Reply to reviewers

April 23, 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

This article presents a new wake and blockage model for wind farm flow simulation, based on the Look Up Table (LUT) constructed from single wake simulations using RANS CFD model with an actuator disk. The proposed methodology aims to fill a current gap in RANS-based models by modeling the effects of atmospheric stability and providing estimates for the wake-added turbulence. Both effects are known to have a strong influence on the build-up of wakes in wind farms and limit the accuracy of many existing models. From that perspective the work presented in this paper is relevant quite novel.

The benefits of the proposed approach are clearly demonstrated by the results of the two tests-cases presented in the article . The authors are also transparent about the remaining limitations of their approach - particularly with regard to computation time - while proposing clear tracks for potential improvement to be explored by future research.

Comparing the results of the RANS LUT using the RANS-AD shows that the model's performances are comparable to that of the full CFD model, while offering significant gains in computational time. Such a validation strategy makes sense, since the accuracy of the LUT depends on that of the underlying CFD model, as the conclusions rightly point out.

The paper is also well structured. The section on methodology provides detailed information that allows the relevance of the approach to be assessed and ensures the reproducibility of the results - at least in theory. The results are presented in a clear way and provide sufficient evidence to support the authors' claims. The paragraph discussing the performances of the various superposition models repeats some elements already covered in the methodology section, making it a bit long and harder to read than the remainder of the paper. It could probably be shortened, but this is a minor issue.

Overall, the quality of the scientific content is high and results demonstrate an appropriate technical depth. I would only point out that a lot of emphasis is put on the ability of the RANS-LUT to reconstruct the wake flow while the authors comment very little about the models performance regarding blockage and speed ups regions. Focusing on the wake makes sense, given the preeminence of the phenomena on the wind farm losses. However, the ability to accurately reproduce the entire flow field around the wind turbine is one of the main advantages of the RANS-LUT approach over engineering models. For instance, when discussing the results of the 8x8 wind farm, it would have been a great addition to address the behavior of the first row of turbines - especially for non-row aligned wind direction where some turbines are expected to benefit from local flow acceleration of the flow caused by their neighbors. Demonstrating the ability of the RANS-LUT to accurately reproduce these complex patterns - which many engineering models fail to capture - would have strengthened the overall argument.

Overall, this paper is very strong, relevant for the community and that introduce a novel approach to fill some of the current modeling gaps. Therefore, my recommendation would be to accept it for publication provided that the authors address the few minor comment listed below.

Thank you for your kind words and constructive feedback. You are right that the focus of the model comparison is the wake flow, while not much is discussed regarding the blockage and speed up effects. We have now added the following in Sect. 3: *The RANS-LUT model includes blockage effects and predict similar values of upstream rotor-averaged streamwise velocity for the Neutral and Unstable cases, as shown in Fig.3e* *and f, for $x/s < 0$. However, the RANS-LUT model overpredicts the wind farm blockage for the Stable case by about 1%, which is not fully understood.* In addition, we agree that the effect of wake superposition and rotor-averaging does not need to be discussed in both the methods and the results sections. We have decided to move these results to Appendix B.

Technical questions

1. In figures 3 and 4, in the unstable case, if my understanding of the graph is correct, the RANS-LUT results show an overestimation the rotor average wind speed for the entire row of turbines. This seems to be the case for all wind speed and spacing cases. Intuitively, overestimating the wind speed should lead to overestimating the power, but that does not seems to be the case. Indeed, on figure 8, it appears that for some turbines, the power obtained with the RANS-LUT is lower than the RANS-AD results. This is particularly noticeable for the 14 m/s cases (4D and 8D spacings). Can the authors comment on this point? I would recommend inserting a brief discussion on this subject.
   This is a good observation. The main reason for this mismatch in error signs regarding rotor-averaged wind speed and turbine power has to do with the fact that the RANS-AD and the RANS-LUT models calculate the power with different methods. In a former draft of the article, we had discussed this in detail but decided to remove it. We have now added a clarification in Sect. 3.3.

