Comments to the Authors

Paper title: Analytical yaw models: a two-dimensional comparison

1 General comments

This paper presents a novel set of measurements characterising the near wake of a yawed, small-scale wind turbine in a wind tunnel. By combining these measurements with existing literature data, the authors compare several analytical velocity deficit models coupled with yaw models. A model is proposed and evaluated alongside established approaches. Before comparison, all models are calibrated using non-yawed wind turbine wake data, ensuring a consistent methodology across the analysis. The proposed paper is interesting and includes a new dataset for model validation. However, this first submission suffers from limitations that will be detailed later. Also, the potential impact of this study and its innovative character are not clear to me.

2 Specific comments

- The analysis is restricted to lateral profiles, which may be misleading. Wake centers are not consistently located near hub height, contrary to the paper's assumptions. Expanding measurements to include 2D $y - z$ planes would provide a more comprehensive understanding of wake behaviour.
- The study's focus on the near wake is limiting in view of validating models. Extending measurements to cover the full wake would offer deeper insights into wake evolution and model applicability.
- The calibration process is unclear and requires significant clarification. Specifically:
  - Which parameters are calibrated, and how?
  - Do parameters vary with streamwise location, or are predefined functions assumed (e.g., a linear approach for the Gaussian width σ or another function for the super-Gaussian order n)?
  - If parameters are set for each streamwise location, how are integrations performed with the super-Gaussian model?
- Was the presented model implementation validated in their original form, before calibration?
- The presented results for the super-Gaussian model deviate from those in other studies using the same dataset (e.g., wind tunnel data from Bastankhah & Porté-Agel). This discrepancy must be addressed, possibly by validating the implementation. Additionally, reporting calibrated values and comparing to accepted model calibration (i.e., https://doi.org/10.5194/wes-8-141-2023 in the present case) would provide some insight. A sensitivity study on the calibration constant could also provide insight.
- Missing information to reproduce the results: based on the information provided in the paper, it is not possible to reproduce the test cases. At least the wind turbine thrust coefficients must be provided, together with other relevant flow and wind turbine operation variables.
- The method used to estimate the yawed thrust coefficient ($C_{T,\Psi}$ with Ψ the yaw angle) is unclear. Different approaches exist (e.g., $C_T cos(\Psi)$, $C_T cos^2(\Psi)$, $C_T cos^3(\Psi)$), and consistency across models is essential for fair comparison. A clear presentation of all models and their assumptions is needed.
- The "new" approach combines two existing models, which raises questions about its novelty and the space devoted to it in the paper. If something new is introduced here, please mention it clearly.
- While mean streamwise velocities and turbulence intensity are compared to ISO standards, further details are required:

  - Clear definition of turbulence intensity (TI)
  - Assessment of the lateral velocity component
  - Uniformity of velocity profiles in the lateral direction
  - Comparison of integral length scales to expectations
  - Provision of both lateral and vertical profiles

- Beyond the new dataset, the study's novelty is unclear. Recent advancements in modelling complex wake shapes under yaw and secondary steering are not addressed, limiting the paper's contribution to the field.

- Surprisingly, the widely used Jimenez model is omitted from the comparison. Including it would provide a more comprehensive benchmark for the proposed and existing models.

3 Technical corrections

1. L.2: The use of "2D" is ambiguous. Please clarify.

2. L.3: The claim that a "new" double-Gaussian model is proposed is misleading. The model used is that of Keane et al., coupled with the Bastankhah & Porté-Agel yaw model. Please clarify what is new in the proposed approach.

3. L.5: The assertion that measurements serve as an undebatable reference overlooks the complexity of full-scale measurements, which are subject to uncontrolled environmental biases. Are these measurements based on neutral atmospheric conditions? A more critical discussion is needed.

4. L.11: The term "more" is vague. Please reformulate for precision and provide references to support the claim.

5. L.17: While the model depends on a single tuning parameter (σ, I assume), σ itself is usually assumed to be a linear function with coefficients dependent on turbine operating and environmental conditions. This should be made explicit.

6. L.29: The mention of wind shear in the Ishihara & Qian paper is unclear. Shear is not a parameter in the models, and previous models (e.g., Bastankhah & Porté-Agel) are compatible with non-uniform vertical streamwise velocity profiles as input. Clarify the intended meaning.

7. L.37: Insert "that" between "setting" and "a wake steering" for grammatical correctness.

8. L.43: The statement "requires no tunable parameters" is misleading, as the model depends on multiple calibration constants. Furthermore, these constants could also be tuned in this study.

9. A discussion on wake deformation (curled-wake effect) and the complex wake shape behind yawed turbines should be included in the introduction.

10. L.81: please use the international system of units

11. L.84: Define turbulence intensity (TI) clearly: is it based on hub-height velocity or local velocity u(z)?

12. L.85: Both positive and negative yaw angles were considered. Did you observe any non-symmetric behavior as claimed in some studies (https://doi.org/10.5194/wes-6-1521-2021)? This should be discussed.

13. Figure 1.: Define TI and include V/U_h, Ti_v, Ti_w, and turbulent length scales for completeness.

14. Section 2.1.2: Please provide thrust coefficients for both unyawed and yawed cases. Is the current controller representative of real-scale turbines, especially regarding yaw misalignment? L.119: Did you verify the statistical convergence of the procedure? L.123 to L.126: Are these filtering operations standard? If so, provide references.

15. What is the main motivation for focusing on near-wake characteristics in these measurements?
16. Section 2.2: The calibration process is unclear. Specify which parameters are tuned, and whether they are tuned independently for each streamwise location and test case. Explain how integration is performed in the super-Gaussian model if the parameters are not continuous.
17. Section 2.2: It would be informative to compare tuned models to their standard calibration, possibly in an appendix.
18. Section 3: The derivation appears to use the double-Gaussian formulation of Keane et al. with the Bastankhah & Porté-Agel yaw model. If no new models are proposed, the extensive derivation should be justified or condensed. If new elements are introduced, they should be clearly highlighted.
19. Eq.2: Explicitly state that γ corresponds to the yaw angle. Ensure all quantities in the derivation are clearly defined.
20. L.152: σ is not a Gaussian function; please correct this phrasing.
21. Eq.6: Provide a reference to the appropriate source for this model.
22. L.158: Please clearly distinguish between non-yawed and yawed C_T throughout the paper.
23. L.166: The claim that the wake center is aligned with hub-height is invalid. Bastankhah & Porté-Agel (Figure 5) clearly shows otherwise.
24. Figure 4.: The x-axis should not be the streamwise distance, x/D, but rather $x/D + (u - u_h)/u_h$ (or similar).
25. Figure 5. he poor agreement between Ishihara & Qian's model and measurements is surprising. Was the implementation validated against the original paper's test cases?
26. L.240: the experimental data also contain a test case at 30^o. Why was this not considered in the analysis?
27. L.253: The use of a square cosine function to modify r_{min} requires justification. A simple cosine might be more appropriate for a purely trigonometric transformation.