Reply to the reviewers of "A framework for the superposition of wind turbines wake properties"

5 February 2026

We would like to thank the Reviewers for their exceptional, thoughtful, and constructive reviews of our manuscript. We did not realize how disorganized our paper had become. We initially wanted to identify the best superposition method for TKE using our own TKE model (KA2025) and found that there was no prior literature on this, only on TI. We quickly realized that the existing literature on TI superposition was very confusing, to the point that we almost could not implement many of the published methods and had to email the authors for clarifications. That's when we came up with the idea of a framework, to prevent other researchers to deal with the same issues. However, we did not update the structure of the paper to reflect the shift in focus. In this revision, we restructured the manuscript to reflect the framework better and hopefully this will satisfy many of the Reviewers' concerns.

Please note that the Reviewers' comments below are in *italic*, our responses in regular font, and the changes to the manuscript in blue color.

Both Reviewers used comments embedded in the article's PDF to provide additional feedback. We found this method of providing input to be really difficult to address. We decided to copy and paste each of these comments using a PDF-reader and then identify them by page and line number in our response under the heading "Comments from the PDF". Please note that the page and line numbers refer to the original manuscript, not the revised version. If possible, we ask that embedded comments not be used in the next round of reviews.

Reviewer #1

In general, I found the manuscript difficult to follow overall, requiring a significant amount of effort to keep track of the authors' reasoning. Moreover, I am not convinced that the authors have clearly established the "framework to standardize wake superposition" that they claim to propose. Even if such a framework is presented, it is not clear what is the concrete contribution of this work to the wind energy community or to the broader knowledge base. Several issues resulting in these concerns, along with additional comments, are outlined below.

We fully agree with this comment and we have made efforts to improve the readability of the manuscript, starting with a clearer structure.

1. *I am not convinced that the title "A framework for the superposition of wind turbines wake properties" accurately reflects the scope or contribution of the work. In my opinion, a title that can directly reflect the central of this study, which is studying the exponent of NSS, would be more proper. As it stands, the current title feels over-ambitious and gives the impression of presenting a completed or definitive framework, which I cannot endorse based on the current content. Moreover, the work does not appear to pioneer this area, and the current title risks overstating the contribution in a way that could unintentionally overshadow or absorb the contributions of earlier studies. Therefore, in my view, the title should be adjusted to one that more realistically conveys the incremental nature of the contribution.*

   We definitely failed at explaining the need for a formalized framework in the previous version of the manuscript. The literature is truly a mess when it comes to this topic. First of all, no paper is available for TKE superposition, ours is the first to address the topic. Many papers have been written on ΔU

or TI superposition, but almost every one introduces its own names for the superposition methods and often there is confusion even about the meaning of the terms involved (e.g., wind speed deficit at the turbine or downstream of it? What value should be used at the denominator to normalize, the upstream or the inflow value?). In two cases, we had to email the paper authors to ask clarifications on how to apply their methods and in one case some of the necessary steps were not even mentioned in their paper. A coherent framework on this topic is sorely needed and the good news is that we were able to identify just two main formulations (not dozens!) that captured the majority of the published methods. This is really valuable in our opinion.

2. *Additionally, I find several crucial prior works that are highly relevant to the current work are not mentioned, for example: Amin Niayifar and Fernando Porté-Agel (2016, https://doi.org/10.3390/en9090741), Haohua Zong and Fernando Porté-Agel (2020, https://doi.org/10.1017/jfm.2020.77), and Majid Bastankhah et al. (2021, https://doi.org/10.1017/jfm.2020.1037). These works, in my opinion, fulfill at least two out of three propositions motioned in the manuscript (perhaps missing the third). Comparing these prior results with the currently proposed framework may help advance the development of wake modeling.*

   We added these papers to our literature review and we agree that they already include some of the framework propositions. The framework is in fact based on findings from the literature, so it is actually very good that our propositions are consistent with prior literature. But this fact does not diminish the importance of having the framework fully spelled out in a general form and thoroughly validated with three benchmark models against terabytes of LES results from other research teams.

   L. 178 - 193: "Using the same definition of wind speed deficit as in Eq. 15, but with the linear approach in Eq. 12, Niayifar and Porté-Agel (2016) proposed a fourth superposition method for wind speed:

$$U = U_\infty - \sum_{i=1}^{N} (U_{in,i} - U_i) \,. \tag{1}$$

   By considering the conservation of the total momentum deficit in the streamwise direction, Zong and Port´e-Agel (2020) introduced a weighting factor to the superposition method developed by Niayifar and Port´e-Agel (2016). They indicated that the total wake velocity deficit can be expressed as a weighted sum of the individual wake velocity deficits, where the weighting factors are defined as the ratio of the wake convection velocity, an x-dependent representation of the mean velocity in the entire cross section of the wake, to the convection velocity of the combined wake. To apply this momentum-conserving method, surface integrals of the wake velocity deficit must be calculated multiple times at each downstream location to achieve convergence through an iterative procedure, which increases the computational cost, especially when modeling large wind farms. The improvements in predicting the velocity deficit from particle imaging velocimetry measurements were not significant. Bastankhah et al. (2021) developed an innovative approach that does not rely on an explicit superposition method *a-priori*, but uses the conservation of momentum deficit to obtain directly the wind velocity field downstream of any waked wind turbine in a farm. The resulting formulation is formally equal to the linear superposition of wind speed deficits in Eq. 12 or 17. The difference is that the ΔU term is obtained neither from U_∞ nor from U_{in}, but from the difference between the resulting wind speed with and without a given waked wind turbine. "

3. *I find the overall structure of the manuscript to be relatively disorganized. This is particularly evident in the abstract and the introduction. As a reader, I often felt overwhelmed by fragmented pieces of information presented without a coherent narrative. Key ideas are scattered across multiple paragraphs, forcing the reader to piece together the intended meaning. I strongly recommend that the authors substantially restructure the manuscript to improve clarity, logical flow, and overall readability. Although this may be somewhat unconventional, may be even controversial, to note, I encourage the authors to consider make use of modern writing-assistance tools to help polish the writing, improve clarity, and reduce the fragmentation. This could substantially enhance the readability and accessibility of the manuscript.*

   We agree that the structure of the previous version of the manuscript was terribly disorganized. We made major efforts to improve it. For example: 1) we moved the description of the KA2025 TKE model, which had its own section 2.1 in the Methods in the previous version, to the Introduction, together with the other two benchmark models for wind speed and for TI; 2) we used subsections for each of the four propositions of the framework; 3) we divided the results by property (wind speed, TI, and TKE) rather than by LES dataset, with subsections; and 4) we removed several figures from the main text and from the Appendix that we had included in the previous version for completeness but did not really convey any new message.

