Response to the reviewers

We thank the reviewers for reviewing the paper again, and for their detailed and constructive comments. Below we give a point-by-point responses, with any numbers referring to equations, figures, or lines referring to the original and revised paper versions in the reviewer comments and replies, respectively (unless otherwise noted).

Because of a minor bug fix, the farm-averaged PB method now performs slightly worse than in the previous revision, with an RMSE of $5.1P_0$ instead of $4.2P_0$. This does not affect the conclusions drawn in the paper.

Reviewer 1

General comments

The authors have made a great effort in addressing the reviewers' comments and their answers have been very detailed, which is highly appreciated. The modifications to their original submission are very helpful, only some minor issues remain, though they are detrimental and need to be addressed. As long as the authors do not generalize their approach (they did raise valid reasons for not doing so) they need to make it extremely clear (also as part of the title) that their model is a concept for applicable fast atmospheric blockage modelling. Currently it is a fast model for rectangular wind farms at fixed set points (wind speed and wind direction), which to me is still not sufficiently highlighted throughout the paper, despite some additions by the authors.

Detailed in-line comments highlighting where changes should be made are attached. Once implemented no further changes should be required.

In-line comments

Reviewer Point P 1.1 — Title: Add "for rectangular", so that the title becomes "Fast models for blockage and gravity-wave effects *for rectangular* wind-farm power output".

Reply: While we agree that it should be made clear that the approach as we propose it here is limited to farms with convex layouts, regular spacing, and uniform turbine setpoints, we feel that "rectangular" is too restrictive. For example, we have no reason to believe the models would not work for circular farms. Several real-life wind farms, such as Horns-Rev or the Belgian-Dutch wind–farm cluster, could reasonably fit these criteria to some degree.
We believe that modifying the title to incorporate this nuance would result in it being either overly long, or overly restrictive, thereby reducing potential interest in the paper. Instead, we have modified the abstract to make the limitations of the 2SM approach more explicit (see point P1.2 below).

Reviewer Point P 1.2 — Abstract: The last two sentences are highlighted, but no comment was added.

Reply: Following the previous comment, we have modified these sentences to read:
The core 2SM hypothesis of scale separation and both developed models are validated using a prior LES dataset of a large rectangular wind farm with regular spacing and constant thrust coefficients. In

this idealized setup, both fast 2SM–APMs perform well, predicting blockage-corrected farm power at a computational cost that is only a factor 35 slower than a standalone wake model.

Reviewer Point P 1.3 — L33: Remove the statement "or approximately one to five minutes on a standard laptop per simulation", as the relative statement is more meaningful. As what is a "standard" laptop (or provide information regarding the machine in a footnote)

Reply: We agree with the reviewer, and have removed the statement. The sentence now reads:
This constitutes the main cost of the model, which is low compared to conventional CFD tools but still several hundred to a thousand times higher than standard wake models.

Reviewer Point P 1.4 — L85: Please add "when operating below rated wind speed or constant C_T".

Reply: Since this is already mentioned in the previous sentence, we have added the proposed emphasis there:
Kirby et al. (2025) investigated this assumption using LES results of large wind farms with uniform turbine densities in below-rated conditions, so that the $C'_{T,k}$ are constant over the farm. They found that the internal power coefficient C^*_p as defined in equation (1) is a good heuristic for internal effects, and remained constant over cases with different mesoscale flow behavior.

Reviewer Point P 1.5 — L93, "at below-rated conditions": Was it constant C_T or actually operating using a controller? If the C_T was constant one could mention its value here.

Reply: We thank the reviewer for the suggestion. The sentence now reads:
This farm has 160 10MW turbines, arranged uniformly in a rectangular staggered layout, operating at below-rated conditions, with the disk-based thrust coefficients fixed to $C'_{T,k} = 1.94$.

Reviewer Point P 1.6 — L111, "or more homogeneous regions": highlighted, but no comment was added.

Reply: Since no comment was added, we made no changes.

