

Referee report on “Effect of inflow turbulence on the wake replenishment mechanisms of twin rotor vertical axis wind turbine”

General comments

The article presents an experimental investigation of the wake recovery mechanisms implicated in the turbulent wake of a vertical axis wind turbine immersed in external free-stream turbulence of two types: low and high. The authors use LDV anemometry to compute the terms involved in the momentum transport equation and pointed out vertical advection as the main contributor to wake recovery. Their results seem to indicate that turbulent transport is negligible. The introduction needs to be reworked and the motivations should be clarified from the beginning, including relevant citations. Authors should avoid the overuse of generalities and be specific with the content of their statements. The article is structured in a very confusing manner and is written in an English that makes it difficult for the reader to appreciate the analysis. The last part of this review has some typos that I could catch but I stopped at some point given the sheer number, please proof-read. The main text contains discussions referencing several figures in appendixes C and D. This defeats the purpose of an appendix. More importantly, I have concerns regarding the measurement accuracy of the turbulent terms since they are derivatives of second order moments, which are clearly under-resolved compared to PIV (see figure B2). Furthermore, the reference area over which the terms are space-averaged is chosen arbitrarily to contain the wake, but disregard the effect of inflow turbulence on the mean wake size. For these reasons, I recommend publication of these results only after major revisions of the elements presented in this review.

Specific comments

1. *[Abstract]* The last two sentences need to be reformulated. They are difficult to understand and have several typos. e.g : 'In contradiction to earlier claims, ours results find a lesser contribution to the budget.....'
2. *[Section 1, Introduction]* Include citations for the description of the wind farm issue (i.e Porte Agel 2019). 'Marine servicing' only applies to off-shore facilities, "Not disturbing other activities" is not the main reason that explains why wind turbines are grouped in wind farms.
3. *[Section 1, Introduction]* Describe better why second array wind turbines are impacted in a wind farm due to wake effects : less mean velocity to extract kinetic energy from, higher turbulent intensities. Include relevant citations. Line 21: 'With the development of floating application' needs to be reformulated. Line 24 'Wake power loss' is a very confusing phrasing for the reader, please clarify. line 25: "But this result lacks of experimental validation": General comment, please reformulate the scientific description of the objective.
4. *[Section 2, Experiment set-up]* The explanation for the use of water instead of air should be better addressed. Justify clearly the claim: *in this case the physics remains the same in air and water*. Moreover, specify why the flume tank provides the correct '*profiles*' for the study in terms of mean flow and turbulent intensity, define turbulence intensity and give specific levels or trends.

5. The authors start using notations for the velocity components (u, v, w) around line 1. 80 without defining them nor specifying to which referential these are tied to. Please either add it to figure 2 or mention it in the main body of the text. This can be extended to earlier in the article where the authors mention the study of the momentum transport equation, it would be useful to the readers to clearly write the equation and define the velocity components, the mean and the fluctuating contributions to the equation.
6. [Section 2, Experiment set-up, l. 87] Here, two grids are used to observe the effect of turbulence on turbine wake dissipation. Please rephrase. In the context of turbulence, dissipation is a different quantity related to the transformation of small scale kinetic energy into heat due to viscosity (see Tennekes & Lumley 1971). The authors maybe meant wake development.
7. [Section 2, Experiment set-up] Not enough details are given as to the differences between the two grids. The reasons behind the use of a solid plate at the bottom for grid 2 is not clear. What is the porosity of each plate? Why do they have different porosities and mesh sizes? I guess the reasons are to pre-shear the mean inflow while generating turbulence, injecting more than one energy-containing scale. This needs to be mentioned with appropriate citations and discussed. See PhD work by T. Huret (2023) and investigations led by K. Steiros, M. Hultmark and J.C Vassilicos about turbulence grids.
8. [Section 2, Experiment set-up, eq. 7] Equation 7 has a problem when analysing the near wake where mean recirculation zones may appear bound by $\bar{u} = 0$. This should be discussed.
9. [Section 2, Experiment set-up, l. 125] This is not a production term at all. It is the Reynolds normal stress term of the momentum transport equation, not the turbulent kinetic energy budget equation. Moreover, your shear layers are not thin. However, the boundary layer gradients argument is valid. More importantly, contrary to the author's claims, scientific literature has examples where the mean pressure term was estimated using the Poisson equation and where terms in the kinetic energy budget was studied for wakes, for recent examples see Shanmughan et al. (2020) and Fuentes-Noriega & Mazellier (2025), Pope (2000) has several historic examples of experiments by Champagne, Corrsin and other seminal contributions.
10. [Section 3.1, Wake description] I have great concerns with the choice of the surface area over which the different terms are averaged. It is an arbitrary geometric surface which does not take into account the effect of the turbulence inflow on the actual mean wake size. This can lead to a bias when comparing both cases. I would suggest choosing an iso-velocity contour close to U_0 which may be a better proxy for representing the actual wake. The reason behind wake recovery is entrainment, i.e free stream flowing into the wake characterised by a velocity deficit. That being said, this mass and momentum exchange happens over a surface area that is unique to each streamwise position and to each wake. The claim '*However, all the conclusions remain the same regardless of the reference surfaces investigated.*' in line 274 seems highly questionable.
11. [Section 3.1, Wake description l. 188] Avoid discussing appendix figures in the main body of the text. Annexes are used for technical complements to the investigation. If the authors need to discuss these contents in the main body of the text then they do not belong in an appendix.
12. [Section 3.3, Investigation on the replenishment mechanisms] I also have concerns on the accuracy of the measurements when computing the turbulence contributions to the mean wake recovery $\partial \bar{u} / \partial x$. From figure B2, there are clearly differences in the gradients of second order

moments when comparing them to PIV with higher spatial resolution. You might be severely underestimating the actual contribution of turbulence to the transport of momentum. Please compare one of the term with PIV to estimate the accuracy.

Technical corrections

- p. 1, title missing 'a' before twin rotor
- p. 1, l. 16: Syntax problem with the sentence (*Although* is not properly used here.).
- Tab. 1: space before units missing and typo in caption.
- Fig. 3: Caption unclear. Vertical profile or vertical velocity?
- p. 6, l. 108: Improper use of *embrace*.
- p. 8, l. 135: plans $\rightarrow$ planes.
- p. 8, : data rate $\rightarrow$ acquisition frequency.
- Fig. 8: Notation $\langle \cdot \rangle$ introduced without defining.
- Fig. 10: Caption typo, repeated from Figure 9.
- p. 11, l. 181: typo in '*compares*'.