Answer to reviewers.

The authors would like to warmly thank the reviewers for their careful correction and constructive comments. The article has been substantially amended to incorporate the clarifications suggested in the comments. The English has been checked and rephrase. Typos and formatting have been systematically corrected in the text. The changes appear in blue in the new version of the article, and we have highlighted the lines containing specific additions in response to each comment. In the following document, the comments are reproduced in black and the answers are written in blue.

1 Reviewer 1

1.1 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.

We understand that our wording may have led to conclude from our article that turbulent transport was negligible compared to mean velocity transport in high and low inflow turbulence. Our objective was instead to highlight the earlier onset of mean velocity transport, alongside the enhanced Reynolds shear stress transport under turbulent inflow conditions. Turbulent transport is not negligible in either inflow condition. The article has been carefully revised to clarify the main question, objective and methodology, in the abstract and in the introduction. The English wording and typos were also addressed thanks to a proofread by an English-speaking colleague.

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.

The grid spacing is indead a key point of our experimental set up and has been the subject of a specific study. Firstly, the grid was chosen in line with other studies in the literature [1]. The effect of grid resolution was also studied using PIV measurements and is presented in details in the answer of comment n°12. The measurement uncertainties caused by the effects of the grid resolution and the LDV acquisition frequency prevent to determine whether advection or turbulent transport is greater. However, the main conclusions of our study (the differences

observed between the two cases, the importance of the vertical advection and its earlier onset caused by the inflow turbulence) are significant as these phenomenons are greater than the estimated bias.

1.2 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.....' :

The sentences have been reworded to make the conclusions clearer (lines 7 to 14).

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.

The citation [2] has been added line 17. 'Not disturbing other activities' remains in second place, as this is not the main reason.

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.

Precise information has been provided with citations (lines 18-21).

Line 21: 'With the development of floating application' needs to be reformulated.

This has been changed to 'As floating farms are developed' line 24 of the revised manuscript.

Line 24 'Wake power loss' is a very confusing phrasing for the reader, please clarify.

Changed for "power production loss due to wake effect" line 27.

line 25: "But this result lacks of experimental validation": General comment, please reformulate the scientific description of the objective.

Precision has been added with experimental results from literature, lines 26 to 28.

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.

The experimental setup section has been modified accordingly: in water, with the same velocity and model size, Reynolds number is 15 times greater in water than in air, bringing the experiment closer to dynamic similarity. Flow seeding is also more convenient. Physics remains the same because:

- There is no free surface deformation; therefore, the top boundary condition does not affect the flow around the wind turbine.
- There is no cavitation so the flow is monophasic.
- Mach number 0.03«1 (0.12 at full scale in air), therefore the flow is incompressible.

The relevance of the use of water has been justified lines 64 to 70.

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.

Section 2 has been divided into subsections for better readability. These specifications are given in the subsection entitled '2.2 Flow characteristics', lines 87 to 130. Mean flow and turbulence intensity profiles are presented in figure 3. Turbulence intensity is defined in Eq. 6,

line 120. Specific levels and trends are given in paragraphs lines 106 to 110 and 121 to 126.

5. The authors start using notations for the velocity components (u,v,w) around line l. 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.

Figure 2 and the text lines 90&91 have been modified accordingly.

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.

The text has been changed to: 'The effect of inflow turbulence on turbine wake recovery.' line 97

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.

The text has been revised to provide more details lines 96 to 101 and in figure 2 caption. The caption of figure 2 has also been modified. Grid 1 is made of two coarse mesh panels made with 2 mm thickness wires evenly-spaced every 10 mm and three fine mesh panels made with 0.7 mm thickness wires evenly-spaced every 2 mm. Grid 2 is made of a 20cm high solid plate at the bottom and of grid 1 above it [3]. The purpose of the grids is to generate a sheared velocity profile. The purpose of the solid plate at the bottom of grid 2 is to generate turbulence. Due to the 10-metre distance between the grid and the VAWT model, the turbulence generated by the solid plate contains more than one energy scale. This is discussed and verified in the "Flow Characteristics" section, lines 101 to 105.

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 u = 0. This should be discussed.

There were no measurement points in PIV or LDV where the mean velocity was zero or close to it. The smallest velocity measured was above $0.2U_0$. Therefore, Eq. 7 can be used as there is no risk of dividing by zero.

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.

There was indeed a confusion, the text has been changed accordingly, line 140.

Moreover, your shear layers are not thin. However, the boundary layer gradients argument is valid.