Reviewer Point P 1.7 — L119, "the idealized conditions tested here": Could mention that this might be due to the C_T being constant across all the different cases (I guess you can easily check if $C_{T,k}$ where similar)

Reply: We have modified the sentence to make clear that this is an explicit part of the 2SM hypothesis:
We conclude that the 2SM assumption, i.e. that C^*_p and C^*_T are solely determined by turbine-level setpoints and wake positions, holds under the idealized conditions tested here.

Reviewer Point P 1.8 — L220: Maybe add a little space here.

Reply: We agree with the suggestion, and have added the space:

$$U_f = \frac{1}{H_f \Omega_f} \int_0^{H_f} \iint_{\Omega_f} u \, \mathrm{d}\Omega_f \mathrm{d}z$$

Reviewer Point P 1.9 — L222-223: Well also your alpha might not be that easily determined either, so even conceptually it might not work. Please mention this when determining alpha that this might be difficult

to derive for irregular farms (maybe not but you are not showing it, so it's pure speculation) and revise this sentence. It is fine to have a model that only works for regular wind farms, but without showing how it could and does work for real wind farms you should not try to make it sound that it would be trivial to do so.

Reply: There are two issues to be addressed in this paragraph. First, the fact that substituting more complex averaging operators in the equations would not meaningfully change the mathematical derivation. Second, that irregular farm layouts might invalidate the underlying assumptions. The highlighted sentence was meant to only refer to the former, but we agree with the reviewer that this was not clear. Since we do not want to trivialize the latter issue, we have removed the sentence.
To further emphasize any potential complications, we have modified the final sentence of this subsection read: *Note that this basic approach does not capture the significant differences in velocity profiles between NBL, CNBL, and realistic flows, or the effects of wind farm layout and thrust distribution. Instead, it is chosen for its simplicity.*

Reviewer Point P 1.10 — L229, "It's": highlighted, but no comment was added.

Reply: We have changed this from "*It's clear that ...*" to "*Clearly, ...*".

Reviewer Point P 1.11 — L263-264, "However, this does not change the following derivation conceptually.": again not sure about this kind of statement.

Reply: We changed the sentence to instead refer to the discussion of such an extension at the end of the subsection. Correspondingly, we moved the paragraph in question to the end of the subsection.

Reviewer Point P 1.12 — L286: Maybe "total wind farm force" would be more correct here, as the individual turbine force is somewhat lost, isn't it.
Ah ok, I guess the next paragraph is mentioning it. So as long as the thrust pattern does not change, just scaling it is fine. Maybe you could mention this here, that in below rated conditions you are essentially assuming that the thrust distribution over the farm is not changing with atmospheric blockage (do you have any knowledge regarding this? Maybe the pattern does really not change too much).

Reply: We agree with the suggestion, and have modified it to read
..., while still incorporating the overall feedback between the total farm force and the blockage effects.
Following the 2SM approach, the assumption is not necessarily that the thrust distribution is unchanged, but rather that local changes in the thrust distribution do not meaningfully affect the farm-averaged velocity. This is discussed in the paragraph above equation 23, and in the second to last paragraph of the introduction.

Reviewer Point P 1.13 — L378: Especially RANS or LES single wake look up tables or surrogate models of single wakes employed in a farm simulator could potentially help here and be fast.

Reply: We agree with the reviewer, but feel that this suggestion is out of scope for this paper. As such, we make no changes.

Reviewer Point P 1.14 — L479: So you are assuming that the perturbation is a log even when the mean is not following a log or am I misunderstanding this?

Reply: This is correct, there is no physical reasoning behind this choice. To emphasize this, we have changed the word "*use*" in this sentence to "*choose*".

Reviewer Point P 1.15 — L482-483: Interesting point. Did you see of how much importance it is, ie what is the error in by Stipa's formulation neglecting it?

