Reply to reviewers, round 2

June 10, 2026

We would like to thank the reviewers for their feedback and suggestions to improve the article. We have copied the feedback below and added answers in blue. A track changes article is added at the end.

1 Reviewer 1

Compared to the first version, this edited manuscript put additional emphasis on the description of the methodology. The authors made significant efforts to clarify all the steps involved in in the process of reconstruction the farm flow from the single-wake look up table (LUT) which is now thoroughly presented. Notably, Figure 2 is a great addition since it captures all the added information in a synthetic way. Additionally, section 2.3.2 provides the reader – who may not be familiar with pyWake – with a clear description of the farm flow reconstruction process. Finally, section 2.3.3 is probably the most valuable addition since it presents the development of the wind speed superposition model as one of the author's original contribution more explicitly (that part was a bit understated in the first version of the manuscript). The resulting section 2 is probably a bit dense (10 pages with mostly text and equations), although this is consistent with the objective of the paper which is to introduce the RANS-LUT methodology. Moreover, the extra information provided clarifies the main design choices and, thus, facilitates reproducibility of the results presented in the remainder of the paper.

The main comments that I raised in my previous review are clearly addressed in this edited version. The differences between power estimated by the RANS-AD and RANS-LUT methods are well justified by explaining the differences between the two power reconstruction algorithms. The choice of the superposition model is well motivated by the results presented in appendix B that clearly supports the author's claim about error compensation. One remaining point worth noting concerns the inclusion of the part of the speed-up region within the wake part of the LUT due to the upstream/downstream split. Since the wake and blockage parts use different superposition methods, this causes the speed-up region to accumulate differently depending on the model. It also implies that wake models will always carry over part of the speed-up region, even in "wake only" (PropagateDownwind) simulations. Solving this caveat falls outside the scope of this study, and its impact is expected to be limited given the small extent and magnitude of the speed-up included in the wake model. Nevertheless, a brief acknowledgment of this limitation in the manuscript would be valuable both for transparency and to orient future research.

In summary, this updated version of the manuscript addresses all the technical questions that were raised during the first round of review and requires only minor revisions before acceptance. For the final version, I would only recommend the authors to address minor points listed below. These remarks do not reflect on the quality of the work, which is already high, but are intended solely to improve readability.

Suggestion for clarification

1. Introduction. I think would be beneficial adding a sentence describing what is the novelty of the superposition method the authors developed compared to that of Zong and Porte-Agel. As I understand this is an original contribution of the authors which is worth pointing out. Furthermore, making the distinction between the two models clear at this stage of the paper would facilitate the understanding of section 2.3.3 (see comment on section 2.3.3 below).
   We have added a sentence in the introduction: *In addition, an alternative superposition method is applied to the velocity field, which is based on the momentum conserving superposition method of Zong*

and Porté-Agel (2020), including a limitation to the superposition weights that avoids individual wakes convecting faster than the background flow.

2. 2.1 Test cases. Could the authors detail what motivated the choice of the turbulence intensity levels for the stable, neutral and unstable cases. Especially since the TI levels for the neutral and unstable cases might seems a bit low. Is it design to be the most adverse case for the models, to meet wind conditions from a specific area or standard values often used in literature? A simple sentence would be enough.
The justification is the resulting roughness length obtained from Eq. 6. We aimed for a roughness length in order of typical offshore conditions ($z_0 = 2 \times 10^{-4}$ m):

Case	I_k	z_0
Stable	0.05	0.0003
Neutral	0.06	0.0002
Unstable	0.1	0.002

For the Neutral case, we get $z_0 = 2 \times 10^{-4}$ m, while the Unstable case requires a higher roughness. It should also be noted that this TI is based on the turbulent kinetic energy. If the TI is based on the streamwise component only ($I_u = \sigma_u/U$), one would get a higher TI (neutral surface layer measurements: $I_u \approx I_k/0.8$) ; and this is perhaps why the reviewer is used to higher TI values. We added the following to Sect. 2.1:
The chosen TI values lead to typical offshore roughness lengths (i.e in order of 10^{-3} to 10^{-4} m).