Text has been changed accordingly line 140.

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.

Thanks for the interesting references. We consider that the assumption to consider $\overline{u}\frac{\partial u_i}{\partial x}$ negligible compared to $\overline{v}\frac{\partial u_i}{\partial y}$ or $\overline{w}\frac{\partial u_i}{\partial z}$ is to strong which does not allow us to calculate $\frac{\partial p}{\partial x_i}$ using the Reynolds equation following the method presented in [4]. Nevertheless, the measurements carried out enable to compare the contribution of mean and turbulent velocities to wake recovery. Precision has been added in the manuscript lines 140 to 142.

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 U0 which may be a better proxy for representing the actual wake.

The paper has been revised using a reference surface delimited by iso-velocity contour $\overline{u} = 0.9\overline{u_0}(z)$ (see lines 167 to 170 and lines 196 to 201). This surface does indeed better capture the areas where both turbulent and mean flow transport occurs. The main observations and conclusions remain.

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.

Indeed, it seems that turbulent transport acts at the core of the wake, while Y and Z advections act more at the boundary, bringing momentum to the shear layer of the wake. These comments have been added to the paper lines 214 to 221 and lines 225 to 231. Therefore, the choice of reference surface impacts the results: the 1.5Dx1.5H reference surface tends to maximise advection and mitigate turbulent flow, whereas the 1Dx1H rotor outline does the opposite. The surface delimited by $\overline{u} = 0.9\overline{u_0}(z)$ seems a good compromise. The main conclusion, i.e. the earlier onset of transport by mean velocity contribute to the wake recovery **in addition to** enhanced Reynolds shear stress transport in turbulent inflow conditions, remains whatever the surface chosen among the surfaces mentioned before.

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.

Paper revised accordingly by including figures 8, 9, 11, 12, 13, 14 and comments lines 185 to 194 and 214 to 231.

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 $\frac{\partial u}{\partial x}$. From figure B2, there are clearly differences in the gradients of second order 2 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.

3C LDV was chosen over 2D-3C PIV because it allows for better measurement of the Y components that would have been out of plane in the PIV setup. Refinement of the measurement grid was an important consideration, with the goal of balancing measurement time with the accuracy of spatial derivatives. PIV measurements were used to investigate the

effect of grid resolution. The PIV measurements were performed at an acquisition frequency of 15Hz during 40s. The PIV data were then processed by adaptive PIV using an initial interrogation area of 128×128 pixels and a minimal initial interrogation of 64×64 with 32 pixels of overlap. The measurement planes are placed in the vertical central plane of one rotor with $0.385m < x < 1.46m$ and $0.01m < z < 0.485m$. Therefore, comparison between PIV and LDV are possible in the vertical central line of the rotor section at $X = 2D$ and $X = 4D$. The following figure shows the PIV plane placement in the LDV grid in red. As the PIV plane is placed on the rotor's symmetry plane, it does not exactly match the position of the LDV measurement points. The LDV-PIV comparison will be made using the nearest LDV points.

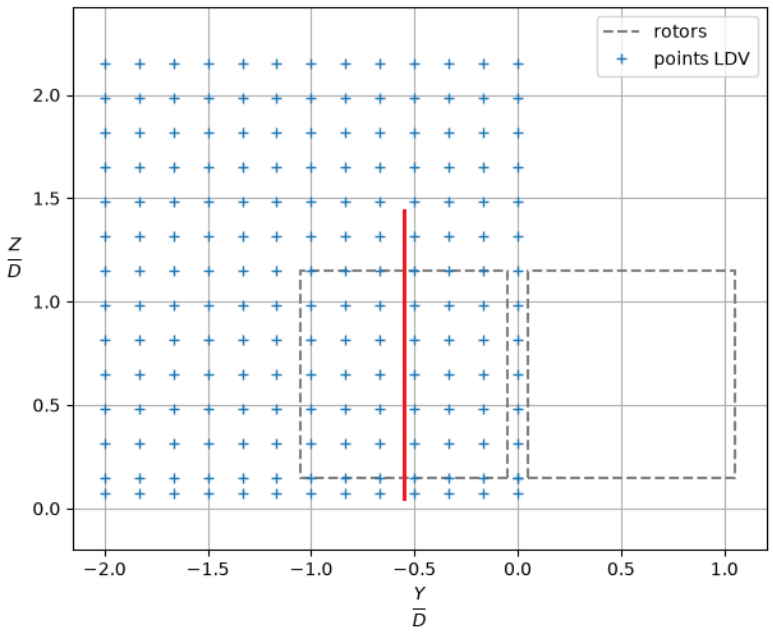


Figure 1: The measurement points in the machine's reference frame. The rotors outline are shown in gray dashed line. The red line is the PIV plane.

