Reply to the reviewers of "An analytical formulation for turbulent kinetic energy added by wind turbines based on large-eddy simulation"

22 January 2025

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

Reviewer #2

While the authors rightly highlight the need for improved TKE models—since most existing engineering models focus on velocity deficit prediction—the model's reliance on 13 tuning parameters may hinder its broader applicability for diverse case studies. The novelty of this work could be enhanced either by incorporating more advanced ML data-driven models or perhaps adopting a more rigorous physics-based approach. While the authors' efforts in accurate predictions of TKE are appreciated, the current formulation appears more like a straightforward curve fitting to existing datasets rather than a highly novel contribution.

We agree that the proposed formulation, at the end of the day, is more or less a curve fitting to existing datasets. However, there are two arguments that we would like to offer. The first is that the novelty of this manuscript is not much in the format of the final result (a 13-parameter equation) or how we got there (curve fitting to existing LES data), but rather in having this formulation at all. An analytical model for added TKE by wind turbines simply does not exist in the literature today. One could use the models by IQ2018 or Tian2022 via conversion between TI and TKE, but their models – which also are curve-fitting models – just do not perform well enough. Thus we believe that publishing this formulation in the literature is a necessary first step to then possibly improve it, for example by using more advanced ML methods, as the reviewer suggested. The second argument is that we are not claiming – and are not required by the journal – to give a "highly novel contribution". We are satisfied by giving a valid and useful contribution to the scientific and engineering communities.

- *Title: The use of "analytical" in the title may be misleading. While "analytical" is sometimes used in the literature to broadly describe engineering models, it typically refers to models derived directly from flow governing equations. Given the empirical nature of the developed model, I recommend replacing "analytical" with "engineering" or "empirical" in the title and throughout the text where applicable to avoid confusion.*

  In the wind energy community, the adjective "analytical" is most often used to characterize wake loss models, such as Jensen's, Larsen's, Fradsen's, etc (Frandsen et al., 2006; Bastankhah and Porté-Agel, 2014; Archer et al., 2014, 2018). These models consist of an analytical equation for the wind speed deficit with one or more tuning parameters (e.g., k_w in Jensen's). For example, the Gaussian model for the wind speed deficit, used by Bastankhah and Porté-Agel (2014) among others, is not an analytical solution to any flow governing equations and yet it is called an analytical wake model. Here we do the same, but for added TKE. Thus we prefer to align with the wind energy community tradition and retain "analytical" in the title. In addition, using "engineering" may confuse the readers into thinking that our model is for engineering applications only, which it is not, while "empirical" would suggest that it is based on observations alone, while our model is based also on advanced analytical derivations.

- *Introduction: The discussion on available engineering velocity-deficit models and WRF modelling, while detailed, is not central to this work. These sections could be shortened and presented more concisely to maintain focus.*

  The discussion was tightened and the two paragraphs were shortened into one, giving a reduction of the number of lines of text from 21 to 13.

- *Please clarify how the added TKE and added TI are computed in this work. Especially, this is not trivial when σ_u in the wake is less than the one in the incoming flow and thus the added TI becomes negative.*

  We added the following:

  In particular, the relationship used in this study between added TI (ΔTI) and added TKE (ΔTKE) is:

  $$\Delta TI = \sqrt{\frac{2}{3}}\,\frac{\Delta TKE}{\overline{U}} = \sqrt{\frac{2}{3}}\,\frac{TKE - TKE_\infty}{\overline{U}},$$

  where TKE_∞ is, broadly speaking, the free-stream turbulent kinetic energy. The exact definition of TKE_∞ depends on the type and distribution of the available data. If three-dimensional simulation data are available from a run without turbines (i.e., a precursor run) and a run with turbines, then the point-by-point difference of the time-averaged TKE of the two runs is used to calculate ΔTKE, e.g., for the validation LES datasets described in Section 2.2. If only a simulation with turbines is available, as is the case for the validation LES datasets described in Section 3.2, then the vertical profile of TKE at an upstream distance of $x = x_0 - 2D$ is obtained by calculating at each level the average of TKE over $-3D \leq y - y_0 \leq +3D$, where x_0, y_0 are the coordinates of the turbine. The value of TKE_∞ to use at each point downstream is, then, the value of TKE in the upstream vertical profile at the same vertical level.