4. *The presentation suffers from fragmentation and a lack of clean, consistent notations. A large number of equations, symbols, and acronyms are introduced throughout the manuscript, but many of them are not properly defined. Several variables appear abruptly or are only loosely described, which makes the mathematical development difficult to follow. While I recognize that some parameters are used only once and may not require extensive explanation, many important quantities are treated with the same level of brevity as those that are incidental. This often left me uncertain about the authors' intent and the precise meaning of the expressions. I strongly recommend that the authors highlight the key components/building-blocks of the proposed framework more clearly, potentially through summary tables, as the literature the authors have cited (tables 2 and 3 of QI2021). Also, in my opinion, providing explicit definitions for all parameters that play a central role could definitely help. Additionally, the authors should reconsider the sequence in which equations and symbols are introduced to ensure a logical and accessible progression.*

   We have indeed a lot of symbols and variables in the paper. We had to introduce three benchmark analytical wake models (for the three wake properties of interest), each of which has many symbols and coefficients, and then we reviewed about ten superposition methods, which had to be "interpreted" and sometimes had additional symbols. The tasks of using a consistent notation and defining everything were enormous and we clearly did not do it well. We have now made sure that all symbols are defined and all concepts introduced. We introduced specific sub-sections for each of the propositions (i.e., the building-blocks). We added a summary table with the final form of all the equations needed to reproduce the results (Table A3), similar to Table 2 in Ishihara and Qian (2018), as well as two other tables with the expressions and values of all the parameters of the QI2021 and KA2025 fitting coefficients (Tables A1 and A2).

5. *Related to my previous comment, many important equations and parameters are introduced in the Introduction section, while another substantial portion is introduced later in the Methods section. This split presentation significantly disrupts the narrative flow and makes the manuscript difficult to follow. Consolidating the definitions and presenting the mathematical elements in a more coherent and centralized manner would greatly improve readability and comprehension.*

   In the revision, we moved the description of the KA2025 TKE model, which had its own section 2.1 in the Methods in the previous version, to the Introduction, together with the other two benchmark models for wind speed and TI.

6. *The figures in the appendix are presented without sufficient explanation or context.*

   We removed almost all the figures from the Appendix because we realized that we had included them in the first version for completeness (e.g., all vertical profiles from all LES datasets) but they did not really add anything to the main story of the paper. The only figures left are now those that relate to the QI2021 wake model for TI. We discovered that it has a flaw: the $\delta(z)$ term is ill-defined because it is not a function of x, but it has the same magnitude for all x points downstream of any turbine. As such, with several inline turbines, this large extra term adds on large amounts of TI and causes unrealistic bumps in the profiles (e.g., Fig. 10 top row). We contacted the authors about it and they said that the $\delta(z)$ term should set to zero after $10D$, but never mentioned it in their paper. We therefore wanted to explain the issue (Fig. A1) and verify that the proposed fix would work (Fig. A2), which it did not (Fig. A3), but we do not think that this discussion belongs to the main text.

With regard to the scientific methods and findings presented in the manuscript, I have several additional concerns, which are outlined below:

7. *It is unclear to me how broadly the "second proposition" is applied within the framework. For example, when using Eq. (24), should the formulation U_∞ or $U_{in,i}$ be employed? I recommend that the authors explicitly present the final set of equations and parameters used in the framework to avoid ambiguity and ensure reproducibility. This can be done in a dedicated section or an appendix.*

   The second proposition is applied everywhere after we introduced it. In the literature review, where we introduce the three benchmark models, the framework had not been introduced yet so the original notation was retained. In the second proposition, we explicitly state that "the inflow value should also be used at the denominator to normalize the property", thus $U_{in,i}$ should be employed. To fully address this valid, we decided to add Table A3, which contains the final form of all the wake models for both superposition methods.

8. *Related to the previous point, I am not sure whether the prediction of turbulence is actually coupled with the estimation of velocity in the current manuscript, as it is unclear to me whether the "second proposition" is applied universally across the framework. If the two fields are indeed coupled, it is not stated how the authors compute TI when predicting the velocity deficit field. Is it obtained using SS or NS, and what value of m is adopted in the framework? Clarifying these choices is essential for understanding the internal consistency of the framework and for assessing how the turbulence predictions relate to the velocity formulations. Moreover, physically/mathematically explaining how these engineering-model-predicted properties would, in my view, be very valuable to the wind energy community.*

   We do **not** need TI to calculate the velocity deficit field. As Eqs. 4–7 show, there is no TI anywhere. But we do need to calculate the wind speed field in order to use the QI2021 and the KA2025 methods for added TI and added TKE because both equations rely on the the thrust coefficient, which is a function of the inflow hub-height wind speed $U_{in,i}(H)$. We originally used the flow chart to explain this, but in one of your annotations you did not think it was necessary and therefore we removed it from the new version of the manuscript. However, the explicit dependency of C_T on $U_{in,i}(H)$ is presented in Table A3. The other relevant text about this is:

   L. 299 - 301: "Furthermore, $\mathcal{P}_{in}$ should also be used to obtain the values of the tuning parameters (e.g., TI_∞ in Table A2 should be TI_{in}) and to calculate the thrust coefficient $C_T(U_{in})$."

   L. 380 - 382: "Since $U_{in}(H)$ is the input variable to obtain the C_T value (Fig. 3), which is a necessary parameter to calculate U with Eq. q. 4 , TI with Eq. q. 8 and TKE with Eq. q. 11 , the findings about U superposition from Section 3:2 will be applied to TI^2 and TKE in Sections 3:3 and 3:4."

9. *I find that the conclusions drawn are rather superficial and may be highly case-dependent. In particular, it is unclear why the authors do not advocate using SS with m = 1.0 if the "second proposition" is to be fully upheld. My concern is based on the basic identity $P_1(x, y, z) = P_\infty(z) + \Delta P_1(x, y, z)$, which holds regardless of whether SS or NS is used, and regardless of the value of m. By extension, one could equally argue for $P_i(x, y, z) = P_{in,i}(z) + \Delta P_i(x, y, z)$. This is especially relevant if one assumes $P_{in,i}(z) = P_{\infty,i}(z)$, an equivalence that the "second proposition" appears to imply in my perspective. I therefore find it unclear why the formulation does not naturally lead to the use of SS with m = 1.0, as I think it is physically more sound. Clarification will be appreciated.*

   The old second proposition was eliminated in the new version.

   The correct form of the added property is now in Eq. 30 (previously Eq. 33): $\Delta\mathcal{P}_i = \mathcal{P}_i - \mathcal{P}_{in,i}$, where $\mathcal{P}_{in,i} = \mathcal{P}_{in,i}(z)$. This is the added property by a **single** turbine. But when multiple wakes overlap on one another, even though the SS method with $m = 1$ would seem to be the most natural, it does not necessarily perform best. Exploring this issue is one of the contributions of our paper.