Reply: A direct comparison to Stipa's formulation is complicated by two factors. First, Stipa et al. did not include the η-related momentum flux terms in the APM momentum equations (9–10). Without these terms, η_c has no effect on u_c, so that the continuity equations can be left out of the correction operator. A second change is that Stipa et al. still used a Fourier spectral method for the correction operator, which might give different results due to its periodic boundary conditions. Because of these technical complications, we do not include a discussion on a direct comparison in the paper.
To simply test the effect of leaving out the continuity equations, we ran the LES comparison again with $\boldsymbol{T}_{h3} = 0$. This raised the RMSE on the farm power to $6.1P_0$ (compared to $5.1P_0$).

Reviewer Point P 1.16 — L519: Please indicate computation cost as done for your results in section 4.3. As it is a little odd to look at Table 2 whilst already some pages down you might want to consider - instead (of the existing respective LES comparison sections) or additionally - adding a final section in which you compare all your model together vs LES + computational costs. This would provide a beneficial overview for the reader as right now it seems more like a list of different models, where currently the model in section 4.2.1 is getting the most attention.

If no final discussion of the different model versions is given it is difficult to judge which one to employ and what the pros and cons are. For you as developer of the different models this might be obvious, but for readers it might not.

Reply: We thank the reviewer for the suggestion. We have added a new section before the conclusions: *This section gives a brief overview and comparison of the performance of the models developed in the previous sections. We emphasize that these conclusions are drawn based on a validation campaign using idealized LES results, and that more work is needed to investigate both the 2SM hypothesis and the models' performance further using real operational data.*

When validated with the idealized LES results from Lanzilao and Meyers (2024), both the C_T^*-based 2SM–APM and the farm-averaged PB method perform similarly well. When compared to a standalone wake model, they have the same RMSEs on power output of 3 to 4% of the farm's installed capacity, as summarized in table 2. They can better capture the spread in the dataset due to blockage effects, but have a larger bias in their power predictions. Their computational cost is 35 times higher than that of the wake model, but still orders of magnitude below conventional CFD or APM tools.

The coefficient-based 2SM–APM represents the farm using pre-defined C_p^* and C_T^* coefficients. Because of this simplicity, it has the advantage of only needing to solve one set of equations. Furthermore, if the farm power and thrust coefficients are known accurately, it has the lowest RMSE out of all the models tested. However, these coefficients, along with the tuned parameter α, are not trivial to estimate. Its applicability is therefore partially dependent on future work developing methods for calculating the C_p^* and C_T^* of real wind farms. Progress on estimating these coefficients would also benefit actuator-farm and mesoscale weather models (van der Laan et al., 2023; Du et al., 2025).

The farm-averaged PB method captures turbine-scale effects through repeated wake model evaluations. As shown in the previous section, this can be interpreted as correcting neutral-flow results for gravity-wave effects. This approach avoids the need for pre-calculating farm coefficients, making it easier

to set up for different farm layouts and operating setpoints. However, it comes at the cost of having to solve an extra set of equations.

Reviewer Point P 1.17 — L569-571, "So far, they have only been validated using LES of infinite farms (Kirby et al., 2022) and farms operating in horizontally homogeneous background conditions with uniform turbine layouts (Kirby et al., 2025).": constant thrust coefficient

Reply: We have added "and constant thrust coefficients" to this sentence.

Reviewer Point P 1.18 — L573, "turbine spacing": highlighted, but no comment was added.

Reply: Since no comment was added, we made no changes.

Reviewer 2

The authors have addressed most of my comments, either in their responses or through revisions to the manuscript. The revised manuscript can be published after the authors address the following points concerning the newly added Figure 5, the updated Figure 6, and the discussion of the poor performance of APM for cases with low capping inversion heights.

Reviewer Point P 2.1 — The authors suggest that the incorrect location of the resonant lee waves in APM may be related to the omission of dispersive and turbulent stresses. First, the impact of the incorrectly predicted lee waves and capping inversion displacement on the velocity deficit is evident in Figure 6(c) and appears significant enough that it should not be overlooked. In my understanding, including these stresses would not resolve the incorrectly predicted capping inversion shape.