- *Please improve the quality of all figures, especially figure 2 where Greek symbols are written in Latin letters. This should be avoided and the authors are expected to use proper typesetting to generate figures.*

  We apologize for the sloppy figures. We made a considerable effort to improve the quality of all the figures and made the following modifications:

  - Figure 1: the coordinates were changed to lower case (x and z).
  - Figure 2: the titles of the y-axes were changed to Greek letters, where needed, and the subscripts were added; the title of the x-axis was changed from "CT" to "C_T"; in the legend, "ti" was replaced with "TI" and a comma was added.
  - Figures 7–10 (now 5–9): replaced "Tian2022" with "TIAN2022", consistent with the rest of the manuscript.
  - Figure 9 (now 5c): the coordinates were changed to lower case (x, y, and z) and the legend item was changed from "Aju2020" to "AJU2020", consistent with the rest of the manuscript.
  - Figure 10 (now 5a): the line across the value 2.0 on the y-axis was removed.
  - Figure 8 (now 6): replaced "Archer2020" with "ARC2020", consistent with the rest of the manuscript.

- *Line 65: The phrase "... flow has less energy" could be misleading, as the far wake typically exhibits more kinetic energy than the near wake due to wake recovery.*

  The sentence was removed.

- *Line 100: The statement, "Notably, Wu et al. (2023) conducted LES that included the effect of atmospheric stability to show that the wind speed deficit behaves differently from the ΔTKE and that the two are not co-located in the wake region," is somewhat vague. Could you clarify what is meant by "wind speed deficit behaves differently from the ΔTKE"? For instance, does this refer to differences in spatial distribution, magnitude, or temporal evolution? A more precise paraphrasing would enhance clarity.*

The sentence was modified as follows:

"the wind speed deficit behaves differently from ΔTKE (e.g., the wind speed deficit reaches the ground within 8D while added TKE remains aloft) and that the two are not co-located in the wake region (e.g., the wind speed deficit peaks at hub height while added TKE near the rotor tip)."

- *Line 125: The statement, "the WRF will add some TKE on its own due to the resolved vertical shear," is unclear. If this detail is not crucial to the discussion or the importance of accurate TKE prediction, consider removing it. Alternatively, if it is essential, please clarify how WRF contributes to TKE through resolved vertical shear and its relevance to the context.*

  This detail is important because it explains why it is crucial not to overestimate TKE in the parameterization. We discussed this issue in detail in the two papers by Ma et al. (2022a,b) cited in the text. The first sentence was expanded to clarify the concept as follows:

  "will add some TKE on its own via the production term in TKE equation, due to the weak, additional, resolved vertical shear caused by the reduced wind speed in the grid cell of the turbines."

- *Line 133: Space is missing in "u,v, andw" . Also "and" should not be written in math mode.*

  Done.

- *Line 139: The phrase " ... is the mean wind speed" should likely be "the incoming wind speed".*

  At this point of the discussion, the definition is general, not just related to wind energy applications, thus we really intend to say "mean" and use the overbar on the wind speed symbol ($\bar{U}$). There is no turbine yet into which the flow would be incoming or outgoing.

- *This recently-published paper (Modelling turbulence in axisymmetric wakes: an application to wind turbine wakes, 2024) could be relevant to the discussion provided in this paper, so the authors may want to include it in their literature review.*

  Thank you for suggesting this paper, which we were not aware of and is very relevant. If it had been published earlier, before we submitted our paper, we would have discussed it in detail and added a comparison of the performance of their new analytical model against ours, IQ2018, and TIAN2022. As a compromise, we added a citation to it in reference to the finding that IQ2018 overestimates TKE as follows around line 365:

  "Large overestimates by the IQ2018 model have also been reported recently by Bastankhah et al. (2024) in the near- and far-wake regions."

