Both reviewers underline that the model is not calibrated nor validated. The authors undertake that in this current form, the model is more a proof of concept than a fully detailed model. We however wanted to publish our results at this point because we think it is still of interest to the scientific community. There are two main reasons for this:

1. This model is based on a new theoretical approach (modelling in the MFOR and then solving convolution products), and thus has added value independently from the resulting formula of the model (Eqs. 35 and 36). This approach can be re-used by other research teams which would like to develop their own models based on the same methodology, but with other shape functions in the MFOR. For instance, deriving the model with a super-gaussian function.
   The two LES cases are only used here to choose appropriate shape functions in the MFOR, verify that our results are consistent, and show the possibilities offered by our model to give different results with same C_T and TI_x.

2. Since we plan to perform an in-depth calibration, we cannot perform it on only two cases. Thus, we need results from new simulations, with many atmospheric and operating conditions. Moreover, we would like to validate the model against in-situ data if possible, or at least wind tunnel measurements or another set of LES. Depending on the variables chosen to calibrate the model, such dataset might be hard to find, or might even demand us to ask for experimental results with our partners. In other words, we think that a proper validation/calibration of our model would be sufficient for another article, or at least a conference proceeding.

Despite this, we have some ideas for the calibration, for instance we got good results with the relation $\sigma_{fy}(x) = I_y^D x$, where I_y^D is the upstream, lateral turbulent intensity averaged over the rotor disk (and similarly for σ_{fz}). However, this is only observed on the three cases and needs to be generalised on more data before publication. For σ_y and σ_z, we expect these parameters to be a function of C_T and maybe I_x. The mixing length l_m might require more in-depth studies.

We added in section 3.4 (before the bullet points):
The model's parameters are not known a-priori: to have a usable model, it is planned to link them to the upstream flow quantities. In particular, a dependency of σ_{fy} on the lateral turbulence intensities and the integral length scale has been observed. However, this is only observed on the present cases and needs to be generalised on more data before publication. Due to the small amount of data at disposal, the present work does not aim at calibrating properly the modelled terms but simply to show that a simple shape function can already lead to a rather good approximation. Therefore, the values of the parameters are here directly deduced from the LES field:

We added in the introduction:
The presented model is not calibrated herein. Nevertheless, the added value of this work is to propose a new framework that can be used with different shape functions in the MFOR to propose other models for turbulence.
And in the conclusion:

Finally, the presented model is a proof of concept and a calibration (i.e. relating different parameters σ, σf and lm to the inflow conditions) under different atmospheric conditions is necessary before it can be used.

And in the abstract:

This model is a proof a concept that shows a methodology where one can calibrate a model in the fixed frame of reference (FFOR) with the use of shape functions chosen in the moving frame of reference (MFOR), and therefore modelling physically the added turbulence

Reviewer 1

This reviewer thanks the authors for answering the comments. However, the authors did not provide enough details regarding critical comments. For example:

(1) no clear comparison with other models is provided because their model is uncalibrated.
Please report to our answer at the beginning of the document.

(2) presenting the results without converging statistics (unstable case).
This remark has also been done by reviewer 1 of the companion paper (wes2022-46). A statistical analysis is proposed in our next submission, however, we did not think it would be pertinent to write it on both papers so it will not appear in this paper. The following text has been added in the methodology section:
Moreover, the duration of the simulation is set to 80, 40 and 10 minutes for the neutral, unstable and stable cases, respectively. An analysis of the statistical convergence of our datasets is proposed in appendix of the companion paper. Overall, it concluded that increasing the duration of simulation for the unstable case would improve the reliability of the simulations. Nevertheless the convergence of the results is assumed to be sufficient since here it is aimed to propose a proof a concept and not a fully developed model.

(3) There is no clear answer regarding the impact of the TI on the result because there had only two cases with similar TI levels.
This indeed will need to be addressed in future work when calibrating the model. Simulations with different turbulence at hub height, and possibly independently different atmospheric stabilities, should be performed.