10. *It is not clear to me why Equations (3), (6), and (24) are given particular emphasis in the manuscript, especially given that there are many well-established alternatives in the literature (e.g., those appeared in the three references mentioned above). Their selection and role within the overall framework are not sufficiently explained, and it is unclear how they bridge to the subsequent analysis. I recommend that the authors clarify the specific importance of these equations and physically explain why they are central to the development of the proposed methodology/framework.*

To assess the performance of the two superposition methods, one has to use an analytical wake model to obtain the $\Delta\mathcal{P}$ term (whether normalized or not). We could not possibly test them all, therefore we chose the three wake models in (old) Eqs. 3, 6, and 24 (now Eqs. 4, 8, and 11). They are not particularly "central" to our study, one could choose different analytical models to see how the superpositions work. We wanted to select wake models that were analytical, because they will be ultimately used in wind farm parameterizations within existing mesoscale models, and that were well known. From our experience with wind speed deficit models, the XA2015 model is superb (Archer et al. 2018) and it is already available in the WRF model. For TI, there were fewer choices and QI2021 was the most cited. For TKE, we used our own model because it is the only analytical one for TKE in the literature. In addition, please note that all references that you provided above are about the velocity deficit, not TI or TKE.

These explanations are in the revised text:

L. 114: "The XA2015 analytical model will be used later as the benchmark for wind speed because of its excellent performance (Archer et al., 2018) and because it is already available in the WRF model in the MAV WFP (Ma et al., 2022a)."

L. 136: "The QI2021 model will be utilized as the benchmark for added TI in this study because it was the first three-dimensional analytical model and because it is widely cited in the literature"

L. 153 - 155: "Since the KA2025 model is the only analytical model for added TKE in the literature and it in good agreement with LES and wind tunnel data under different atmospheric stabilities and turbine characteristics, it was selected as the benchmark for added TKE in this study. "

11. *Related to point 3, I currently do not clearly understand why NS or SS would be considered more physically sound than LSS (linear sum of squares). The manuscript does not provide sufficient justification for favoring NS or SS from a physical/mathematical/theoretical standpoint, nor does it explain under what conditions LSS would be inappropriate. I encourage the authors to elaborate on the physical rationale that distinguishes these approaches. Additionally, in the work that established QI2021, they have used an approach to superposition* $sigma_u(i.e., TI)$ *that was different from NS and SS, therefore, I don't think the comparison in this work, for example figure 12, is fair.*

    This comment profoundly impacted us and it is the reason why we now use TI^2 instead of TI as the property of interest. TI is not a good variable to use to study turbulence superposition because the standard deviation has no statistical or physical properties, only the variance does. The LSS model is indeed physically sound (in fact it is also used in the IEC standard!), while the SS and NS caused all kinds of problems in our previous version precisely because the standard deviation is not an additive variable. What we discovered is that, by using TI^2, the LSS method applied to TI coincides formally with the SS method (with m=1) applied to TI^2, thus we could still focus on just two formulations, SS and NS, for the superposition.

    We are very grateful for your comment.

    L. 276: "In addition, by using TI^2, the LSS method by IQ2021 (Eq. 21) reduces to a sub-case of the simple summation method, described shortly in Proposition 3, with $m = 1$. "

12. *U_∞ for the LES benchmark cases is not explicitly provided, yet several parts of the analysis rely on absolute values that depend on this quantity. The absence of a precise values of hampers the reader's ability to interpret and reproduce the results. I recommend that the authors explicitly specify the upstream wind speed used for each LES case and perhaps only presenting non-dimensionalized quantities.*

    In the revised manuscript, Table 1 provides U_∞ and other important parameters for each LES dataset.

13. *Even if the proposed framework does not perform well in predicting turbulence properties within wind farms in an absolute sense (which appears to be the case for the current formulation), it is important to articulate what can still be learned from the study (the authors have briefly discussed about the x-positions where the peak values of TKE appear). I fully recognize that accurately predicting both velocity deficit and turbulence simultaneously is extremely challenging for engineering models. However, I find that the manuscript lacks a meaningful discussion of the broader insights, implications, or limitations*

that emerge from the analysis. I encourage the authors to reflect more explicitly on what the community can take away from the work, even if the turbulence predictions remain only of limited accuracy.

We would not say that the framework performs poorly. Perhaps some superposition methods or the analytical model for TKE did not perform well (in certain aspects). The framework only provides a consistent definition of terms and collapses all superposition methods into two, no matter what the property is. Our discovery that, at the end of the day, the vast majority of published superposition methods can be reduced to two, no matter what the wake property is, is definitely valuable in our opinion.

14. *Lastly, if the objective of the current work is to establish a framework based on engineering or analytical models for predicting the flow field within wind farms, it would be greatly appreciated if the authors could share the code used in their implementation. Doing so would greatly improve transparency and reproducibility, and would strengthen the practical relevance of the current work for the community.*

    We did, thank you.

There are additional points highlighted in the attached PDF files that I encountered while reading the manuscript. Several of these are also important from my perspective, and I strongly encourage the authors to address them as part of a comprehensive revision. These comments are not exhaustive, and I believe it is reasonable to expect the authors to identify and resolve further issues beyond those explicitly noted. Please also note that some highlighted passages in the PDF are simply anchors for my own reference and should not be interpreted as specific concerns.

Overall, I can only reconsider on recommending this manuscript for publication after a substantial and thorough revision.

Comments from the PDF

1. *Title: I think the title should be more concise/specific, as the field of engineering/analytical wind turbine wake model has already been studied extensively. This title is somehow equivalent to "A framework for the LES simulation of wind turbines wakes", which is really over claiming the scope of study and over exaggerating the position of this research in the scope of wind energy. I consider that a title that reflects the specific contribution/focus of this work would be better.*

   We agree that it was not clear in the previous version that our paper is about how to perform wake superposition using **analytical** wake models. Without a proper framework for definitions and methods, this task is difficult and the findings arbitrary and non-repeatable. We revised the title to make it specific to analytical wake models, as opposed to wake properties: "A framework for the superposition of analytical wind turbine wake models"

2. *Abstract: It is unclear for me what I will learn through reading this abstract. Apart from the general information of wind farm aerodynamic and the ambition of standardize the wake super-position method, a mysterious parameter "m" is brought out, which I have completely no idea what it stand for before reading the context. Therefore, I recommend to rewrite the abstract to better represent the current work.*

   We have revised the abstract completely and hopefully it will address this Reviewer's concerns now. In particular, the primary objective of this work is not to tune or evaluate new wake models, but rather to propose and formalize a coherent framework for wake superposition that can be applied consistently across existing analytical wake models. We still mention the power exponent m because it is a powerful tuning parameter, but we explain better what it is, without getting too much into the details because in the abstract no equations can be used:

   L. 19: "with the value of an exponent m controlling the linearity $(m = 1)$ or non-linearity $(m \neq 1)$ of the summed terms."