Figures 2 and 3 show a comparison of PIV and LDV measurements of the mean and turbulent quantities required for the computation of transport terms. Measurement of $\overline{u}$ shows good agreement between the LDV and PIV measurement. The downward velocity $\overline{w}$ in the upper part of the wake is higher in the LDV measurement than in the PIV measurements. It is unclear at this stage whether the discrepancy originates from the PIV or LDV measurements, or from the misplacement between the LDV and PIV measurement planes. This difference will affect subsequent transport computations. The Reynolds shear stress $\overline{u'w'}$ shows good agreement between PIV and LDV in coincident mode.

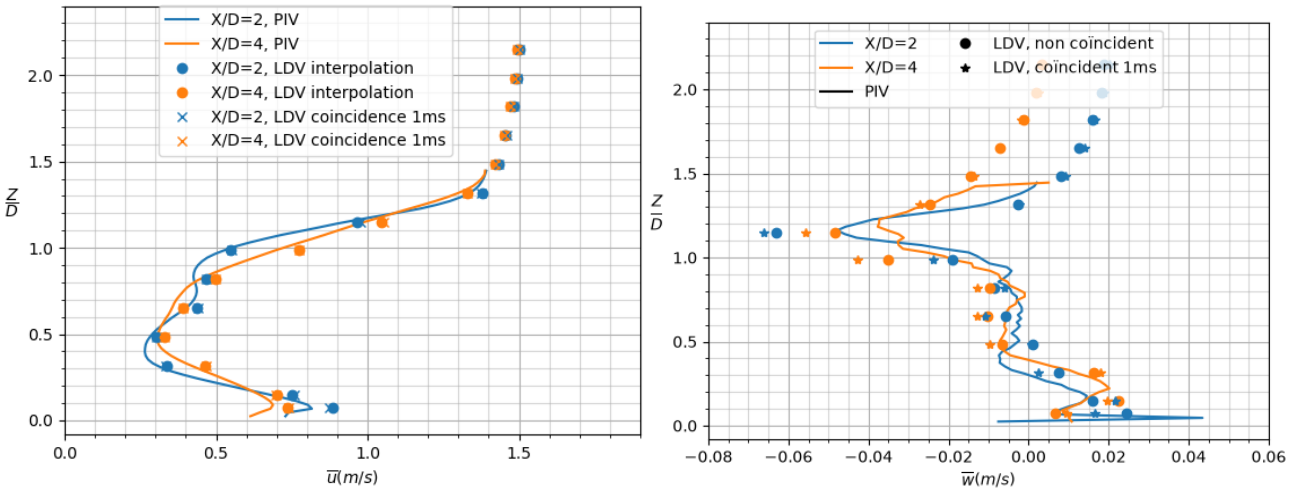


Figure 2: Vertical profiles of mean velocities $\overline{u}$ and $\overline{w}$ obtained with PIV and LDV measurement in the middle plane of one rotor, at $X/D = 2$ and $X/D = 4$.

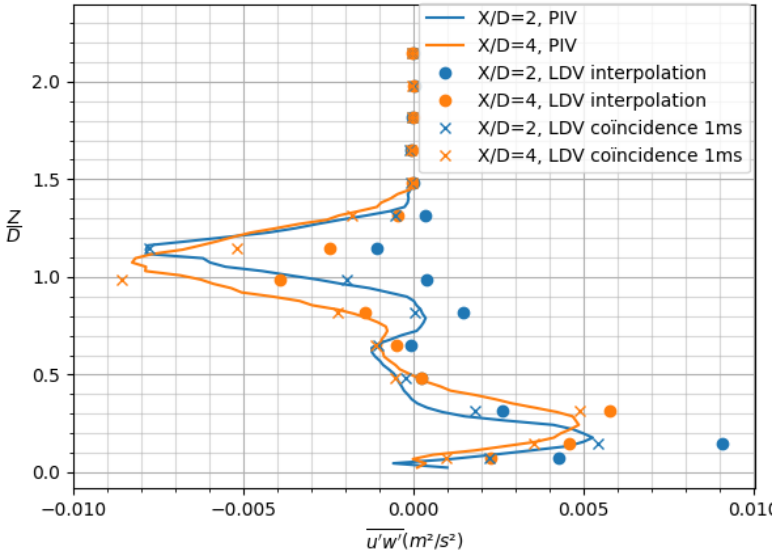


Figure 3: Vertical profiles of X-Z Reynolds shear stress obtained with PIV and LDV measurement in the middle plane of one rotor, at $X/D = 2$ and $X/D = 4$.