3. 2.3.1 Single turbine model. Could the authors provide additional information on what is involved in the process of "collapsing all dimensions into a 1D array"? I think this points is worth detailing a bit more to ensure reproducibility. Alternatively a reference detailing the method could be included.
This the same a converting a matrix with N dimensions to a vector. We think this is already clear enough in the text.

4. 2.3.3 Superposition methods. The section describing the wind speed superposition method may be a bit hard to follow. I think this is partly due to the fact that the authors' original contribution is not stated clearly until late in the paragraph. My understanding this that equations (16) and (17) are directly taken from Zong and Porte-Agel, while equation (19) is introduced by the authors to generalize the momentum-conserving superposition model to wind farm flows with multiple wakes. The original paper from Zong and Porte-Agel is paywalled. Therefore it makes sense to recall the basic principles since the reader may not have access to it. Nevertheless, I would recommend to put more emphasis on what is new – potentially by mentioning it earlier in the paragraph – and what motivated these developments.
As this part was added in the revision it did not entirely fit into the existing paper and we did indeed not sufficiently highlight our contribution. We have modified a few lines in the introduction:
In addition, an alternative superposition method is applied to the velocity field, which represents a generalization of the momentum conserving superposition method of Zong and Porté-Agel (2020) for wind farm flows. We derive its analytical solution and show how it can be applied in conjunction with RANS-AD deficit fields without becoming numerically unstable.
Following your comment we now are also trying to state from the start of the paragraph in Section 2.3.3 why we chose to adapt the Zong approach and give a quick overview of what will be presented:
In deep arrays with strong wake effects we generally find that linear superposition overestimates deficits. This was previously reported by Zong and Porté-Agel (2020), who successfully addressed deep farm wakes by devising a momentum deficit conserving superposition method. Hence, here we apply their methodology to the superposition of RANS-AD deficit fields by fitting self-similar Gaussian profiles to individual wakes and generalizing their momentum deficit conserving approach for wind farm flows, yielding an analytical solution.

5. I would recommend adding, after the sentence detailing the choice of linear superposition for the blockage deficits on line 260, a brief discussion on the influence of the upstream/downstream splits on the reconstruction of the speed up region with different superposition model (see my above comment).
This is a good point. We agree that the inclusion of the speed up regions in the wake model is

something one could move the blockage model. We have now added a discussion this in Sect. 3.6 Model limitations:
The wake velocity deficit model (Fig.2d) currently includes speed up regions that are superposed with the weighted superposition method to construct a wind farm flow. However, some blockage models include both the turbine induction zone and speed up regions, as for example the VortexCylinder model in PyWake based on the work of Branlard and Gaunaa (2015); and are often linear superposed. One could investigate whether moving the speed up regions to the blockage model can lead to improved results.

6. 3 Results. I would recommend defining the variables U_{LUT} and k_{LUT} at their first occurrence in the paper (i.e., intro of section 3). This point is important to clarify, notably, that the reconstructed flow field includes self-induction and can therefore be legitimately compared with the RANS-AD results. Citing equations (13) and (14) should probably be enough.
   Good point. We have added: *Furthermore, U_{LUT} and k_{LUT} are obtained from Eqs. 13 and 14, respectively.*

7. 3.3 Wind turbine power. Can the authors add a brief description of the method used for estimating the power coefficients from the local flow conditions for the reader who does not have time to read the reference. Moreover, the cited paper presents three distinct methods. I assume the one used in this study is the "AD Variable Scaling Method" (method II) but it would be good to clear any ambiguity. A simple sentence would suffice.
   We have added a bit more info:
   The alternative power coefficient and its dependency on the actuator disk averaged wind speed can be determined from 1D momentum theory (Calaf et al., 2010) or single RANS-AD simulations (Method II in van der Laan et al. (2015a)); we employ the latter for the RANS-AD wind farm simulations in the present work in combination with an analytical force distribution model, as discussed in Sect. 2.2.

Minor corrections:

1. Lines 43-45: the sentence "Fuga and the RANS-LUT models of Schulte and Stoevesandt (2014), and Jacquet et al. (2022), do not take wake-added turbulence intensity (TI) into account." reads a bit awkward. It should be rephrased to "Neither Fuga nor the RANS-LUT models of Schulte and Stoevesandt (2014) and Jacquet et al. (2022) take the wake-added turbulence intensity (TI) into account."
   Adopted.