2. In section 2.3, it is mentioned that the LUT results are split in upstream/downstream parts. How is this split done? More precisely, how the speed up region is handled? The latter typically extend both upstream and downstream depending on the atmospheric stability. A cut based purely based on the streamwise location would result in including part of the speed up region in the blockage model and another part in the wake model which seems somewhat arbitrary. Could the authors comment on this point, and add a bit more details on how such a split is made?
   This is good point. In the RANS-LUT model implementaton in PyWake, the split is performed by streamwise location. The blockage and wake model LUTs are the upstream and downstream flows, respectively. This means that some of the speed ups along side the turbine are included in the wake model. We have clarified this in Sect. 2.3.1 by adding: *This results in some effects related to blockage, as for example the speed up in wind speed around the turbine wake, becoming part of the wake deficit model.* The split of wake and blockage models is also illustrated in a new figure, namely, Fig. 2.

3. Is the superposition model for the blockage model the same as that for the wake model?
   We use linear superposition for the blockage model and a weighted sum superposition for the wake model. It seems that we have forgotten to clarify the blockage model superposition in the article. We have added this information to a new figure describing the methods, Fig. 2, and have added the following to Sect. 2.3.3: *As blockage deficits are limited, they are also linearly superimposed—a valid approach as demonstrated by Meyer Forsting et al. (2023).*

4. What is the spatial extent of the LUT? Does it cover the entire CFD domain, or is there some cutoff after a certain distance?
   The LUT fields represent the entire CFD domain in horizontal space, while vertical we only keep the vertical levels covering the rotor area. We have added the following to Sect. 2.3.1: *To optimize memory usage, $\mathcal{D}_L$ retains only the vertical CFD layers spanning the rotor swept area—defined by the range between the minimum and maximum tip heights across all turbines within a farm.*

5. Related to the two previous questions. In figure 6, in the stable case, it appears that the LUT results overestimate the blockage upstream of the first row quite significantly. Visually at least, this might be an effect of the color map. What is the typical error in the upstream region compared to the RANS-AD results in this case? This is purely speculative but could this be a numerical artifact due to the density of the wind farm? For stable conditions, the effects of induction remain tangible quite far upstream even for a single turbine. If not forced to zero in the LUT, small - yet existing - deficits can be carried over large distances. I wonder if the linear superposition does not yield to an overestimation of the overall blockage effect for dense layouts, where many small contributions are added together. Such effect should be even more visible on the 4D case. I am curious to hear the authors' thoughts on that.
   We have changed the color map for Fig. 6 to be more color blind friendly. The comparison of the contour plots should be taken with care and we prefer to not make strong conclusion about them.

However, when looking at the difference between RANS-AD and RANS-LUT models in terms of the rotor-averaged wind speed for the turbine rows, and a below-rated wind speed, shown in Fig. 3e and 6, we obtain an error of about 1% upstream, which is also mentioned in the text. We do not fully understand why this error occurs; it could be an effect of the strong shear present in the Stable case but we are not sure.

6. In section 3.1, would it be possible to include a figure illustrating the error on the estimated turbine power for the different superposition models tested? I think this would really support the authors' claim about error cancellation.
   This is a great idea. We have added the results of turbine power in the figure in Appendix B.
7. How is the power P0, used to normalize the power and errors terms, calculated? Does it take into account effect of shear? For the same Uref, the rotor averaged wind speed - and thus the power - won't be the same for different stability regimes. Is this effect accounted for? This is probably minor, but could alter the interpretation of the wind farm's efficiency results.
   P_0 (now P_{ref}) is the power obtained from the simple generic turbine model power curve (Eq. 1) and only varies with wind speed. One could use multi-dimensional power curves, but this is out of the scope of the present work. In the revised article, we now refer to Eq. 1 in the text where the errors are defined (beginning of Sect. 3).

Minor comments

1. On line 14, shouldn't the reference to engineering wake models be plural to accommodate with the remainder of the sentence: "commonly referred to as engineering wake models".
   Adopted.
2. Similar comment to the following sentence, the use of singular sounds a bit odd. The use of plural sound more appropriate: "However such models often rely [...], and rarely take into account") since there are multiple engineering wake models available.
   Adopted.
3. On line 64, the comma after "respectively" should be replaced by a period: "respectively. The resulting normalized [...]".
   Adopted.
4. A period is missing on line 96 at the end of the line: "[...] are listed in Tab, 3.".
   Adopted.
5. A period is missing on line 113 after "constant": "[...] Kármán constant. However, [...]"
   Adopted.
6. The references on lines 196 and 239 should be put between parenthesis - or the sentence rephrased.
   Adopted.
7. On Figure 8's caption, the labelling of the subgraph seems to be incorrect. It should rather be "Wind turbine power (a-d) and RANS-LUT model error (e-h)..."
   Adopted.
8. On line 370, it seems that a space is missing after "RANS-AD model": " [...] RANS-AD model, but".
   Adopted.
9. This is just a suggestion, but the term P0 - used to normalize the error on power and farm efficiency - can be a bit confusing to the reader who might think it refers to the U0 used for generating the single-wake LUT, while it seems rather linked to the Uref parameter. I would recommend to rename it to Pref for consistency.
   This is a good point that we have adopted.

