Review of ”Investigation of higher order statistics in wind turbine wakes using Large Eddy Simulations”

The manuscript presents an analysis of higher order statistics of the streamwise velocity component in wind turbine wakes and investigates the intermittency rind around a wind turbine wake. Overall the manuscript presents interesting and relevant results and is well written. However, I have some concerns on the chosen methodology and recommend minor improvements to the manuscript. I recommend the article for acceptance after major revision.

Major

The motivation for the chosen methodology is not clear to me. The comparison between blade resolved and actuator line seems not very conclusive due to the different set ups and the BR approach does not have a clear superiority in fidelity due to the linear superposition of synthetic turbulence and laminar wake flow. The approach to include the BR results disregards the effects of inflow turbulence on the vortices generated by the blade which will trigger non-linear instabilities much sooner than in a laminar case. Furthermore, I don't see the advantage of resolving the blades but then disregarding turbulent inflow. Wouldn't one of the benefit of resolving the blades be the ability to resolve the transient effects of turbulent inflow on the airfoil lift and drag, which the standard ALM cannot represent? Finally, the appendix highlights another shortcoming of comparing the two approaches as the intermittency parameter of the empty domain evolves with downstream distance. Since the turbulent inflow is introduced at two different positions, the background flow evolves differently between the two approaches. I think the approach either needs to be validated more to highlight the benefits, or, in light of the negligible difference in the results, the BR results should not be included. This would also help to straighten the manuscript.

Minor

- l. 122: From the text I infer that the turbine has a fixed rotor speed, this fact should be emphasized more. Furthermore, rated speed is by definition the point where the pitch controller starts to pitch the blades to the keep the rotor speed constant, changing the thrust coefficient and therefore the ratio of turbulence intensity to normalized wake deficit. I think for the purpose of this study, a lower wind speed well within region 2 of the controller, i.e. between 8 and 10 m/s would be more suitable as the thrust coefficient stays constant in that region. Therefore, either the choice should be motivated more clearly or a different condition should be chosen.
- Methodology: The methodology section is lacking some critical information. In particular the BR simulations leave a couple of questions: is a rotating overset mesh used? Is simulation really conducted using LES or is it DDES as in Bock (2026)? For the AL, the smearing width compared to the cord length would be an important quantity to judge the difference to the BR simulation.
- ll. 254-261: I found the interpretation here somewhat difficult to follow since the discussed feature is not very easy to spot. Maybe the two distributions can be pointed out directly in one of the figures to better guide the reader. The differentiation into two groups is also not obvious to me. Is it based on the fact that there are 2 distributions? If so the sentence should be moved closer to the explanation.

Technical Comments

- l. 32: λ^2 is not described.
- l. 52: p should be in italics.
- l. 62: The reference introduces the actuator disk, since it is an axisymmetric mesh. A line embedded in 3D space and the force smearing, which is a crucial part of the method in my opinion, were introduced in Sørensen and Shen (2002) (`doi.org/10.1115/1.1471361`)

- Eq 6: the parameter Λ_0 is not explained.
- l. 221: The nacelle can be modelled using an actuator point with a drag coefficient, see e.g., Churchfield et al. (2015) (`https://arc.aiaa.org/doi/abs/10.2514/6.2015-0214`)
- l. 319: I would remove this sentence from the conclusions as this is not a major point of the article in my opinion.