Figure 4 shows the computation of the transport terms $\frac{\overline{w}}{\overline{u}}\frac{\partial \overline{u}}{\partial z}$ and $\frac{1}{\overline{u}}\frac{\partial \overline{u'w'}}{\partial z}$ from PIV measurements, averaged over the height of the PIV plane. The averaged transport is computed the same way as in the article :

- The transport terms are computed using the measurement grid.
- The transport terms are bi-linearly interpolated into a finer grid **to avoid truncation error**.
- The transport terms are averaged over the surface.

To demonstrate the impact of the measurement grid resolution, the PIV resolution is artificially reduced, and the outcomes is presented as a function of grid spacing. As you suggested, using a coarser computation grid leads to an underestimation of the averaged transport terms. Using the grid employed for LDV measurements resulted in an underestimation of approximately 20%. From the figures, a comparison between the two terms seems possible up to a grid spacing of

0.07 m. Beyond that, the values obtained are highly unreliable. It is notable that the advection transport computed from LDV is significantly higher than that computed from PIV. This is due to the higher cross-stream velocity $\overline{w}$ in the LDV data, rather than the grid resolution.

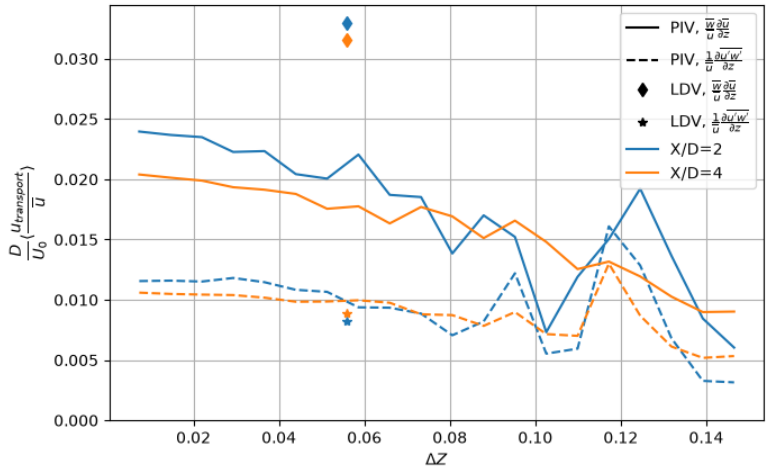


Figure 4: Averaged momentum transport from PIV as a function of computation grid spacing.

Although the LDV and PIV comparisons show differences, we believe that they do not affect the main conclusions of the article:

- Considering the low inflow turbulence case, results are in good agreement with literature
- The ranking of turbulent and mean flow transport is the same in PIV or LDV. It is also in agreement with the scientific literature.
- The objective of the paper is to highlight and understand the earlier onset of mean flow transport, the ranking of contributions is not the issue here.
- Computed transport is consistent with the observation of cross-stream velocities: higher cross-stream velocities lead to higher mean flow transport.

1.3 Technical corrections

- p. 1, title missing 'a' before twin rotor see Title.
- p. 1, l. 16: Syntax problem with the sentence (Although is not properly used here.). see line 18.
- Tab. 1: space before units missing and typo in caption. see page 4.
- Fig. 3: Caption unclear. Vertical profile or vertical velocity? see page 6 figure 3.
- p. 6, l. 108: Improper use of embrace. see line 121 to 126.
- p. 8, l. 135: plans → planes. see line 147.
- p. 8, : data rate → acquisition frequency. text revised accordingly
- Fig. 8: Notation $\langle \cdot \rangle$ introduced without defining. see line 201
- Fig. 10: Caption typo, repeated from Figure 9. see figure 14 page 15
- p. 11, l. 181: typo in 'compares'. see line 180

