Reviewer 1

Review of the manuscript wes-2021-13, entitled “Investigation of the dissipation in the wake of wind turbine array”, by I. Neunaber, J. Peinke, M. Obligado.

This manuscript leverages single-component hot-wire measurements collected through wind tunnel tests of downscaled wind turbine models to explore the potential of modeling the mean velocity deficit in wind turbine wakes and wake width through the classical theory of wakes for bluff bodies developed by Townsend and George. In the Introduction, this theory is qualitatively described, while the empirical model of the wake velocity deficit and wake width as a function of the downstream location is provided for both equilibrium and non-equilibrium turbulence in Sect. 2.1. The wake models used as a benchmark, namely the Jensen and the Gaussian wake models are introduced in Sect. 2.2. The experimental setup is reported in Sect. 3.1. A key part of the manuscript is Sect. 3.2, where the authors attempt to verify the requirements for the Townsend-George theory. Eventually, these requirements seem to be fulfilled in the far-wake of the case turbine 1, turbine 2, and only partially for turbine 2 -side. However, the proposed model will be applied to all three cases. The results seem to indicate that the proposed model fits well with the experimental data, with the equilibrium case having a smaller error than the non-equilibrium case. The authors suggest considering a virtual origin for the application of wake models, which is not a novel feature for wind turbine wake models.

We would like to thank the reviewer for taking the time to review our paper. His/her feedback does help to improve the quality of the paper by adding to its clarity and readability. In the following, we detail how we addressed each specific comment.

My main comments are:

- The clarity and sharpness of the statements and discussion should be largely improved throughout the manuscript. This is particularly important in Sect. 3.2 when assessing the requirements of the Townsend-George theory. The authors should report graphically or in a table for what range of the wake and flow case each requirement is fulfilled. It should be clarified when a complete equilibrium is achieved and when non-equilibrium turbulence is considered. The discussion about the wake turbulence properties is interesting, yet it seems elusive rather than conclusive. For instance, I was not able to find details why the core of the wake is considered in equilibrium turbulence state.

We took this comment into account and re-evaluated the manuscript accordingly. We agree that the addition of a table to summarize the fulfillment of each requirement helps the readability and added it on page 14. Also, we removed any discussion about possible occurrence of non-equilibrium turbulence from chapter 3.2 so that only the general requirements are discussed.
Also, we improved the clarity of the identification of equilibrium and non-equilibrium turbulence by adding the criteria that need to be checked in chapter 2.1 in a similar manner as the requirements, cf. page 6. In accordance with these criteria, the results are checked for the occurrence of non-equilibrium turbulence at the beginning of chapter 4, where we first investigate the behavior of the Taylor Reynolds number and the local Reynolds number finding inconclusive results (ll. 323) and then look at the behavior of Cε finding evidence for equilibrium turbulence (ll. 332, l. 483).
We also state more precisely in ll. 404,
“*This shows that beside the wake core that is characterized by equilibrium turbulence (indicated by Cε≈const. around the centerline), there are distinctive regions with non-trivial turbulence that can be found in the wake of a wind turbine.*“

We hope that this additionally helps to clarify the statements that are made regarding the occurrence of equilibrium and non-equilibrium turbulence.

- The validity of the power law to characterize downstream evolution of the wake velocity deficit is not new (see e.g. the review by F. Porte-Agel et al., BLM, 2020 and references therein). I am not sure if this work actually provides more predictable capabilities, than what is known about the use of power laws for prediction of wake features.

We agree with the reviewer that the application of a power law to wind turbine wakes is not new and we point this out in the Discussion (ll. 387):
"*Although the power law approach is not new to the description of the mean velocity deficit, see e.g. Porté-Agel et al. (2020), we add new physical aspects to the use as the formulas used here are derived from certain assumptions, and we test the requirements needed to apply the theory before using it*."
The novelty of our work therefore comes from the physical model that supports such power laws.

- The use of a virtual origin is not a new feature for a wake model, see e.g. Ishihara T, Qian G. A new Gaussian-based analytical wake model for wind turbines considering ambient turbulence intensities and thrust coefficient effects. J. Wind Eng. Ind. Aerodyn. 2018;177:275-292, and reference therein. Connections to previous works should be provided about this wake feature.