2 Reviewer 2

This manuscript proposes a RANS Look-Up Table (RANS-LUT) surrogate model derived from a single-turbine RANS Actuator Disk (AD) wake database. Expanding upon previous research, the authors incorporate dimensions for atmospheric stability—utilizing Monin-Obukhov Similarity Theory (MOST)—and wake-added turbulence intensity (TI). Furthermore, they implement an iterative superposition framework within PyWake to facilitate wind farm simulations. Methodologically, the study defines a comprehensive test matrix (comprising 1×8 and 8×8 arrays, 4D/8D turbine spacings, 11/14 m/s wind speeds, and stable/neutral/unstable atmospheric conditions) and integrates rotor-averaging techniques alongside a weighted-sum limiter during the superposition phase. In the results section, the authors present a comparative analysis of flow field discrepancies, power output errors, wind farm efficiency errors, and computational overhead. Ultimately, they highlight the critical role of wake-added TI in mitigating errors and note that while the proposed surrogate model accelerates computations significantly compared to full RANS simulations, it remains more computationally demanding than traditional engineering wake models.

I appreciate the ambition of this work and the extensive simulations conducted. The topic is highly relevant, and the manuscript shows clear potential. However, the presentation requires significant improvement. In particular, the methodological coupling lacks the clarity necessary to ensure reproducibility. Therefore, I recommend rejection in its current form, but I encourage the authors to revise and resubmit.

Specific Comments:

1. Lines 10-15: For wind turbines, fatigue loads typically arise from wake-induced unsteady turbulence. The current statement claiming that blockage effects inherently lead to increased loads is inaccurate, or at least requires heavy qualification regarding specific operating conditions.
   We have removed: *and increased fatigue loads.*

2. Lines 15-20: The phrase "as the thrust force distribution and wake rotation" should be revised to "including the thrust force distribution and wake rotation."
   Adopted.

3. General Note on Language: There are numerous grammatical errors throughout the manuscript. A thorough proofreading is necessary to meet publication standards. We have fixed several typos. Note that we are not native English speakers and language style errors are normally resolved during the proofreading step performed by the journal.

4. Introduction Structure: A paragraph should be added immediately following the first paragraph of the introduction. This new paragraph needs to explicitly discuss how atmospheric stability and blockage effects influence wake modeling.
   We understand the reasoning behind this suggestion. However, the effect of atmospheric stability on wind turbine wakes has been reviewed by Porté-Agel et al. (2020), which we already refer to in the introduction. We added the following in the introduction: *In addition, Porté-Agel et al. (2020) also reviewed the effect of different atmospheric conditions on wind turbine wakes, characterized by ambient turbulence intensity and atmospheric stability.*

5. Lines 20-25: Assuming the proposed model does not use linearized RANS, the authors should clarify this distinction upfront. I recommend starting this paragraph by outlining the different methodological approaches, explicitly distinguishing between linearized RANS and direct RANS solvers, to better position your chosen method.
   The reviewer correctly understands that our RANS-LUT model does not used linearized RANS single wake shapes. We prefer to keep the structure of this paragraph, as it builds up the model fidelity, starting with linearized RANS as the lowest fidelity among these models. Note that we have added *linearized RANS* in Table 1, to highlight the difference between Fuga and the full RANS models. In addition, we start the paragraph by introducing the full RANS wind farm flow model and RANS model surrogates, and we introduce Tab. 1 earlier.

6. Lines 20-40: The review of past methodologies in this section is currently too scattered. It must be reorganized to provide a clear, logical storyline that properly contextualizes the evolution of these

methods and leads smoothly into the current work.
See answer to Comment 5.

