WIND ENERGY SCIENCE DISCUSSIONS

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Comment on wes-2022-56

Anonymous Referee #2

Referee comment on "Brief communication: A clarification of wake recovery mechanisms" by Maarten Paul van der Laan et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2022-56-RC2, 2022

The proposed manuscript discusses wake recovery mechanisms for wind turbines. An analytical model, assuming constant eddy viscosity within the Boussinesq hypothesis, is developed to assess the momentum transfer from the divergence of Reynolds shear stresses. Furthermore, large eddy simulation data is also used to better quantify the role of the latter on the recovery of the wake. The manuscript main conclusion is that the lateral and vertical gradients of Reynolds shear stress are the main mechanism for wake recovery.

I find the manuscript very well written and about a topic of interest for the community. The conclusions, in terms of RANS modelling, are also useful for future developments. I therefore recommend the manuscript for publication with only the following minor remarks:

- The model relies on a constant eddy viscosity assumption. While it is approximately valid, it is still unphysical as the eddy viscosity cannot be non-zero beyond the turbulent/non-turbulent interface (for the case of a zero-pressure gradient wake with laminar inflow). A correction has therefore been proposed (see A. Townsend, Australian Journal of Chemistry 2, 1949; Cafiero et al., Journal of turbulence, 2020), that predicts radial profiles that follow a modified Gaussian shape. Such profiles differ from a pure Gaussian precisely at the edges of the wake, and can therefore be relevant when assessing the recovery mechanisms of it. While probably this point may not affect the qualitative discussion made in the manuscript, I consider it still should be addressed.
- While this is a recurrent discussion in the field, and the answer is not clear, the far wake should be better defined. This work covers streamwise distances up to 10 diameters, and therefore the wake recovery mechanisms may change further away, when the velocity deficit scales as a power law.
- While there is some qualitative agreement between figures 1 and 3, the authors never compare them quantitively. First, figure 1 corresponds to which streamwise distance? Furthermore, figures 2 and 3 cannot be compared at 7.5D by matching the maximum velocity deficit?

- A relevant assumption of the model is the self-similarity of some averaged turbulence quantities. This is an important point for both theoretical and numerical modelling (like shown, for instance, in Johansson et al., Physics of Fluids, 2003). I think it would strengthen the manuscript to comment of the self-similar nature (or not), of the curves from figure 3a.