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 #1

The paper presents a new analytical formulation for turbulent kinetic energy in the wake of a single wind turbine. The model proposes a detailed three-dimensional description of the tke field, intended to remain valid in both the near and far wake regions. In total, 15 parameters are introduced and calibrated against the results of large-eddy simulations through a two-step least-squares method. The paper is clear, easy to read, and has the potential to contribute to the improvement of the latest wake models.

Major comments:

- *Line 189, 203: For both Weibull-like laws, the shape parameters are set before starting the fitting process. The authors should motivate the choice of the values kA=2 and kW=4, and clarify why those parameters are not fitted using the two-step least-squares method.*

  We really appreciate the comment because we too have actually tried to find the fitted values for k_A and k_W. Unfortunately, we could never get convergence with a total of 7 parameters, thus we had to reduce the number of parameters and select reasonable values for k_A and k_W manually. The choice of $k_A = 2$ is motivated by the need for the fitting function $A(x)$ to be exactly zero at x_0 (only possible for $k_A > 1$) and to be rapidly increasing past x_0, but not too rapidly (which would be the case for $k_A < 2$). For k_W, by trial and error we found that a value of 4 would give a steep increase in TKE above the rotor top and a gentler decrease below it, as shown in the LES results. We added the following in the manuscript around line 190:

  We set $k_A = 2$ to reduce the overall number of parameters to fit and to obtain a function with the desired properties, i.e., equal to zero at x_0 (thus $k_A > 1$) and rapidly increasing past x_0, but not too rapidly (which would be the case for $k_A < 2$).

  and around line 205:

  ... the shape parameter k_W is set equal to 4 after a trial-and-error process to ensure a steeper decrease in ΔTKE above the top tip than below it, as shown in the LES results.

  and around line 248:

  We attempted to find fitted values also for k_A and k_W, but with 7 parameters we could never reach convergence of the least-square error fitting procedure.

- *Line 274: The authors claim that kr is independent of CT. However, Figure 2b shows differences up to 20% between the value of kr at CT=0.4 and at CT=0.9. It would perhaps be interesting to consider an expression other than CT^b for the fitting.*

We would like to clarify that the dotted lines shown in Figure 2b are not those obtained with b=0, but they are the original result of the fitting procedure, with b = -0.061. Since the lines are almost perfectly horizontal, the dependency on C_T is insignificant, which is why we wrote in the manuscript that k_r is basically independent of C_T (now we say that "the fit for k_r" is).

We agree that the original points (not the fitted lines) show a weak dependency on the thrust coefficient. However, this dependency is confusing. For the high TI cases (VPA-TI108), it is slightly decreasing with C_T, but for low TI cases (VPA-TI064) it is slightly increasing. We believe that this is the reason why the least-square error fit produced a flat line. We added the following around line ...:

By contrast, the fit for k_r (Fig. 2b) is basically independent of C_T, despite a weak and conflicting dependency in the direct-fitting values, thus b is overwritten as zero from the original value of -0.061 in Table 2.

As much as we would like to try your suggestion of using a different functional relationship for C_T, at this point it would be too massive an effort. Plus the power function has been used in the literature.

- *Figure 2: Because the form of Eq. 15 cannot capture the stability conditions, the differences between the "direct fit" values and that of the functional relationships are often large in the stable and unstable cases. For consistency, shouldn't only neutral conditions be used for calibration?*

  In general, stable conditions are characterized by lower TI than unstable conditions, thus the reasoning behind picking TI as the metric for atmospheric conditions was that it would be a decent (although not perfect) proxy for stability. In addition, we want a set of functions that could be used in all stability conditions. If we only calibrated our coefficients on neutral cases, then we would not be able to capture other stabilities at all. By including stable and unstable cases in the calibration, the proposed fitting functions are better equipped to treat non-neutral conditions. Lastly, the stable and unstable cases were clear outliers only for ε_r, as discussed in the manuscript; in the validation, the stable case (SOWFA) was the one with the lowest error (Table 3).

- *Line 305: The authors should clarify what they mean by "entire wake regions" and specify the limits of the regions along y and z as well, as this will influence the value of the RMSE.*

  Ali, write the exact limits that you used for the wake regions That's true the size of wake region influence the value of the RMSE, indeed. Thus, in this study, The entire wake region covers an area with y from -1D to +1D and z from 0-2H. Please note that we have different values along the x direction as some LES cases and the experimental test just cover a small regions in downstream

Minor comments

- *Line 6: The notation $x = 4 - 6D$ appears a bit confusing. It might be worth considering an alternative notation.*

  Changed to 4D–6D.

- *P2: The introduction is rich and well-documented. However, the relevance of the section between lines 28 and 46 is questionable in the scope of this work as it addresses velocity deficit models.*

  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.

- *Line 76: "We note that also Eq. 1 and 2 can be reduced to the same form". This is not true for CT in the case of Eq. 2. The authors should maybe re-phrase this sentence for consistency.*

  The sentence was rephrased as follows:

  We note that also Eq. 1 can be reduced to this same form and Eq. 2 to a close form (with $(1-\sqrt{1-C_T})^b$ instead of C_T^b).

- *Line 133: Typo "u, v, andw"*

  Done.

- *Line 160: Different definitions of ΔTI exist in the literature. It is worth clarifying which one is used and its connection to ΔTKE.*

  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.

- *Line 189: The expression selected for $A(x)$ is very similar to the one proposed by T Delvaux et al2024 Phys.: Conf. Ser. 2767 092089. The authors could consider providing additional reference for it.*

  Thank you for bringing this article to our attention. We added it to the list of references and cited it in the manuscript around line 192 as follows:

  The Weibull distribution was also recently proposed for the x-dependency of added TI by Delvaux et al. (2024, their Eq. 3).

- *Line 194: Missing space after the bracket.*

  Done.

- *Line 277, 279: Missing space before the bracket.*

  Done.

- *Line 351: Typo "citeWuetal2023"*

  Fixed.

- *3.2: The proposed model appears to outperform the model of Ishara and Qian (2018) in most of the validation cases. The comparison could be further enriched with the 3D model of Tian et al. (2022).*

  We added a comparison of the TIAN2022 performance in Figures 5–7 and Table 3.