Reply: While the error on the lee wave position can be significant, we believe this is not the main cause of error on P_f, as for instance the second-worst performing case H300-$\Delta\theta$2-Γ1 does not have any resonant lee waves, as shown in figure RR1. To clarify this in the paper, we have reworded the sentence on L333-334 to read
*While the model predicts the wrong location for the resonant lee waves for some of the H*300 *cases, as can be seen in figure 6 (right), this is not a consistent source of error.*.
Regardless, we agree with the reviewer that including dispersive and turbulent stress parametrizations might not improve the results. We have removed the sentence recommending this as a solution.

Reviewer Point P 2.2 — Second, the multi-scale coupled (MSC) model of Stipa et al. and its application to develop an LES model for wind-farm simulations under CNBL conditions (Stipa et al., 2024b) demonstrate that MSC correctly predicts the lee waves and capping inversion displacement under both subcritical and supercritical conditions. Since MSC and APM are conceptually consistent, this raises the possibility of an implementation issue in APM. I therefore suggest that the authors validate APM against MSC for one of the low-capping- inversion cases.

Reply: The paper by Stipa et al. (2024a) on the MSC model has validation cases with $H \approx 500m$. For the cases in our dataset with $H \approx 500m$, the APM shows reasonable agreement on resonant lee wave location as well.
Furthermore, previous work has shown that our APM implementation reproduces the resonant lee wave

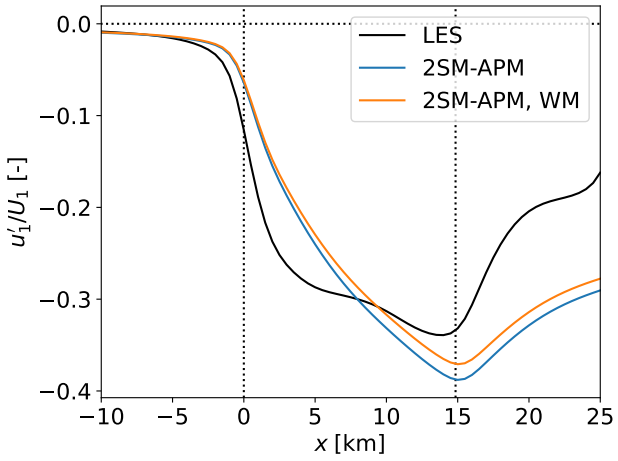


Figure RR1: Lower-layer velocity deficit through the center of the farm as predicted by LES (black) and the fast 2SM–APM (blue, orange) for the case H300-$\Delta\theta$2-Γ1.

patterns expected by linear theory (see Devesse et al., Including realistic upper atmospheres in a wind-farm gravity-wave model, Wind Energy Science, 2022). Therefore, we suspect that the errors present for some of the $H300$ cases might be due to conceptual limitations of these APM-type models, which would be shared by the MSC.
Finally, the MSC does not include the η-related turbulence terms in its momentum equations. Any potential differences between the MSC and the APM in resonant lee wave location could reasonably be attributed to this.
For these reasons, we do not perform a verification study against the MSC.

Reviewer Point P 2.3 — To further investigate the poor performance of APM for low ABL heights, the authors could plot (P_f/P_0) in Figure 5 as a function of appropriate nondimensional parameters (e.g., Froude numbers), with separate subplots for each ABL height. This may reveal consistent trends across subcritical, critical, and supercritical conditions.

Reply: We thank the reviewer for the suggestion. Figure RR2 shows the error as a function of Fr (left), P_N (middle), and both combined (right). Unfortunately, there do not appear to be any straightforward trends. For the Fr plot, one can see the choking phenomenon discussed by Smith (2010) around $Fr \approx 1$. The effect of P_N seems to be that at higher P_N, there is more scatter in the error. We speculate that this is related to the choking phenomenon as well, as at higher P_N the internal waves will provide less damping. The figure on the right shows a recreation of figure 5 from Smith (2010). When ignoring the mean bias, it seems that the larger errors correspond to the cases where linear theory predicts stronger blockage effects.