7. Lines 30-35: The statement that "the effect of the thrust coefficient is nonlinear even for low thrust coefficients (e.g., $C_T = 0.1$) because the wake recovery increases with C_T" lacks a clear physical justification. The authors must elaborate on the underlying aerodynamics to explain exactly why this relationship results in the observed nonlinearity.
   We have added: *A higher CT leads to larger velocity gradients that reduces the downstream extend of the near wake, as shown by LES of Sørensen et al. (2015).*

8. Lines 40-45: The sentence beginning with "In this work..." introduces the core contributions of this study. It should start a new paragraph to clearly delineate the end of the literature review from the present methodology.
   Adopted.

9. Lines 40-45: The articulation of the study's contributions is confusing. The text mentions an "extension" of previous work alongside an "alternative superposition method." The authors need to clarify the hierarchy of these updates: is the improved superposition method a core component of the main "extension," or does it represent a secondary, distinct improvement to the model?
   We have added the following after *..., which is based on the momentum conserving superposition method of Zong and Porté-Agel (2020)*: *The latter leads to a more consistent model that can be used to obtain good results of both the wind farm flow and turbine power.* In addition, we added *consistent* to the title to better reflect the contributions of our work.

10. Table 1: The use of the term "dimensions" in this table is currently confusing. It is unclear how these dimensions link to the proposed model, for example, whether they represent dependent variables of what, without searching through the main text. Furthermore, the relationship between the "velocity deficit," "wake-added TI," and the LUTs is ambiguous. The authors must clarify whether these are the specific output parameters retrieved from the lookup database or if they serve another function. As tables in scientific manuscripts should be entirely self-contained, I recommend redesigning the table for clearer categorization or providing comprehensive explanations in the footnotes.
    We have changed the column labels to *LUT flow variables* and *Input dimensions*, and updated the caption. The relationship between the velocity deficit and wake-added TI LUTs is already introduced in the introduction.

11. Lines 50-55: "The RANS-LUT surrogate model is based on a database of single wake simulations" should be changed to "The proposed RANS-LUT surrogate model for all atmospheric stability conditions, including stable conditions, is based on a database of single wake simulations" to make it more specific.
    We adopted the word *proposed*. The RANS-LUT model is based on single wake simulations with multiple input dimensions, and this is explained further down in the text.

12. Lines 50-55: "RANS-AD simulations are employed" should be changed to "RANS-AD simulations are employed to generate the aforementioned database and to validate the RANS-LUT model."
    We have adopted *RANS-AD simulations are employed to generate the aforementioned database.*

13. Lines 50-55: "The RANS-LUT model of Criado Risco et al. (2023) and proposed extensions are discussed in Sect. 2.3." should be changed to "The original RANS-LUT model of Criado Risco et al. (2023) for the neutral case and its proposed extensions are discussed in Sect. 2.3." This clarifies the relationship between the original model and the current one.
    Adopted.

14. Lines 55-60: The phrase "a wind turbine row and a square wind farm" lacks scientific precision. Please use more formal terminology (e.g., "an array of wind turbines" or "a regular grid wind farm").
    We now use *a row of regularly spaced wind turbines* and *a square regular wind farm layout*. Note that a turbine array could also be interpreted as a 2D shape. It is also common in literature to use *turbine row*.

15. Lines 55-60: Regarding the "270 and 315 degree" inflow angles: what are these angles relative to, and why was zero not used? Please make this crystal clear in the text.
    We have added: *…, where* 270 *and* 315° *are aligned with a wind farm edge and a wind farm diagonal, respectively.*

16. Lines 60-65: Regarding the "below- and above-rated wind speeds": why was it necessary to select these two extremes? Please specify your reasoning.
    This is because below and above-rated wind speed cases lead to different thrust coefficient distributions in the turbine arrays and farms. This has now been clarified.

17. Lines 60-65: You have three reference turbulence intensities (TIs) and three reference stability parameters. Should these be paired? For example, under stable conditions, the typical TI should be very low; therefore, 5% might be a better choice than 10%.
    This is true for the inflow profile. However, we assume that the effect of inflow TI is the same as the effect of wake-added TI, and this leads to challenges for the stable single wake database. This issue is discussed in detail in Sect. 2.2.3 and also mentioned as a limitation in Sect. 3.6.