We agree that some wake models, as the Bastankhah-Porté-Agel model and the comparable model by Ishihara T and Qian G have a term like $k \cdot X/D + b$ where k is defined as the wake growth rate and b is an initial wake width that could also be interpreted as a virtual origin. b should therefore be in the order of magnitude of the turbine diameter.
In contrast, in the Townsend-George theory, a *true* virtual origin that is depending on the flow's streamwise development is present. In it, the transition of the wake to its final turbulent state is taken into account, and it is not restricted. We clarify this in ll. 461 in the Discussion,
"*Another reason is the implied virtual origin that is common in free shear flow analysis as it includes the transition of the wake to the final turbulent wake state. It gives these models a high flexibility to adapt to different wakes. While the concept of a virtual origin does occur in the form of the initial wake width in other wake models, e.g. the Bastankhah-Porté-Agel model, a "true" virtual origin that is not limited to a value of the order of magnitude of the turbine diameter improves the flexibility*."
And in ll. 504 in the Conclusion
"The virtual origin native to the Townsend-George theory was identified as one main advantage of the bluff body wake models. As mentioned above, this virtual origin differs from the concept of the initial wake width used in some of the engineering wake models in that it accounts for the turbulence transition (and therefore is common to several turbulence one-point quantity scalings)."

More comments are reported below, which I hope might help to revise the current manuscript.
Comments:

1. L1-6, These statements seem more suitable for an introduction rather than an abstract. I would suggest sharpening the focus of the abstract highlighting the research strategy and the results obtained.

We rewrote the abstract. It now focuses on the research strategy and the results.

2. L16-17, you can clarify that wake interactions may occur only under certain wind conditions, namely wind direction, incoming wind speed and stability regimes. Wake interactions do not occur in a continuous fashion. Please add some references on this topic.

We clarified that wake interactions do not continuously occur and added a reference (ll. 19) "*Wind turbines are usually clustered in wind farms with the consequence that downstream turbines operate depending on the wind direction and the wind speed in the turbulent wakes of upstream turbines (e.g. Barthelmie et al. (2007); Sun et al. (2020)).* "

3. L35, Please clarify the following statement. What do you mean for "…the shear layers surrounding the wake have met"?

We agree the reviewer that the sentence was unclear. We did change the sentence (ll. 39) to: "*The far wake is typically identified as the part of the wake where the shear layers that evolve between the faster ambient flow and the lee of the object of investigation have met and the turbulence is fully developed.*"

4. L106, cross-check the equation for the momentum thickness. I believe the first term in the integral should be U rather than U_infty.

Thank you, we changed this.

5. L110-120, can you provide concisely the difference in flow properties for an equilibrium and non-equilibrium turbulence?

While we left this part unchanged, we listed the criteria for equilibrium and non-equilibrium turbulence on page 6 to clarify the difference:

„***Equilibrium/Non-Equilibrium turbulence Criteria.***

i. *Does $C\varepsilon$=const. hold? In this case, no Reynolds number dependence should be seen.*
yes*: equilibrium turbulence*
no*: indication for non-equilibrium turbulence*

ii. *The Taylor Reynolds number $Re\lambda$ and the local Reynolds number ReL need to change with downstream distance in order to verify criterion i. More specifically, ReL has to decrease according to George (1989) in the case of non-equilibrium turbulence.*
If ReL and $Re\lambda$ do not change, it is therefore not possible to draw conclusions on the occurrence of equilibrium and non-equilibrium turbulence and the results are inconclusive."

6. L221, "In addition, the Taylor Reynolds number is supposed to change so that the presence of equilibrium and non-equilibrium turbulence can be disentangled." This statement is not really clear. Do you mean that status of non-equilibrium turbulence occurs when the Taylor Reynolds number varies with x? Please clarify.

To clarify this part, we introduced criteria to distinguish between equilibrium and non-equilibrium turbulence on page 6.
Also, we now separate the discussion of the fulfillment of the requirements in chapter 3.2 and the investigation of the flow regarding equilibrium/non-equilibrium turbulence in the beginning of chapter 4 to improve the readability by not mixing the two parts of the investigation.

7. Sect. 3.2.2 should be re-written and clarified. Express clearly the conditions for Re_λ and Re_L and state for each flow case under what wake regions those are satisfied.

We re-wrote section 3.2.2. so that it contains now solely the discussion of the requirement $Re_\lambda > 200$, and we shifted the discussion of $Re_\lambda(X/D)$ and $Re_L(X/D)$ to the beginning of chapter 4, the Results. There, we added also the wake regions

8. Fig. 5. Please specify the locations where these velocity signals were collected.

We added the locations in the caption of figure 4.

9. L 265, "The errors for L/δ were calculated using error propagation." Please provide more details on the mentioned error propagation method.

We now provide more information on the calculation of the individual errors of L and delta in the appendix.