3. *P. 2, L. 26: I am sure that farm-farm wake interaction is not the focus of this work, but how the text written here make me think it is.*

The interaction between wind farms is a wake superposition problem, except the upstream wind turbines belong to another wind farm.

4. *wind farm efficiency*

   Done. (see L. 46).

5. *"under condition with relatively low turbulence level". The original wording could only be understand by expert working with ABL.*

   Done.

   L. 46 - 49: "Moreover, strong wakes can persist and extend tens of kilometers downstream of the wind farms in offshore environments (Bodini et al., 2019; Platis et al., 2018; Golbazi et al., 2022), where turbulence levels are generally lower than onshore due to lower surface roughness."

6. *To my knowledge, they also use turbulence length scale to for the spectrum. Additionally, they have also stated some number for the ratios between σ_U to σ_V and σ_W.*

   We replaced "only" with "dominant" as follows:

   L. 50: "Turbulence intensity (TI) has been the dominant variable for turbulence characterization in wind farms in the International Electrotechnical Commission (IEC) standard (International Electrotechnical Commission, 2019)."

   and added the turbulence length scale here:

   L. 58: "... higher-order statistics, turbulence length scales, and individual turbulence intensities along each axis, should not be neglected (Morales et al., 2012, Archer, 2025)."

7. *It is well know that TI = sqrt(2 * TKE / 3) for near isotropic turbulence. So, the question is, what problem will we face when using this equivalence in the context of this article?*

   We agree that this is the right place to introduce this conversion between TI and TKE (now Eq. 3). In the previous version, we introduced this much later, in Eq. 38. A discussion of the validity of the isotropic assumption is well beyond the scope of this paper. Here we just mention that this equivalence is used when needed:

   L.72 - 74: "Since natural turbulence is hardly ever isotropic, this equivalence should be used sparingly, only in cases when no other information about turbulence characteristics is available. In the context of this paper, we will use it only to obtain the terms listed in Table A2, discussed later. "

8. *P. 4, L. 102: These terms are finally clear to me when I check the work the authors have cited and table 1. Please make the expression more clear.*

   We added Table A1 to better explain all these terms and modified the text as follows:

   L. 129 - 131: "where $\sigma^2_{U,T}$ is the variance induced by the wind turbine, d, e, f, k_1, k_2, and σ are functions of C_T and TI_∞ of the form $aC_T^b TI_\infty^c$ (see details in Table A1), and the $\delta(z)$ term, introduced to reduce the peak of TI at the lower rotor tip, was later slightly modified ..."

9. *P. 4, L. 106: It is unclear to me why subtracting the added turbulence with delta(z) unrealistic. On the opposite, I find it reasonable, as the presence of ground inhibit the turbulence, as this is the purposed of the term if I understand the context of Qian and Ishihara correctly.*

   The $\delta(z)$ term was originally introduced to control the excessive TI values generated near the lower tip in the near-wake region, which is a good approach. However, since the analytical formulation does not include a dependency on x to limit $\delta(z)$ downstream, it can lead to negative TI values in the far wake, which is unrealistic.

   We decided that it is too early to discuss this issue and therefore we moved this discussion to the Results section around L. 444. This is also shown in Fig. A1 in the revised manuscript. The text was modified as follows:

   L. 444 - 448: "A notable feature is the formation of a bogus peak near the rotor bottom, especially evident after turbine 7 (Fig. 11). This nonphysical peak is the result of the $\delta(z)$ term in Eq. 10, which is always positive for $z < H$ and is not a function of x. Therefore, given the minus sign in front of it, it will always subtract a fixed amount of TI from the region below H for all streamwise distances (Fig. A1a). Since the property of interest is TI^2, this negative term causes a bogus positive contribution to TI in the far wake, as shown in Fig. A1b."

10. *P. 5 L. 124: Why prefer this model over the other existing model?*

    The KA2025 is actually the only analytical model available in the literature for added TKE. We clarified this in the manuscript as follows:

    L. 153 - 155: "Since the KA2025 model is the only analytical model for added TKE in the literature and it in good agreement with LES and wind tunnel data under different atmospheric stabilities and turbine characteristics, it was selected as the benchmark for added TKE in this study. "

11. *P. 5, L. 126: A table summarizing the pros and cons (or characteristics) in the authors opinion about the super position methods reviewed could help a lot.*

    We cannot discuss pros and cons based just on the literature review, we do it in the Results and Conclusions.

12. *P. 6, L. 151: Will $U_{in,i}$ has a dimension other than m/s? I am confused by what does "dimension" here mean.*

    This sentence was removed.

13. *P. 6, L. 158: First of all, it is not clear why α_{ij} is brought out in the current context, I think explaining its purpose is needed. Next, in my opinion, α_{ij} is cleverly engineered by Qian and Ishihara (2021) and their reasoning is convincing. Therefore, I wonder if it is too assertive saying it is "only suitable" for their model.*

    We agree that a discussion of the α_{ij} term is not really needed here and therefore we removed it. The text was shortened and modified as follows:

    L. 210 - 212: However, when they used the QI2021 model for the ΔTI_i terms (Eqs. 8 and 10), they found that the LSS method in Eq. 20 was "not availing", to the point that they needed to develop an ad-hoc empirical correction term to the QI2021 model that, for an in-line and a partially-offset configuration, delivered accurate predictions when compared against one LES run.

14. *P. 6, L. 165: I disagree with the authors' explanation. Here, TI^2 is directly related with TKE (when assuming isotropic turbulence), thus the equation should be interpret as $TKE = TKE_\infty + \Delta TKE$, which is physically sound. Also, TI cannot be added, as super positioning a flow field of 2% TI with another flow field of 4% would never get 6% of TI, according to Cauchy in-equality. On the other side, I find this expression is actually physical invalid for TKE since this will involve TKE^2.*

    Thank you very much for your thoughtful comment, which we also addressed in #11 above. We removed this paragraph and rewrote all text related to TI (now TI^2), such as equations, figures, and result discussions.

15. *P. 9, L. 229: I believe that adding a table summarizing the building blocks used will help a lot. For example, a column with properties to predict (e.g. U deficit), the equation used to calculate Δ property (eq 12 or 13), the equation used to predict (eq 3), the superposition used (SS or NS). The table could help the reader quickly figure out and overview the framework the authors are using without the need to keep track all the labels, symbols, and acronym come up in the manuscript.*

    We agree that such a table would help a lot and we added Table A3 in the appendix.