18. Lines 65-70: How is the "NREL-5MW reference turbine" linked to the general turbine model? If you are only using a general turbine model, you could simply state that you use a general model that could also represent the NREL-5MW reference turbine.
    We believe that this is already clearly written in present text.

19. Lines 75-80: Please provide a more detailed introduction to the Actuator Disk (AD) model and explain how it is used to simulate wind turbine effects in your framework.
    All important details are already mentioned and the reader is referred to the listed references about the employed AD model. We acknowledge that the AD model is an important part of the RANS-AD modeling framework. However, we do not desire to include all details of all those previous works, as this would cover several pages.

20. Lines 80-85: This section is confusing. If there is no detailed information provided about the AD, why is a polar grid needed? Furthermore, under what framework are the thrust and tangential forces calculated?
    The distributions are based on the analytic rotor model of Sørensen et al. (2020) as mentioned in the text. AD models can be based on a separate polar grid or the AD forces can be directly injected in the CFD domain. However, the prior is preferred because fewer AD cells are needed to represent a circular shape.

21. Lines 80-85: Regarding the statement, "The force distributions are scaled with the local shear, while maintaining the input integral forces": why is this necessary? What does this mean, how does it fit into your framework, and which specific components are affected?
    The original analytic rotor model of Sørensen et al. (2020) is inconsistent regarding the input thrust coefficient and output thrust coefficient when shear and or veer is present. A rescaling of the force distributions fixes this issue, which was proposed in a previous work (van der Laan et al., 2020), as mentioned in this Section.

22. Lines 85-90: Please explain why an "inner refined domain" is needed.
    We have added: *The inner refined domain is employed to resolve the wind turbine wake(s), it has dimensions* $24D \times 5D \times 3D$*, it is placed at* $4 \leq x/D \leq 20$ *and* $2.5 \leq y/D \leq 2,5$*, and contains a mesh with uniform horizontal spacing of* $D/8$*.*

23. Lines 90-95: You state that "A finer grid spacing may be necessary for stable inflow cases depending on the user's quantity of interest." If this is the case, a grid resolution sensitivity test should be provided to justify the chosen spacing.
    For the present study, we test our RANS-LUT surrogate model against the RANS-AD model with the same grid spacing. One could refine both models if desired by the user. We have added a grid refinement study in Appendix C.

24. Lines 95-100: Regarding the statement, "We employ a two-equation k-ϵ turbulence model that is in balance with MOST": Please clarify how it is in balance with Monin-Obukhov Similarity Theory (MOST) and explain why this balance is necessary for your methodology.
    We have added: *.., which is important for isolating the turbine and wind farm flow effects.* In the same section, we already further discuss the inflow development and the use of 1D precursor to remove numerical errors that can otherwise lead to a minor downstream development of the inflow.

25. Equations 2-4: Is this a realizable two-layer RANS model based on your formulation in Equation 2? If so, please specify the formulation of f_p and explain how ϵ is formulated as it approaches the ground.
    Yes, the f_P function reduces the C_μ coefficient in regions of high shear, which mitigates the problem of the standard k-ε model regarding unrealizable Reynolds-stresses for high shear regions. Physically, the f_P is best understood as a near-wake turbulence length scale limiter. The full expression can be found in the provided reference (Baungaard et al., 2022a).
    The boundary conditions of ε at the wall uses a dirichlet condition, where its value is set to the logarithmic solution, as discussed in the provided reference, van der Laan et al. (2024). The reviewer can also take a look at Sørensen et al. (2007).

26. Lines 105-110: "The MOST profiles are in balance with the turbulence model using an additional source term S_k and a height-dependent $C_{\epsilon,3}$." Please provide more detail on exactly how this balance is achieved.
    We have added: *They are derived by substituting the MOST similarity functions of the normalized shear and potential temperature gradient into the k-ε turbulence model; the full expressions of S_k and $C_{\varepsilon,3}$ are provided in van der Laan et al. (2017).*

27. Lines 110-115: "We use a constant turbulent buoyancy source term, $B = u_*^3/(\kappa L)$, that is better suited for wind turbine wakes subjected to unstable conditions." Please explain the physical reasoning behind why this formulation is better suited for unstable conditions.
    We have clarified this using: *The original turbulence model used a buoyancy source that was a function of the stability and local shear. However, this can lead to a non-physical wake recovery for unstable conditions, where an increase in turbulent buoyancy production could lead to less wake recovery. In the present work, we use a constant turbulent buoyancy source term, $\mathcal{B} = u_*^3/(\kappa L)$, which solves the wake recovery problem for unstable conditions (Baungaard et al., 2022a).*

