

The TANDEM wake model: coupled turbulence and deficit momentum modeling in stratified atmospheric boundary layers

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In this paper, the authors propose the TANDEM wake model, in which the wake velocity deficit and the wake-added turbulence are solved together with two parabolic equations. The main new part is a mixing length closure based on the balance of production and dissipation of the wake-added turbulence, and a new near wake length model that includes the effect of wind veer. The model is calibrated with a few single turbine LES cases and then tested for single turbines and wind farms. Coupling the velocity deficit with the wake-added turbulence is very good, because turbulence has a very high impact on the velocity deficit. The focus on stable boundary layers is also useful, since these conditions happen in both onshore and offshore wind farms and can make the wake effects worse. The model works very well for single turbines in stable and veered conditions, but the improvement for wind farms is smaller and the runtime of the model is not reported. I think this is a good contribution to wake modelling, but some points need to be addressed before publication. My comments are listed below.

Comments:

1. The authors mentioned that the main applications of such models are wind farm siting and control optimization (lines 31–32), and they compared TANDEM with much faster models like the Gaussian model. However, the manuscript does not give any information about the runtime or computational cost of TANDEM. It could be better if the authors provide some runtime data and compare it with the other models studied in the paper. This is important to understand how cost efficient this model is for the applications that the authors themselves mentioned.
2. The ultimate purpose of such models is to give good results for wind farms, not only for a single turbine. The improvement of TANDEM is very clear for single turbines, but for the wind farm cases it is smaller. In greedy control all models give similar results, and in the wake steering case the k – ℓ model gives similar or even better results than TANDEM (MAE 0.048 vs 0.051 and correlation 0.84 vs 0.74 in Table 3). However, the k – ℓ model is not mentioned in the abstract. I suggest the authors discuss this more in the paper and also mention the k – ℓ results in the abstract.
3. Most of the new parameters of TANDEM are for the near wake (x_0 , α , R_d , σ_0 and ℓ_{NW}), but they are calibrated with actuator disk LES, which is usually fine for the far wake but less accurate for the near wake. Also, the actuator disk used appears to impose thrust only, without wake rotation. The interaction between wake rotation and veer may affect wake skewing and recovery, which a non-rotating disk cannot represent. The difference between $\alpha = 2.0$ in this study and $\alpha \approx 0.58$ from the wind tunnel data of a three-bladed model turbine (lines 374–376) can be related to this. It is better if the authors discuss this difference and what it means for using the x_0 model for real turbines. For future work, maybe it is better to test the model with more accurate simulations like actuator line LES.
4. C_ν and ℓ_ε are both calibrated from only one LES case, the uniform inflow with 5% TI (lines 238 and 286). It could be good if the authors provide a sensitivity analysis to show how

much the results change with these constants. Maybe in future work the model can also be developed with fewer calibration constants.

5. In Fig. 11 the error of TANDEM is very low for the stable cases. C_w is calibrated with two SBL cases with $C_r = 0.3$ and 0.5 K/h (lines 120–121 and 281), and the test cases in Table 2 use the same turbine and the same LES code, and also include $C_r = 0.3$ and 0.5 K/h. The main difference is the surface roughness (1 cm vs 10 cm). I suggest the authors discuss how much this similarity affects the very low errors in Fig. 11.
6. One symbol ℓ is used for two definitions in the manuscript: the master mixing length in line 216, and the mixing length $\ell = C_\nu \ell_{\text{md}}$ in line 244. One of them includes C_ν and the other one does not. This is confusing, and it is better to use more distinguished symbols for better clarity.
7. In the derivation of ℓ_{md} (lines 228–232), ℓ_ε is assumed to be proportional to ℓ_{md} , and C_ν is dropped from Eq. (10). Then $C_\nu = 0.35$ is obtained from the slope in Fig. 1. So this fitted value also includes the dropped proportionality constant, and it is not exactly the same C_ν as in Eq. (9). Also, later ℓ_ε is taken as a constant value of $0.78D$ (line 288), which is different from the assumption $\ell_\varepsilon \propto \ell_{\text{md}}$ in the derivation. It is better to explain these points more clearly.
8. It is better to compare TANDEM with the other parabolic models in Figs. 6 and 7. The Curl and k – ℓ models are not shown in Fig. 6, and in Fig. 7 TANDEM is only compared with the Gaussian and Vortex models, which do not have any mechanism for the effect of veer on wake recovery.
9. Fig. 7 is a bit difficult to read, because colour is used for both the veer angle and the model type. For example, TANDEM is shown in black in the legend, but its curves are veer-coloured. It would be helpful to make this clearer. Also, in line 551 the Gaussian and Vortex models are said to be orange and red, but in the legend Gaussian is red and Vortex is orange. In the caption, “inflow magnitude” may be “veer magnitude”.
10. Some small corrections:
 - Line 130: “turublent” should be “turbulent”.
 - Line 282: Eq. (14) is cited, but I think Eq. (15) is meant.
 - Line 586: “ in recovery in the recovery in TNBL” sentence could be written better.
 - Fig. 16 caption: “fora” should be “for a”.
 - The reference Heck et al. (2026) (line 531) has no journal, preprint information or DOI in the reference list. Is it under review?