16. *P. 9, L. 231: This model suddenly appeared without any context.*

    We moved this section to the Introduction (p. 5), where all the other wake models were introduced.

17. *I am not sure if "unknown parameters" is the correct term to use. Should they be called "model constants"?*

    We renamed them "fitting" parameters.

18. *Are you indicating that TI_∞ in the denominator should be replaced with something else?*

    Yes. We rewrote this as Proposition 4. It is easy to follow now.

    L. 349 - 357: "To understand why, let us consider the NS method for added TI^2 with a constant value for $\mathcal{P}_{in} = TI^2_{in}(H)$ at the denominator in Eq. 34 and focus on a level z_2 near the ground, where turbulence is generally high and therefore $TI^2_{in}(z_2) > TI^2_{in}(H)$ at any x, y. As can be appreciated in Fig. 2b, the superposition with a constant value $TI^2_{in}(H)$ used to normalize the added TI^2 causes an underestimate for $z > H$ and a slight overestimate for $z < H$. The only vertical level for which a constant value $TI^2_{in}(H)$ would lead to the correct value of TI^2 is $z = H$; for all other levels it would lead to inaccurate results. The same issue is also found for TKE in Fig. 2a."

19. *P. 12, L. 318: Is it function of i or function of x? If N is really a function of i then I do think dropping i is appropriate.*

    It is a function of x.

    L. 318 - 320: "N is technically a function of x, but, for consistency with the literature, we are omitting this explicit x dependency. N should also be limited to the upstream turbines whose wakes can actually reach x; here we recommend those within $20D$ upstream. "

20. *P. 12, L. 331: Does the authors mean C_T is also needed to calculated TI, TKE, and ΔU?*

    Yes it does. All analytical models used in this study need C_T as an input.

    L. 380 - 382: " ... the C_T value (Fig. 3), which is a necessary parameter to calculate U with Eq. 4, TI with Eq. 8 and TKE with Eq. 11 ..."

21. *P. 13, L. 345: In the results, the authors reports absolute values with physical units. However, the absolute value of the set inflow velocity is not clearly provided (at least I did not find it. Speaking to this, this is another example that make me to recommend the authors to improve the writing of the manuscript). This is especially problematic when comparing TI with TKE, since value of U_∞ to use cannot be precisely quantified.*

    We added Table 1 tabulating all important parameters from LES datasets used in this study.

22. *P. 12, L. 332: Which equation is used here? 12 or 13?*

    We added Table A3 to show the final equations used for each property.

23. *P. 15, L. 371: Is the superposition of the model designed to be normalizing with U_∞ or U_{in}?*

    All normalizations should use $\mathcal{P}_{in}$. The XA2015 values are normalized by design already and therefore we had to de-normalize them by multiplying by $U_{in}(z)$ for the SS method (not for the NS one).

24. *P. 15, L. 373: I think the authors should provide more physical/mathematical explanations. For me, by doing some hand calculation, it is very reasonable for NS with m = 0.8 yield negative velocity. Also, as can be seen, with m = 2.0, the results of U field seems to be reasonable.*

    We agree that it is not surprising. We added this:

    L. 410: "... although it performs well for $m = 2$."

25. *Fig. 1: Please make the ticks of the color ticks more properly*

    Regarding this and all other comments relating to figures: we updated all the mentioned figures in the revised manuscript.

26. *P. 17, L. 408: I can only understand the meaning of this sentence after substantial thinking. The authors are encouraged to improve the readability here.*

    We deleted the profiles of TKE as a function of y at $z = H$ and $x = 5D$ from the KA2025 model because we discussed the same feature in Fig. 13.

27. *P. 17, L. 416: does the authors mean at a given x position? At each gird point has an entire different meaning compared to the one just mentioned.*

    Yes, we made an error, we meant at a given x position. We rewrote the sentence as follows:

    L. 433 - 435: "To assess the evolution of the wake TI downstream, it is common practice in the literature (Delvaux et al., 2024; Risco et al.,2023) to calculate its average inside the rotor area at each x point, centered at hub height, downstream of each wind turbine (called a "rotor-integrated" TI)."

28. *P. 17, L. 418: This is for sure, but the authors has reported as an "finding". Which I find mis-leading.*

    We changed this discussion slightly by introducing the concept of "saturation" as follows:

    L. 435 - 437: "for the first few turbines, the peaks of rotor-integrated TI from the LES increase, but then plateau at about the same magnitude after each additional turbine, a feature referred to as "saturation". "

29. *P. 20, L. 437: Does delta z = H - z? If so, please clearly stated./delta z or delta(z)?*

    Thank you for pointing out those typos, which are all now fixed or removed. The correct form is $\delta(z)$.

30. *P. 21, L. 451: At this moment, it is not clear to me why the author mention "below the rotor" here. Perhaps this is in relation in some other paragraph but I would leave this at this moment and await the authors to clarify.*

    Because of all the changes that we did, this issue no longer exists.

31. *P. 21, L. 454: In my view point, even with KA2025 SS is not even close to LES. With this large error margin, the three curves of m = [0.8, 1.0, 2.0] are considered almost the same from my perspective. However, I am also curious how would the authors claim that m = 0.8 outperform the other two m, and perhaps why the wind energy community should consider m = 0.8 over the other two values.*

    We simply meant that the error is slightly lower with $m = 0.8$ than with higher values of m (see Fig. 18). We rewrote the sentence as follows:

    L. 495 - 500: "The rotor-integrated TKE is grossly underestimated in the JHU dataset ... all superposition methods with the KA2025 model fail at reproducing such large amounts of TKE, especially above the rotors, although the match is excellent at the first turbine. "

32. *P. 24, L. 468: What about QI2021?*

    This discussion is no longer valid.

33. *P. 29, L. 507: Also for U deficit or only for TKE and TI, please clarify.*

    We no longer use the wind speed deficit as a property, just wind speed. Thus, the statement holds for all three properties, as explained in the text.

    L. 537 - 540: "In general, we found that the SS method tends to generate higher values of the wake property than the NS method. For example, the maximum value of added TI with $m = 1$ is up to 25% higher with SS than with NS for all stabilities in the WRFLES results. This is also true for U: the wind speed deficits are weaker with the SS method, thus the resulting U is higher with the SS method."

34. *P. 29, L. 517: If we view the property here as "$U_{deficit}$" and recall that the authors claim that SS is more additive, is not that SS should be more prone to negative velocity (excessive velocity deficit) than NS? According to the setup of eqs 36 and 37.*

    No, the property is actually U, not ΔU, thus the reasoning still holds.