- *Line 141: "Typically the largest one is σ_u, followed by σ_v (approximately $0.75\sigma_u$ in neutral conditions) and then by σ_w (approximately $0.52\sigma_u$ in neutral conditions) (Arya, 2001)." This needs to be moved to the next paragraph after discussing that x is aligned with streamwise direction in this study. Otherwise, this statement is incorrect based on west-east definition for coordinates mentioned initially.*

  The statement is correct and belongs here. In the real atmosphere, on average, the east-west variance is the largest and the other two variances are smaller, as reported. The reviewer is welcome to check the Arya book to confirm that the real atmosphere is not exactly isotropic. We also added a citation to Stull (2017), see his Chapter 18.

- *Line 271: "The implication is that the magnitude of added TKE in the wake of a wind turbine is essentially independent of atmospheric properties (such as turbulence intensity or stability)" . Is that consistent with previous works? Normally, it is expected to observe a negative correlation between the added TKE and the ambient TKE as suggested in Crespo's work and also observed in some numerical and experimental studies.*

- *Figure 2: For some cases such as WRFLES-S or WRFLES-N, there are only one dataset shown in figures. Fitting a line to only one data point sounds tricky. Can you please clarify this?*

The figure just shows the shape of the functional relationship curves for all the cases, using the C_T and TI_∞ values for each case. For WRFLES-S, for example, we picked the values C_T=0.70 and TI_∞=0.065 and plotted the resulting curves in green. The only point shown is the actual original value (sometimes we have many points, like for VPA-TI064). Ideally the point should lay on the curve, or close to it. The farther away it is, the poorer the performance of the fitting. But the actual fitting was done with, literally, millions of points.

- *In several places including but not limited to line 277, there is no space between the text and the parentheses including the citation.*

  Thank you, we fixed it.

- *Line 297: "Once again, it is physically correct that a more turbulent atmosphere causes a rising of the location of the added TKE peak." Can you please explain why is that?*

  The sentence was removed.

- *Line 303: repeated citation!*

  Fixed.

- *Figures 3-5: I'm not sure if contours shown in these figures are really necessary and add value to the paper as both vertical and lateral profiles are provided later. I suggest removing these figures for brevity.*

  We absolutely love these figures and think that they are very important. They are the reason why we chose the Weibull function for the vertical and horizontal directions and the Gaussian function for the radial one. They give a more immediate and intuitive idea of the 3D distribution of added TKE, which is not that well known from the literature, and of the effects of stability on the shape of added TKE. They also allow the readers to appreciate the differences between the first and second fits (smooth) and the real distribution (more jagged).

  But we agree that perhaps showing an example from all three stabilities means too many figures. Thus we kept Figure 3 in the main paper and moved old Figure 4 and 5 to Appendix A (now Figures A1 and A2).

- *All figures especially Figs 6-10 seem to be inconsistent with the main text in terms of font size. They are also too big. Please consider making them smaller and grouping them to make the paper more concise.*

  Figures 6–10 appear to be inconsistent because their datasets are actually inconsistent with one another, since they come from disparate sources. For example, data for x up to 14D were available from ARC2020, but only up to 8D from XA2017. Since we wanted to maximize readability, we made each figure as large as possible. We have now rescaled all these figures to be the same height and combined XA2017 and AJU202 in a single figure (Figure 5).

- *Line 393: "is associated with the reduction in vertical wind shear due to the wind speed deficit, is not reproduced with the proposed fit because it is not accounted for in its equations". The reduced TKE level at lower heights have been reported in several studies, but the current model does not capture that. Please comment on how it can be included in the empirical formulation and how important it is to be modelled.*

  At this early point, it is important for us to avoid the increase in TKE near the ground that plagues IQ2018 and TIAN2022. Reproducing the decreased (or the unchanged) TKE near the ground is a task that we will address in the future. We rephrased the text as follows:

  "We note that the slight reduction in TKE near the surface shown in the LES results, which has been observed and simulated in the literature (Archer et al., 2019; Wu and Archer, 2021), is not reproduced with the proposed fit because it is not accounted for in its equations yet. A way to account for it in the future could be via a correction similar to the δ function of IQ2018, shown here in Eq. 4."

References

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