28. Lines 110-115: "Using a constant buoyancy source term for stable conditions can lead to numerical instabilities." Please elaborate on the root cause of these numerical instabilities.
    The problem is related to obtaining a negative TKE and eddy viscosity for stable conditions. We have now added the following in the text: *For stable conditions, the constant buoyancy source is negative and reduces k. When the k-equation is solved for, the other source terms can vary, while the buoyancy source remains constant, and this can lead to negative k values.* and: *The idea of using $\nu_T/\nu_{T,\mathrm{MOST}}$ in $\mathcal{B}$ is based on the fact that both the original buoyancy production and the turbulent shear production also scale with ν_T.*

29. Equation 5: The reasoning behind this modification is confusing. Please provide a clearer physical or mathematical justification for why this modification is needed.
    See answer to Comment 28.

30. Lines 115-120: How is the parameter f_p linked to the typical formulation of the two-layer realizable k-ϵ model? Please clarify this relationship.
    This is an interesting question. We have an article on this topic, showing that several modified k-ε models have an f_P function that have a similar behavior van der Laan and Andersen (2018). This reference has now been provided in the article where we discuss the f_P function.

31. Lines 125-130: "Even though a turbulence model is employed that is analytically in balance with the turbulence model..." This appears to be a typo or redundant phrasing. Please review and revise.
    Thanks for pointing this out, we have fixed this typo as: *Even though a turbulence model is employed that is analytically in balance with MOST, numerical deviations can occur.*

32. Lines 130-135: "The friction velocity from Eq. (6) is rescaled to get the desired wind speed at the reference height. These scaling factors are 0.9901, 0.9959 and 0.9962, for the Stable, Neutral and Unstable cases, respectively." Please explain the physical or numerical justification for why this scaling is necessary, and detail exactly how it is applied.
    We have added more information in this part: *The friction velocity from Eq. (6) is rescaled to get the desired wind speed at the reference height,* U_{ref}*, using Reynolds-number similarity. These scaling factors, computed as* $U_{\mathrm{ref}}/U_{\mathrm{1Dprecursor}}$*, are 0.9901, 0.9959 and 0.9962, for the Stable, Neutral and Unstable cases, respectively. The scaling factors indicate the numerical error in the streamwise velocity at the reference height.*

33. Lines 135-140: Are "free stream velocity" and "inflow wind speed" referring to the exact same quantity in this context? If they are synonymous, please use consistent terminology throughout the manuscript to avoid reader confusion.
    Yes, they are the same. We have now used inflow wind speed everywhere, except where the errors are defined at the start of Sect. 3.

34. Line 140: You state that "the inflow wind speed is not a relevant parameter." However, the transition between below-rated and above-rated wind speeds causes significant changes in turbine operation (such as thrust coefficient variations) and subsequent wake behavior. Please clarify this statement or revise it to reflect these operational differences.
    The inflow wind speed can be set to 1.0 m/s or 100 m/s, as long as the operational parameters as C_T, C_P and the tip speed ratio are set correctly. This is discussed in detail in the provided reference (van der Laan et al., 2020). We have added: *The prior holds if the operational parameters as* C_T*,* C_P *and* λ*, are set correctly.*

35. Lines 140-150: The explanation of how to reduce eight dependent variables to two is well-written and clear. The rest of the manuscript should strive to follow this level of clarity.
    Thank you.

36. Lines 155-160: The statement, "This is overcome by replacing the stable single wake cases for $I_{\mathrm{ref}} \geq 0.1$ in the database with the neutral single wake cases," is unclear. Please elaborate on the justification and the mechanics behind this replacement.
    We have modified this to: *This is overcome by replacing the stable single wake cases for* $I_{\mathrm{ref}} \geq 0.1$ *in the database with the neutral single wake cases, which is justified by the fact that a high* I_{ref} *often correlates with neutral conditions.*