Reviewer #2

1. *The paper investigates different wake and turbulence superposition approaches used within analytical wind farm yield simulations and compare them to mean velocity and turbulence fields from LES simulations. Whilst generally an interesting topic, the paper unfortunately does not expand on the current level of knowledge in this field. Different to the title, no "framework of wake properties models" is provided, but instead a mix between a (incomplete) review of wake and added turbulence models and simple application of empirical superposition approach is presented. The issue with analytical wake, TI models and superposition is that a clear definition of each is needed to clearly show how they all interact (which is actually mentioned by the authors). In this context it is good that the authors highlight that scaling (called normalisation in the paper) of the deficits/added TI also comes into play. However this has been discussed and laid out much more clearly by other authors before (see Bastankhah et al 2021 or Zong et al 2020 for instance, both JFM).*

   We are really sorry that we failed to explain the purpose and contribution of our paper so severely. As we mentioned at the beginning of this reply, the structure of our manuscript did not properly reflect its goal and confused the story that we were trying to convey. We made several changes to improve readability and clarity, among which: 1) we moved the description of the KA2025 TKE model, which had its own section 2.1 in the Methods in the previous version, to the Introduction, together with the other two benchmark models for wind speed and for TI; 2) we used subsections for each of the four propositions of the framework; 3) we divided the validation results by property (wind speed, TI, and TKE) rather than by LES dataset, with subsections; and 4) we removed several figures from the main text and from the Appendix that we had included in the previous version for completeness but did not really convey any new message.

   The main contribution of this study is the framework, which is nothing but a series of recommendations, based on the existing literature, on basic definitions and naming conventions to use for wake property superposition, no matter what the property actually is or which analytical model is chosen. We found that the existing literature was extremely confusing and incoherent. We also discovered that the vast majority of existing superposition methods can actually be collapsed into just two main forms: the simple and the normalized summation, with the index m as an additional tuning parameter. Our intent was not to evaluate the performance of the various methods against each other to have a "winner" because the performance of the superposition method depends on which analytical wake model one chooses and we only considered one analytical model per property. Rather, our intent was to find common themes and general behaviors that may help future users select an appropriate method for whichever property and wake model they are using.

   We added the two papers to our introduction, modified our final general equation (now Eq. 36) to include weights as in Zong et al. (2020), and added a summary table (Table 1).

   L. 181 - 193: "By considering the conservation of the total momentum deficit in the streamwise direction, Zong and Port´e-Agel (2020) introduced a weighting factor to the superposition method developed by Niayifar and Port´e-Agel (2016). They indicated that the total wake velocity deficit can be expressed as a weighted sum of the individual wake velocity deficits, where the weighting factors are defined as the ratio of the wake convection velocity, an x-dependent representation of the mean velocity in the entire cross section of the wake, to the convection velocity of the combined wake. To apply this momentum-conserving method, surface integrals of the wake velocity deficit must be calculated multiple times at each downstream location to achieve convergence through an iterative procedure, which increases the computational cost, especially when modeling large wind farms. The improvements in predicting the velocity deficit from particle imaging velocimetry measurements were not significant. Bastankhah et al. (2021) developed an innovative approach that does not rely on an explicit superposition method *a-priori*, but uses the conservation of momentum deficit to obtain directly the wind velocity field downstream of any waked wind turbine in a farm. The resulting formulation is formally equal to the linear superposition of wind speed deficits in Eq. 12 or 17. The difference is that the ΔU term is obtained neither from U_∞ nor from U_{in}, but from the difference between the resulting wind speed with and without a given waked wind turbine. "

2. *Without the presentation of a framework the paper essentially applies a wake and TI model (both previously developed by one of the authors) to different LES cases and tries to see whether they can be made to agree with them, solely by changing the superposition model. A simple tuning factor "m" is introduced to do so, however this is exactly the issue many times when discussing engineering wake modelling, as many times the inadequacy of single wake models is compensated by tweaking the superposition model. However this only works in certain conditions and does not really advance the current field of research. There have been other papers that have provided proper frameworks for tuning (see for instance van Binsbergen et al 2024, WES), this should be used as a starting point to push beyond the status quo. A simple manual superposition model tuning is not sufficient for a journal paper with regard to the existing body of research.*

   Our paper is not a tuning study. It's a framework study with propositions and recommendations. The tuning is not the goal, it's a possible application of the framework.

   The Reviewer is correct that ultimately our paper focuses on two superposition methods and three values of the tuning coefficient m. However, this is not by design or because we arbitrarily wanted to focus on just those: all the superposition methods published so far, with a few unpractical exceptions, reduce to one of these forms, no matter what the wake property is. This is important in our opinion and was not found before. In addition, as we mentioned at item #1, we proposed a naming convention and coherent definitions that are sorely missing in the literature today.

   There is nothing wrong with tweaking a superposition model, and it is even less wrong to try to compensate for the weaknesses of the wake model that way. What is the alternative? Improving the wake model alone is always a good idea, but even the perfect wake model will give different results depending on the superposition method chosen because there is **not** one superposition model or one exponent that will be best for all cases.

   As for the paper by van Binsbergen et al. (2024), it does not seem to support the point that the Reviewer is raising about tuning parameters because: 1) it is only about one wake property (the wind speed deficit) and one wake model for it, the GCH model (which we were not aware of, but is fundamentally a modified Gaussian), and 2) it only uses one wake superposition method, that by Katic at al. (1987) (which is listed now in Table 1). The hyper-parameter tuning that was described is nothing but a tweaking of the parameters to best match the specific SCADA dataset, which is precisely what this Reviewer is criticizing (but is perfectly valid in our opinion). The final set of values for the 4 tuning parameters may or may not be the best choice for other wind turbines or datasets. In addition, van Binsbergen et al. (2024) did not evaluate the performance of the superposition method.

Comments from the PDF

1. *Title: The title does not really reflect the work presented in the paper, I expected to see this "framework"*

   We revised the title to make it specific to **analytical** wake models, as opposed to wake properties: "A framework for the superposition of analytical wind turbine wake models"

2. *Abstract: The abstract reads more like an introduction and needs a stronger focus. It remains unclear what the aim of this paper is.*

   We revised the abstract to better explain our primary goal: to propose and formalize a coherent framework for wake superposition that can be applied consistently across existing analytical wake models. We are confident that you will appreciate the revised abstract much better than the previous one!

