Wind Energ. Sci. Discuss., referee comment RC2
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Comment on wes-2021-13

Anonymous Referee #2

Referee comment on "Application of the Townsend-George theory for free shear flows to single and double wind turbine wakes - a wind tunnel study" by Ingrid Neunaber et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2021-13-RC2, 2021

The paper investigates the applicability of equilibrium and non-equilibrium turbulence models from classical wake theories for wind turbine wake flows. Single hot-wire anemometry is used to characterise turbine wakes in three different scenarios: (i) a turbine subject to laminar inflow, (ii) one subject to full wake conditions, and finally (iii) a case in which the turbine is partially located in the wake of another turbine. The focus of the paper is given to verification of requirements for the validity of Townsend-George theory. The paper also studies in detail the evolution of wake centre velocity with streamwise distance and compare results of classical wake theories with the experimental data and common engineering wake models used in the wind energy community. Indeed, the subject of this work is interesting and relevant to the wind energy community. I appreciate the efforts of the authors to bridge the gap between turbulence research on wake flows in fluid mechanic community and the research on turbine wakes in the wind energy community. There are however major issues with suitability of experimental data and presentation of results. I will elaborate my comments in the following in the hope that it helps authors improve the quality of their manuscript:

- Unsuitability of experimental dataset: I think the streamwise measurement range is too short which makes it very difficult to distinguish which model works better. For instance, in figure 12, all different relationships (x, x^1/3, x^1/2) seem to capture the variation of wake width with streamwise distance. At least, the authors could use log plots instead of linear plots for both velocity deficit and wake width plots. That way it would be much easier to find more systematically the exponent of a power function, for instance, whether the wake centre velocity deficit decreases with x^-2/3 based on Eq. 1 or with x^-1 based on Eq. 3.

- Comparison of model predictions: I think the way that the comparison with previous models has been made is not fair. First, EQ or NEQ models have two coefficients that can be tuned, whereas the other two models only have one empirical input. More importantly, it is not clear over which range of streamwise distance, fitting has been done. It is problematic if the whole range of [2D, 8D] is used to fit the model. The purpose of existing turbine wake models is to predict the far wake region, and these models are not expected to work well in the near wake region. For instance, in figure 9b, if you try to fit the BP and Jensen model only for [4D, 8D], their predictions should be improved.

- Misleading title: there are two key words in the title: "dissipation" and "wind turbine array". None of them are really the main focus of this work. While C_epsilon is discussed in the paper, there is minimal discussion on dissipation in the turbine wake. Moreover, there is no more than two turbines used in this work. I therefore think it is a bit of stretch to use "wind turbine array" in the title.

- Abstract should be more specific. The first half is more like an introduction talking about the importance of turbine wake studies. Also experimental setup and the data used to study these different models are not discussed in the abstract.

- Line 80: By placing a turbine in the wake of another turbine, the behaviour of a turbine within a turbulent background is studied. However, the turbulence generated by an upwind turbine consisting of wake rotation and shedding vorticity is not expected to be identical to the one generated in the atmospheric boundary layer flow. Please clarify this either in line 80 or somewhere else in the manuscript.

- Section 3.2.2: For completeness, it would be useful to define the Taylor Reynolds number here.

- Figure 6: Self-similarity of velocity deficit profiles is examined here, but the self-similarity of shear-stress profiles should be also checked. I am conscious that with

single hotwire anemometry, it is not possible to look into this. Ideally repeat some of your measurements with x-wire, or at least mention this as a limitation of the experimental setup.

- Integral length scale: Final results seem to be quite sensitive to the value of the integral length scale. Did you try estimating its value via other methods, eg autocorrelation function? It is of interest to add a brief discussion on the impact of integral length scale evaluation on final results.

- Line 261: wake axisymmetry: Please add a brief discussion on how the presence of ground and boundary layer may affect the axisymmetry of the turbine wake in real situations.

- Line 275: Please consider using a different title for this section. By the first look, "summary" may imply that this section is the summary of the whole manuscript.

- Line 299: Please rephrase this sentence. It does not read well.

- Line 29: "extend" should be replaced with "extent".

- Line 181: "be" in "This is achieved by measuring" should be replaced with "by".

- Line 136: “a axisymmetric” should be replaced with “an …”.