37. Lines 160-165: Why are there 153 cases? Based on your selected combination of parameters, there should be $7 \times 3 \times 9 = 189$ cases. A follow-up question: for stable conditions, some of these parameter combinations seem unrealistic (as mentioned previously). For instance, a lower Turbulence Intensity (TI) should be selected for stable conditions. Please clarify this discrepancy in case numbers and parameter selection.
    There are 153 cases, because we skip running the stable cases for $I_{\mathrm{ref}} \geq 0.1$. This also answers the follow up question and we like to refer the reviewer to Comment 17. We have added: *Note that the stable single wake cases for* $I_{\mathrm{ref}} \geq 0.1$ *are not simulated, leading to* $189 - 36 = 153$ *cases.*

38. Lines 165-170: How is "background shear" defined in this context? Please provide the exact mathematical definition or formulation.
    We have added the following in Sect. 2.3.2: *Generally the background flow in PyWake is allowed to vary freely in space, yet here we ensure it follows the RANS inflow conditions that only include vertical shear following MOST:*

$$U_b\left(z\right) = U_{ref}\frac{\ln\left(z/z_0\right) - \Psi_m\left(\zeta\right)}{\ln\left(z_{\mathrm{ref}}/z_0\right) - \Psi_m\left(\zeta_{\mathrm{ref}}\right)}, \tag{1}$$

    with Ψ_m *as the integrated normalized shear from MOST, as defined in van der Laan (2017).*

39. Line 175: Please define the acronym "AEP" (Annual Energy Production) upon its first use in the text.
    Adopted.

40. Lines 175-180: "An iterative approach is employed to obtain the effective wind speed and TI." Please explain why an iterative method is necessary here rather than a direct calculation.

    We have added: *Due to the inclusion of up- and downstream effects, an iterative approach needs to be employed to obtain the effective wind speed and TI at all turbines.*

41. Lines 175-180: The text mentions "engineering wake and blockage models." However, I do not see any description in the methodology of how blockage effects are simulated. Please ensure the methodology explicitly covers blockage if it is being referenced here.

    We have discussed how the LUTs are split into upstream and downstream LUTs to model blockage and wake effects. We have added more information in revised regarding this, see Comment 2 of Reviewer 1.

42. Lines 175-180: "...by interpolating and scaling a wake and blockage shape from the LUTs using the local wind speed (C_T) and TI at each turbine position." This phrasing is confusing. Please clarify exactly what is being interpolated and how the local wind speed and thrust coefficient (C_T) are utilized in this scaling process.

    We fully agree with the reviewer that this part lacks a clear explanation. We have added a subsection 2.3.2 where the scaling, rotor-averaging and superposition operator are presented and discussed with equations.

43. Lines 180-185: Assuming that the effect of inflow TI on a single turbine is identical to the effect of wake-added TI on a turbine inside a farm is a very strong assumption. Please explicitly justify this assumption and validate it, either through references to existing literature or by comparison with an open database.

    We agree with the reviewer that this is a strong assumption, and we also mention this in the model limitation section. However, we would like to point out that excluding wake-added TI leads to large errors in the velocity deficit of a turbine row, as shown in Fig. 6. When we include wake-added TI using the wake-added TI LUTs that are generated by changing the inflow TI, we do get much better results of the turbine row deficit with respect to the results of the RANS-AD model. This justifies our assumption indirectly. More research should be performed to further investigate the assumption, which is out of the scope of the present article.

44. Lines 180-185: "Finally, the inflow shear following MOST is added as a post step to the superposed wind farm flow." Why is this added as a post-processing step rather than being integrated natively? Please explain the physical or numerical rationale.

    Thank you for pointing this out, we did not write this correctly. The shear is exactly added through out the iterative solving procedure as written in the revised version, Sect. 2.3.2. However, for a wind farm consisting of turbines with the same hub height, the shear does not change the power. This is different from a RANS-AD wind farm simulation. We have added the following in the same subsection: *This signifies that background flow variations over the rotor area (shear, veer) do not impact the effective wind speed and consequently neither turbine power nor thrust. Note that this differs from the way the RANS-AD reacts to the flow, which responds to the local variation over the AD instead, i.e* $U_b(x_i) - \hat{U}_j \Delta\tilde{U}_j(x_{i,n} - x_{H,j})$.