3. *P. 2, L. 39: how is this defined*

   We now explain how mean speed and standard deviation are defined based on the IEC standard:

   L. 53 - 57: "TI is the ratio of the standard deviation of the streamwise component of the wind (σ_u, also referred to as σ_1 in the IEC standard) to the mean wind speed ($\bar{U}$):

$$TI = \frac{\sigma_u}{\bar{U}}, \tag{2}$$

where $U = \sqrt{u^2 + v^2 + w^2}$ and u, v, and w are the wind velocity components along the x, y, and z axes, aligned with the streamwise (or longitudinal), spanwise (or lateral), and vertical (or upward) directions."

4. *P. 2, L. 41:in terms of standards, but within research spectra are usually and commonly used as TI is of course too strong a simplification*

   We agree, but all analytical models for turbulence (whether TI or TKE) depend on TI_∞.

   L. 57 - 59: "However, there is much more information in the wake region than what can be captured with TI, and higher-order statistics, turbulence length scales, and individual turbulence intensities along each axis should not be neglected (Morales et al., 2020; Archer, 2025)."

5. *P. 2, L. 45: how are these defined*

   We defined the coordinate system at line 56:

   L. 56: "the x, y, and z axes, aligned with the streamwise (or longitudinal), spanwise (or lateral), and vertical (or upward) directions."

6. *P. 2, L. 59: I would remove this statement as afterwards you are stating that there are still open issues with the parametrization. It is available but maybe not actually yet "appropiate"*

   We modified the text as follows:

   L. 81: "is an effective tool to study the wind farm interaction with ..."

7. *P. 3, L. 69: I am confused as to why this section is in here? Is the paper not focusing on analytical wake models? How do meso-scale models relate to the work presented within the paper that they warrant extra mention?*

   The very reason why we are focusing on analytical wake models is that advanced wind farm parameterizations for mesoscale models require wake models in order to account for sub-grid scale wake effects. However, analytical wake models and superposition methods need to be thoroughly studied before proposing a WFP. A recent study by Ma et al. (2022a) proposed a WFP that calculates velocity deficits in mesoscale models by coupling different analytical wind speed deficit models and superposition methods within WRF. However, to the best of our knowledge, no similar study has incorporated turbulence quantities into a WFP. We made this clearer in the Abstract and in the Introduction as follows:

   L. 89 - 94: "To better capture the wind speed deficit caused by the sub-grid wakes, several alternative WFPs have been recently added in the WRF model (Ma et al., 2022b, a), referred to as the MAV wind farm parameterizations from the authors' initials, based on analytical wind speed deficit models, discussed in the next section. However, to the best of our knowledge, no study has incorporated analytical models of wake turbulence into a WFP. It should also be noted that analytical wake models and superposition techniques require comprehensive examination before they can be incorporated into a WFP."

   L. 7: "... the process of inserting analytical wake models into mesoscale models to better represent the sub-grid-scale impacts of wind farms on the atmospheric boundary layer is hindered."

8. *P. 3, L. 72: Still the go to in yield assessment as well, as they cheap and relatively accurate regarding power prediction*

   Done!

   L. 96: "Analytical models are a preferred choice for optimizing wind farm layout over flat terrain, as they are relatively accurate in power prediction and computationally more efficient than numerical simulation tools (Porté-Agel et al., 2020)."

9. *P. 3, L. 75: not really, the Gaussian model is nowadays the go to*

   Done!

   L. 99: "The first is the Jensen model (Jensen, 1983), which ..."

10. *P. 3, L. 77: not really a deficit model, but a superposition model*

    We removed the citation Katic et al. (1986).

11. *P.3, L.78: subjective, I would say "modified" or similar that is more neutral. It is still a Gaussian model and relies on the exact same assumptions as the original one by Bastankhah. It is still a flavour of Gaussian wake model and Bastankhah could have easily added this dependancy. For readability I would just say Gaussian in the rest of the paper or just wake model as you are not using any other wake models. That's also why it seems weird to introduce all the other wake models. There are plenty of review papers on this and does not have to be repeated.*

    We need one benchmark model for each property, thus we picked XA2015 because it is indeed slightly better than the Bastankhah's model. We agree that Bastankhah "could have easily added this dependency", but the fact remains that they did not. We agree that a full literature review on wake models is not the purpose of our study, but it is still important to introduce at least the most famous models. We replaced "improved" with "refined" as follows:

    L. 101: "Bastankhah and Porté-Agel (2014) proposed the first Gaussian model of the (normalized) wind speed deficit, which was later refined by Xie and Archer (2015), "XA2015" hereafter, by introducing two different expansion rates along y and z (k_y and k_z)..."

12. *P.4, L. 89: There is also a very recent model from Blondel. I am not sure why a non-exhaustive descritption of added turbulence models is presented*

    We are focusing on **analytical** wake models and have provided an exhaustive literature review of them. The Blondel model appears only in a preprint (https://arxiv.org/abs/2508.20012), but we have not even been able to find its associated publication. Regardless, it is based on the Lattice Boltzmann solver and therefore it cannot be described as analytical and cannot be inserted in a numerical weather prediction model as a wind farm parameterization.

    There are some turbulence wake models derived by solving the partial differential equations governing TKE transport (Bastankhah et al., 2024; Klemmer and Howland, 2024). These proposed models need be solved or integrated numerically and rely on more input parameters than KA2025 and QI2021, which makes them difficult to implement when developing a WFP for mesoscale models. Therefore, we do not consider them in this study.

13. *P.5, L. 125: This part needs to be rewritten as it is quite convoluted and confusing. Have a look at Bastankhah JFM 2021 or Zong et al 2020 also in JFM, that provide a very clear and straight forward distinction between the superposition and wind speed definitions.*

    This section was rewritten and Table 1 was added to provide a clear summary of all the equations used in the literature, as well as their equivalent within the framework. We added the two references that you suggested and included them in the discussion of the superposition methods for wind speed deficit. Also, we modified the unified superposition formulation (Eq. 36) to include weights w_i, since the Zong et al. (2020) paper used them. Note that the final superposition formulation in Proposition 3 is not just for wind speed, but also TI and TKE, thus a method, like those of Bastankah JFM 2021, which only works for wind speed deficit and does not actually perform any superposition, are not included in the framework because of their limited applicability.