10. At the end of section 3, maybe where now there is a not-numbered section denote "Summary" I would add a table or a sketch summarizing the results of this analysis for each flow case, namely the five requirements and the wake regions where those are fulfilled.

We added a table (table 1) on page 14 to summarize the results of the investigation of the requirements.

11. Figs. 9, 11 and related text, I am not sure applications of the Jensen and BP models have been done properly. Typically, wake velocity fields are calculated for individual wind turbines; then, an overlapping wake model is used to predict the wake flow in presence of wake interactions. Is this the procedure you applied? Please provide details.

The reviewer raises a fair point. Indeed, contrary to the often-used overlapping wake models, here we treat each wake as individual wake. We clarify this now in the new manuscript, that now reads (ll. 352):
"*Note that we do not apply superposition wake models for the wakes of turbine 2 mid and side here but treat the wakes individually because we are interested in the difference a turbulent inflow has on the fit. With the hypothesis that a final universal turbulence state can be reached within a wind farm where multiple wakes are overlapping, the modeling of these multiple wake scenarios is not a question of superposition but rather of how and where this final turbulence state is reached. In this philosophy, the investigation of the individual wakes is thus of interest.*"

12. L 353, I am not sure you discussed the detection of a turbulent/non-turbulent interface for the wake. Please clarify, in case I missed this discussion.

We did not, and clarified this in the text, it now reads (ll. 399):
"*Another very interesting effect is visible at some points in these plots: at the wake edges at the turbulent–non-turbulent interface between the wake and the laminar inflow, C_ε is significantly higher than inside the wake.*"

13. L359-360, what flow properties did you leverage to infer that the wake center shows equilibrium turbulence.

In the wake center, *Cε~const.* which, together with a change of the local Reynolds number *ReL* is a fulfilment of the criteria for equilibrium turbulence as stated in the added criteria on page 6. In addition, we specify now in ll. 404
"This shows that beside the wake core that is characterized by equilibrium turbulence (indicated by Cε≈const. around the centerline), there are distinctive regions with non-trivial turbulence that can be found in the wake of a wind turbine."

14. Fig. 10 and related text (L355 - 360). This mentioned ring with enhanced values of $C_{\$}$ is a bit elusive for all the three flow cases investigated. The region with higher $C_{\$}$ is located at the boundary of the measurement domain, which makes it difficult to assess if this is actually a ring or an artifact due to the interaction between the wake and the background wind tunnel flow. Any comment clarifying this comment would be beneficial.

We agree that our conclusion was drawn from experience rather from data evidence, and we therefore changed the text to clarify this. It now reads (ll. 399):
"*Another very interesting effect is visible at some points in these plots: at the wake edges at the turbulent–non-turbulent interface between the wake and the laminar inflow, Cε is significantly higher than inside the wake. While the data presented here is not sufficient to draw conclusions, we suspect that this may indicate a ring of large Cε surrounding the wake, similar to the ring of high intermittency that was found to surround the wake in Schottler et al. (2018) and that was shown to be traceable along the whole measured range in Neunaber et al. (2020). However, further investigation on this topic is needed to confirm this."*

15. L 426-426, please specify for what wake region the five requirements of the Townsend-George theory are fulfilled for each flow case.

We specify this now in table 1 on page 14.

16. L 429-430, specify what feature of $C_{\$}$ provides evidence for equilibrium evidence, in what wake region and for what flow case.

We clarify now in ll. 483
"*By means of Cε(Reλ)≈const., we found evidence for equilibrium turbulence in the investigated parts of the wake for all three scenarios"*
Which clarifies, as we hope, the passage together with the criteria from page 6.

17. L454, please revise the text reporting that the Jensen model was formulated only considering mass conservation (JensenNO.A note on wind generator interaction. Risø-M1983)

Thank you, we corrected this (l. 516)

18. L446, The use of a virtual origin for a wind turbine wake model is not a novel concept, see e.g. Ishihara T, Qian G. A new Gaussian-based analytical wake model for wind turbines considering ambient turbulence intensities and thrust coefficient effects. J. Wind Eng. Ind. Aerodyn. 2018;177:275-292, and reference therein.

As discussed in detail above, we agree on this and added in ll. 504 in the Conclusion
"The virtual origin native to the Townsend-George theory was identified as one main advantage of the bluff body wake models. As mentioned above, this virtual origin differs from the concept of the initial wake width used in some of the engineering wake models in that it

accounts for the turbulence transition (and therefore is common to several turbulence one-point quantity scalings)."