Although these points are critical to validate any introduced model, my overall impression of this manuscript is still positive. Therefore, the authors need to clarify these points in the abstract and conclusion, before recommending the work for publication.

Reviewer 2

The manuscript entitled “Breakdown of the velocity and turbulence in the wake of a wind turbine - Part 2: Analytical modeling” endeavors to describe the turbulent velocity field in a wind turbine wake by accounting for energy in both the meandering and fixed frames of reference. Many of the comments that arose during the previous round of reviews were addressed by the authors, and I’d like to thank them for their frank and direct responses. In all, the manuscript describes important work deriving an analytical expression for wind turbine wake turbulence. However, because the work ends with a theoretical description, it remains unclear how effective the model will be in application for wind plant simulation, prediction, controls, design, etc. Without calibrating or validating the proposed model, training over a broader range of atmospheric conditions, error analysis, uncertainty estimation, and detailed comparison to existing models, the proposed work is incomplete and will not be likely to have the intended impact on the field of wind engineering.

Comments:

- The authors state in the opening sentence of the abstract that the novelty and benefit of the proposed model is that, "the expansion and meandering of the wake can be independently calibrated." However, no attempt is made to complete this step, and only a limited range of large eddy simulations were used to deduce model parameter values. This is a necessary step before the model can be validated and its range of application understood.
  This sentence has been changed to ***"the expansion and the meandering are taken into account independently"*** in order to avoid misleading the reader.
- The authors stated in their response to the previous comments that model parameters should be related to underlying causes (i.e., the standard deviations of velocities) rather than stability metrics. However variability in the velocity field is in fact a product of both mechanical and thermally driven turbulence. The results of the reference pointed out by the authors [1] concluded that, "With the same turbulence intensity, atmospheric stability can significantly change the turbulent kinetic energy distribution in the three spatial directions." This can only emphasize the importance of accounting for buoyancy in the model.
  The authors agree that atmospheric stability is indeed of primary importance for predicting the overall wake recovery. As shown in your citation, this is allegedly attributed in reference [1] to modifications of TKE distribution in the three spatial directions. Moreover, in [1], a mean kinetic energy budget (page 6) shows that the buoyancy term has a negligible impact on wake recovery. It thus seems that thermal effects modify the wake recovery because it modifies the distribution of TKE and the eddies' size, but not because of the buoyancy itself, and thus this buoyancy can be neglected. We did not verify this assumption, but we showed in a previous work [2] that between the neutral and unstable cases, the flow is almost identical in the MFOR.
  It may be needed to write σ_{fy} and σ_{fz} as a function of the stability, but if this can be avoided and only written as a function of the directional TKE, it will be preferred because such data will be easier to acquire. Maybe writing it as a function of the integral time scale will be needed as in the Taylor diffusion theory [3]. We think that all these questions about calibration are worth addressing as an entire paper and not only a quick note, and that is also why we wanted to address it later.
- The authors indicate that comparison to existing wake-added turbulence models would be confusing without calibration. I agree with this point in that results of such a comparison would be difficult to interpret, but I see it as another reason to pursue calibration data, rather than as a reason to omit model comparisons, error estimates, and uncertainty analysis.
  Please report to our answer at the beginning of the document.

[1] Bowen Du et al. *"Influence of atmospheric stability on wind-turbine wakes with a certain hub-height turbulence intensity".* In: Physics of Fluids 33.5 (2021), p. 055111.

[2] Jézéquel, E.; Blondel, F. & Masson, V. *“Analysis of wake properties and meandering under different cases of atmospheric stability: a large eddy simulation study”* Journal of Physics: Conference Series, IOP Publishing, 2022, 2265, 022067

[3] Cheng, W.-C. & Porté-Agel, F. *“A Simple Physically-Based Model for Wind-Turbine Wake Growth in a Turbulent Boundary Layer “* Boundary Layer Meteorol, 2018, 169, 1-10