2. Line 96-97: the use of the semicolon ";" followed by "however" makes the flow of the sentence a bit odd. It could be "The same mesh topology and boundary conditions as those used by van der Laan et al. are applied, while the domain dimensions are defined on a case-by-case basis."
   Adopted.

3. Lines 234: A comma should be added after "generally" since it is placed at the beginning of the sentence. "Generally, the background flow [. . .]"
   Adopted.

4. The term G_L in equation (20) appears to be undefined, based on equation (8), this should likely be D_L.
   We had added:
   ..., and $\mathcal{L}$ maps the discrete entries in $\mathcal{G}_L$ to a continuous domain.

5. While grammatically correct, the sentence on line 362: "Hence, it is important to include wake-added TI LUTs in the RANS-LUT" ends the section a bit abruptly. I would recommend rephrasing to "The results presented in this section demonstrate the importance of including the dependency on the wake-added TI in the RANS-LUT model."
   Adopted.

2 Reviewer 2

The topic addresses the critical trade-off between computational cost and physical fidelity in wake modeling. The proposed model shows significant improvements over existing surrogate models, especially in terms of physical consistency and prediction accuracy for both flow fields and turbine power. The manuscript is generally well-structured and clearly written. It can be accepted for publication after the authors address the few minor comments listed below.

1. Please explain why did you employ the modified turbulence model constants instead of the standard version.
   We use the turbulence model constants from our MOST k-ε model and we have now added a reference, (van der Laan et al., 2017), to the sentence where these constants are defined in the present work.

2. While the paper uses RANS-AD as the reference, adding a brief comparison against field measurements or LES data (if available) would significantly enhance confidence in the model. If not feasible, clearly state that validation is limited to numerical data.
   We agree with the reviewer that a validation with measurements and LES results would be a good next step. Note that this has already been written in Sect. 3.6 Model limitations. We could add a number of validation cases based on previous work where the RANS-AD model was validated for wind farms under neutral conditions (van der Laan et al., 2015b), but we prefer to not further increase the length of the present paper.

3. Please discuss the choice of linear interpolation.
   This is already discussed in Sect 2.3.1: *Furthermore, we use linear interpolation, due to its robustness and speed.*

4. Check for incomplete or inconsistent citations (e.g., missing DOIs, inconsistent journal abbreviations).
   We checked this, added a missing DOI to the reference of Göçmen et al. 2016 and fixed a number of typos.

References

E. Branlard and M. Gaunaa. Cylindrical vortex wake model: right cylinder. *Wind Energy*, 18(11):1973–1987, 2015. doi: https://doi.org/10.1002/we.1800. URL `https://onlinelibrary.wiley.com/doi/abs/10.1002/we.1800`.

M. Calaf, C. Meneveau, and J. Meyers. Large eddy simulation study of fully developed wind-turbine array boundary layers. *Physics of Fluids*, 22:015110, 2010. doi: 10.1063/1.3291077.

M. P. van der Laan, N. N. Sørensen, P.-E. Réthoré, J. Mann, M. C. Kelly, and N. Troldborg. The k-ε-f_p model applied to double wind turbine wakes using different actuator disk force methods. *Wind Energy*, 18(12):2223–2240, December 2015a. doi: 10.1002/we.1816.

M. P. van der Laan, N. N. Sørensen, P.-E. Réthoré, J. Mann, M. C. Kelly, N. Troldborg, K. S. Hansen, and J. P. Murcia. The k-ε-f_p model applied to wind farms. *Wind Energy*, 18(12):2065–2084, December 2015b. doi: 10.1002/we.1804.

M. P. van der Laan, N. N. Sørensen, and M. C. Kelly. A new k-epsilon model consistent with Monin-Obukhov similarity theory. *Wind Energy*, 20(3):479–489, 2017. doi: 10.1002/we.2017.

Haohua Zong and Fernando Porté-Agel. A momentum-conserving wake superposition method for wind farm power prediction. *Journal of Fluid Mechanics*, 889:A8, 2020. doi: 10.1017/jfm.2020.77.