spanwise direction with a given shift velocity (this velocity is not applied to the flow but physically to the inflow data, so that the flow is still aligned with the x direction even with shifting). As can be seen, while high-frequency oscillations can be detected in the first case, the second simulation depicts almost a perfectly converged average velocity despite running for half the time. We do not believe that such shortcoming causes any problem apart from visualizations of lower quality. In fact, it can be removed completely by increasing the time over which averaging is performed. We believe that the obtained averages have converged sufficiently for the purpose of the present study, and an increased averaging time would have represented an unjustified use of additional computational resources.



(a)



(b)

Figure 3. (a) Hub-height wind speed for the simulation that ran for 40000 s without spanwise inflow shift; (b) Hub-height wind speed for the simulation that ran for 20000 s with spanwise inflow shift.

720: ‘at least 10%’ . Please clarify that this is for the simulated conditions, at high thrust.

This has been added.

731: again, please clarify that the lapse rate is above the boundary layer.

This has been added.

732: mention of inversion strength, but not inversion thickness. Is this because your results are not sensitive to this?

We did not investigate the sensitivity on inversion thickness. However, the capping inversion is modeled as a temperature jump with zero thickness in the MSC model. Hence, this parameter is only an input for the LES simulations. With increasing inversion thickness, the temperature profile would be more and more smoothed across the inversion, leading to something that is not strictly an inversion anymore, but rather a variable stratification in the free atmosphere. Extension of the 3LM to an arbitrarily stratified free atmosphere has been covered by Devesse et al. (2022).

References

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