    We modified the text as follows:

    L. 181 - 193: "By considering the conservation of the total momentum deficit in the streamwise direction, Zong and Port´e-Agel (2020) introduced a weighting factor to the superposition method developed by Niayifar and Port´e-Agel (2016). They indicated that the total wake velocity deficit can be expressed as a weighted sum of the individual wake velocity deficits, where the weighting factors are defined as the ratio of the wake convection velocity, an x-dependent representation of the mean velocity in the entire cross section of the wake, to the convection velocity of the combined wake. To apply this momentum-conserving method, surface integrals of the wake velocity deficit must be calculated multiple times at each downstream location to achieve convergence through an iterative procedure, which increases the computational cost, especially when modeling large wind farms. The improvements in predicting the velocity deficit from particle imaging velocimetry measurements were not significant. Bastankhah et al. (2021) developed an innovative approach that does not rely on an explicit superposition method *a-priori*, but uses the conservation of momentum deficit to obtain directly the wind velocity field downstream of any waked wind turbine in a farm. The resulting formulation is formally equal to the linear superposition of wind speed deficits in Eq. 12 or 17. The difference is that the ΔU term is obtained neither from U_∞ nor from U_{in}, but from the difference between the resulting wind speed with and without a given waked wind turbine. "

14. *P.7, L. 184: What is the reasoning behind the exponential approach? How do you ensure it is not just another tuning parameter?*

    It was proposed by somebody else without explanations. It is a mathematical extension. It is indeed a tuning parameter.

15. *P.9, L.231: So are we now mixing superposition and added TI method or how do we distinguish clearly between the different ways of normalisation.*

    The original analytical wake model proposed by KA2025 for TKE was normalized by wind speed at hub height. We need to de-normalized it first before employing it in the SS superposition method. In the revised paper, we added Table A3 to show the final form of all the wake models used, including the form used to de-normalized this TKE equation (if needed).

16. *P.10, L.255: Why was this model chosen and if so why were all the other methods reviewed as well? Could one not just retune the Crespo model as many others have done in the past?*

    While the Crespo model calculates TI, the KA2025 is the first analytical model for TKE and it is able to calculate TKE in three dimensions. However, one can chose any other analytical model.

    L. 153:" Since the KA2025 model is the only analytical model for added TKE in the literature and it in good agreement with LES and wind tunnel data under different atmospheric stabilities and turbine characteristics, it was selected as the benchmark for added TKE in this study. "

17. *P. 10, L. 258: I do not see this in the subsequent description and discussion.*

    This is now addressed explicitly in (new) Proposition 1, Section 2.1.

18. *P.11, L. 274: Katic and Rathmann (Wasp) already discussed this a long time ago*

    We could not find these references and none of the publications that used this distinction mentioned them either.

19. *P. 11, L. 289: ? I do not understand why this is needed in analytical wake modelling. There is no self-induction there so you can sample at the turbine location, so why not do so. In LES that is of course not possible, so maybe that's why it is done here? There is also work by Mann et al., Graham that looked into how turbulence changes within the induction zone. But if you working with mean flows, one can assumed the turbulence to be simply avected by the mean flow.*

    We removed this proposition entirely from the framework. What we meant above was that a standard location (-1D) should be selected for the LES results, not for the wake models.

20. *P. 11, L.286: How is this discussion relevant?*

    In revised paper, this part was removed.

21. *P. 12, L.308: This needs to be discussed earlier as you already have introduced you wake and added TI models.*

    We did discuss this earlier in the literature review, see old Eqs. 22 and 23. We are calling it differently here because we are introducing it for the framework.

22. *P.12, L. 312: There are more advanced superposition models available already, why not work with those?*

    As discussed already at item #12, there are some advanced implicit superposition methods, like Howland's, and iterative methods, like Bastankhah's, but they can't be inserted in a wind farm parameterization, at least not in a straightforward way, because the numerical equations solved by the mesoscale model are not the same as those utilized in the derivation of the implicit (or iterative) method and therefore some of the terms cannot be calculated. Our scope here is superposition methods that can be used in wind farm parameterizations, thus they must be analytical and not require excessive iterations.

We have now explicitly added the adjective "analytical" in the title and made this connection between mesoscale models and analytical models clearer throughout the paper, starting with the abstract:

L. 7: "As such, the process of inserting analytical wake models into mesoscale models to better represent the sub-grid-scale impacts of wind farms on the atmospheric boundary layer is hindered. To address this issue, here a framework is proposed with the goal of standardizing wake superposition for use with any analytical model of wake properties."

23. *P.12, L. 320: Isn't the normalization a form of scaling? And is is not missing here? I am also wondering why you not looking into the max. square or similar that has been successfully used for TI superposition?*

    The term "normalization" refers to the fact that the added property is made unit-less via a division by a selected value, as opposed to the "simple" summation in which there is no such normalization.

    The max operator is not truly a superposition method because it specifically eliminates any superposition by design by only considering the effect on one upstream turbine. It also has been found to work well only for spacings less than 5D (e.g., Delvaux et al. 2024), which is very tight and not common. According to Li et al. (2023), the max method was found to underestimate turbulence intensity and "the importance of the adjacent turbine is greatly emphasized in the superposition, but the effects of all the other turbines are ignored. Therefore, this method cannot quantitatively describe the interference and superposition of the streamwise turbulence intensity in wind farms." Lastly, the max method violates Proposition 1, by which only TI^2 (or ΔTI^2) is additive, not ΔTI. As such, we did not consider it in the framework.

24. *P. 13, L.344: I think this flowchart is not needed as it is obvious from your sum operator.*

    We removed it.

25. *P. 14, L. 358: Why not use a below-rated case where we expect the largest wake effects? That's when wake and TI models get challenged. And what about the SOWFA case? At what wind speed were those?*

    The high geostrophic wind was just in the initial conditions and at the top of the ABL; at hub height the wind speed was actually below rated, as shown now in Table 2, together with the details of the SOWFA and other datasets.

26. *P. 15, L. 368: Difficult to follow with all the abbreviations*

    We have restructured the paper and this discussion was shortened and there are now only the three abbreviations for the three benchmark analytical wake models.

27. *P. 15, L. 375: please make this explicit. What do you mean here? I also do not understand why wind and ti superposition need to follow the same approach. What is you reasoning for that*

    We selected one benchmark analytical model for each wake property because we could not possibly test them all. For the case of wind speed, the analytical model is XA2015; for TI, it is QI2021, and for TKE it is KA2025. We did not make it clear in the previous version, but hopefully this is clearer in the revised manuscript. For example, we state it in the abstract:

    L. 20: "The performance of the superposition methods within the framework is then assessed for three benchmark analytical wake models (one for each of three wake properties: U, TI2, and TKE)"

    in Section 3:

    L. 371 - 373: "Three benchmark analytical models for: wind speed deficit by XA2015 (Eq. 4), added TI by QI2020 (Eq. 8), and added TKE by KA2025 (Eq. 11) are used in this section with the proposed framework, thus in Eqs. 38 - 39 the property $\mathcal{P}$ is equal to U, TI^2, and TKE, respectively."

    and in the Conclusions:

L. 531: "We then used this framework with three existing analytical wake models as benchmarks – XA2015 for the wind speed deficit, QI2021 for added TI, and KA2025 for added TKE "