45. Lines 190-195: You note that this method "leads to a well performing model in terms of the flow field but strongly underpredicts wind turbine power production." Consider clarifying in the text that this occurs because the flow field depends linearly on velocity (u), whereas power production depends on the cube of the velocity (u^3). Taking the average of the flow field will naturally underpredict the power due to Jensen's inequality.

    This is not problem that we discuss here. The problem is about error cancellation and we have now moved this part to Appendix B, where we also added more results and discussion. See Comment 6 of Reviewer 2.

46. Lines 195-200: The explanation regarding calculating "superposition weights iteratively based on the ratio of the single wake convection velocity and the summed convection velocity" is hard to follow. Please rephrase this for clarity or provide a supporting equation to guide the reader.

We understand the present explanation of the weighted superposition method is hard to follow without prior knowledge of Zong and Porté-Agel (2020). In addition, Zong and Porté-Agel (2020) did not provide a numerical recipe how to implement their method. We have now added equations and text how explaining how our implementation in PyWake works in Sect. 2.3.3. Here, we also discuss the need for limiting the superposition weights to 1, to enforce that the individual wakes are not convected faster than the background flow.

47. Lines 200-205: "The weighted superposition method is implemented with efficient analytical integrals assuming a Gaussian velocity deficit." This assumption is questionable, as a Gaussian profile may not be applicable under stable atmospheric conditions. Please justify this choice and discuss its potential limitations.
    We agree with the reviewer since the near wake in stable conditions extends further downstream. A future work could further investigate alternative profiles for the near wake, but this requires also changes in the superposition model since the latter is currently using a Gaussian.

48. Methodology Section: It is highly recommended that you include a detailed flowchart of your methodology. The current text-only description makes it very difficult to follow the complex sequence of steps, iterations, and inputs.
    This is good point that we also considered to add for the initial submission, but we lacked the time to do so. We have now added a method plot (Fig. 2) including an overview of the RANS-AD single wake database.

49. Figures 3 and 5: It appears that the stable conditions yield the best results. This is counterintuitive compared to typical modeling expectations. Please discuss this finding in the text and explain why the model performs best under these conditions.
    This depends on what error is looked at (velocity or wake-added TI) and at which downstream location. For example, the Stable case performs worst for the near wake of the first turbine wake and upstream of the turbine row. We currently do not have more insights in why certain errors are larger than others with respect to the investigated stability cases. The models results do include errors from several sources as wake superposition, differences due to the use of a background shear as a post step in RANS-LUT, while the RANS-AD model using the shear directly in the RANS equations, etc. We have discussed some of the compounded errors in Appendix B.

50. Figure 6: There is a significant difference upstream between the RANS-AD and RANS-LUT results under stable conditions. What is the cause of this discrepancy? Please address and explain this in the text.
    See Comment 5 of Reviewer 1.

3 Own changes

We have made the following additional changes:

1. Changed to a more color blind friendly color scheme in Fig. 6 (contour plots).

2. The wake superposition method based on the weighted sum employs fitted velocity deficit profiles based on a Gaussian function. Initially, we made a fit for every downstream distance with an interval of 1 rotor diameter, D. We have decided to now use the original RANS-AD grid cell centers leading to an interval of $D/8$. In addition, we only perform a fit at $x = 0$ and $x > 2D$, which partly avoids the problem of fitting a Gaussian profile to the near wake. Overall, these choices only lead to only minor model changes. We have updated all the RANS-LUT results accordingly. The following is written in Section 2.3.1: *The fitting is performed over the downstream plane at hub height,* $z' = 0$ *and* $2 \leq x'/D \leq 100$, *with an additional fit at* $x'/D = 0$ *where the magnitude is limited to the axial induction from 1D momentum theory, see Fig. 2e.*

3. The RANS-LUT was previously implemented in a development of PyWake. During the review process, this implementation has been released in PyWake v2.6.18. Furthermore, the open git repository including the turbine row examples has been updated: https://gitlab.windenergy.dtu.dk/TOPFARM/pywake_ranslut/-/tree/v0.4. This repository can be used to recreate Figs. 3, 4 and 8.
4. We have added *consistent* to the title to better reflect the content of the paper. This is because we propose to use a weighted superposition method and rotor-averaging leading to a more consistent model that does not rely on error cancellation compared to using linear superposition without rotor-averaging.
5. Section 2.3 has been rewritten into several sub sections including additional text and equations describing the RANS-LUT model in more detail.

References

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