Overall, these results indicate to us that the scatter on the error mostly scales with the strength of the blockage effects. This is already mentioned in the paper (L325). Since we feel a discussion on fundamental gravity wave theory is out of scope for this paper, we do not expand on it further.

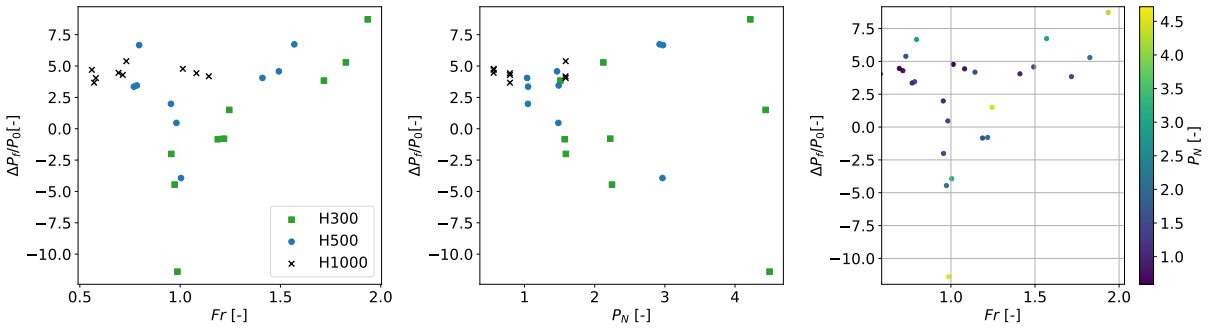


Figure RR2: The error on P_f/P_0 as a function of Fr (left), P_N (middle), and both combined (right). The different symbols in the left and middle plots indicate different boundary layer heights.

Reviewer 3

I have reviewed the revised manuscript. The authors have largely addressed my comments from the original manuscript. The remaining (minor) issues I see are below. The line numbers refer to the redlined revision.

Reviewer Point P 3.1 — Regarding "Reviewer Point P 3.2". I confess that I'm still a little confused on this one. The Ct curve is usually provided as a function of freestream wind speed. How does one then come up with the Ct' (Ct-prime) *curve* for these analyses?

Reply: In practical applications, one indeed typically uses the free-stream values for the wind speed and the thrust coefficients. The reason we use disk-based values here is because this aligns more closely with the LES results we use in the valudation. To clarify this, we have added the following sentences below equation (15):
Note that this paper uses the disk-averaged velocities and thrust coefficients, as this simplifies the comparison against LES. In practical application, their free-stream counterparts can also be used, as is commonly done by wake models.

Reviewer Point P 3.2 — Line 31: Consider dropping "inter-farm". I do not think it is needed and may momentarily confuse the reader. When I read inter-farm, my mind went right to "between farms".

Reply: Thank you for the suggestion, we have removed the words "inter-farm".

Reviewer Point P 3.3 — Line 41: "ideas" should probably be "idea" or if "ideas" is kept, the preceding "the" should be removed.

Reply: We thank the reviewer for noticing this oversight, we have corrected it.

Reviewer Point P 3.4 — Line 209: Should the second "10" be an "11"?

Reply: Thank you for noticing this oversight. We have removed the double references to the set of equations, since they refer to the same ones.

Reviewer Point P 3.5 — Equation 26: I think you may have inadvertently dropped $G_{l,k}$ from the numerator.

Reply: We thank the reviewer for noticing this mistake, and have corrected it.

Reviewer Point P 3.6 — Line 355: Typo. “bee”

Reply: Thank you, this has been corrected.

Reviewer Point P 3.7 — Line 370: Consider removing, “and do not attempt to minimize its computational cost.”

Reply: We agree with the suggestion, and have removed it.

Reviewer Point P 3.8 — Figure 8 caption. There is a full reference at the end of the caption. I think it is the same as the brief reference in the first sentence, which should be good enough, I think.

Reply: We have removed the first brief reference, and left the full one intact.