2 Reviewer 2

2.1 General comments

The study describes and experimental approach to identify wake recovery for a novel twin-rotor vertical axis turbine. At this stage is not yet well organised and still needs a significant amount of language editing. Beyond these technical shortcomings, some choices in the experimental setup are not well justified.
The article has been carefully revised by an external party to clarify the main question, objective and methodology. We also addressed the English formulation and typos, and hope that it improved the article.
My main concern is that both the temporal and spatial resolution of the wake measurements might not be sufficient to draw the conclusions made about the main wake recovery mechanisms. These seem at this stage rather uncertain, a deeper analysis of uncertainty would help to trust the results. Furthermore, measurements with higher resolution (PIV?) would also make the presented results more trustworthy.
In the previous version, we emphasised that momentum transport by mean flow is greater than turbulent transport. However, taking grid resolution into consideration, we believe this to be an overstatement, and the paper has been revised accordingly to mitigate this conclusion. In the revised article, we highlight the earlier onset of mean velocity transport **in addition to** enhanced Reynolds shear stress transport in turbulent inflow conditions.
Temporal and spatial resolution were key elements of this study. We used PIV measurements and bootstrapping to ensure that our conclusions were robust. The details are presented in the following section.

2.2 Specific comments

• Twin turbine setup: While the twin turbine setup of two two-stage vertical axis rotors is very interesting, there are some points that need to be clarified in the manuscript:
(1) Single rotor reference case: Could it be a strategy to analyse the wake recovery mechanisms of one single vertical axis rotor as a reference case first? It is difficult to distinguish to which degree the recovery mechanisms presented for the twin rotor are influenced by rotor-rotor interactions or if they are inherent for a single rotor as well. This is indeed an effective strategy for improving our understanding of how the two wakes interact. This has been done in the scientific literature [1]. This was not done here due to time and budget constraints.

(2) Rotational direction: It is stated that the turbine is a inner downwind configuration, which is not completely clear to me what that means. Also, are the two rotors co- or counter-rotating? I consider this important information when it comes to the symmetry of the wake flow and as a justification of measuring only one half of the wake.
The two rotors are counter-rotating. This has been added to the text line 73. The inner downwind condition means that the blades move downwind when they are near the wind turbine's symmetry plane. This is well known in the VAWT field, but can be added in the text if the reviewer thinks that it is necessary.

(3) Rotor synchronization: It is stated in the text that the two rotors are not angularly synchronized. In table 1 an angular offset of 90 deg is mentioned. Does that refer to the two stages of the rotors, or an angular offset between the two rotors? Moreover, I would guess that the synchronization/angular offset of the two rotors does potentially influence the wake recovery. If a twin rotor setup is first analysed this parameter should be investigated (or for a reference case defined to a fixed value).

The angular offset of 90° is between the two levels of each rotor. We don't know if the rotor's synchronisation affect the wake resorption and this parameter was not investigated.

• Wake measurements- Temporal resolution: The manuscript states that the coincidence data rate for multi-component LDV measurements was approximately 1 Hz. Given the masnuscripts incoming flow speed and turbine's rotational speed, the blade passing frequency and blade-vortex shedding dynamics should occur at frequencies above 10 Hz. While a 1 Hz effective sampling rate can yield accurate mean velocity statistics (u, v,w) if the acquisition window is sufficiently long, estimating second-order turbulent moments (uv, uw) and their spatial derivatives requires statistical convergence. Non-equispaced LDV arrival times combined with low coincidence rates can bias turbulent shear stress measurements toward higher-velocity bursts. Please include error bounds or confidence intervals (e.g., via bootstrapping) on the estimated Reynolds stress gradients (uv/y) to demonstrate that statistical uncertainty does not overshadow the physical transport terms.
The low temporal resolution is due to the coincidence mode of the 3C LDV. As requested, we have calculated the bootstrap error with B = 1000, and the results are presented in the main body of the paper figures 15 and 16. The bootstrap uncertainty does not overshadow the results.

• Wake measurements- Spatial resolution: Wake measurements were per formed at downstream distances of 2D, 4D, 6D and 10D. The authors present a momentum budget analysis across the turbine wake based on a 13 × 13 measurement grid (y = z = 0.05 m = D/6). While this spatial resolution may be sufficient to capture macro-scale velocity recovery in the far wake, it appears inadequate for accurately resolving spatial derivatives in the shear layers of the near-wake. Near-wake shear layers typically have a thickness on the order of 0.05D to 0.1D. With only 6 grid points across the entire rotor diameter, the discretization provides at most 1–2 points across the steep gradient region. Discrete spatial differenti ation (e.g., central differencing) on such a coarse grid will introduce significant spatial smearing and truncation errors, leading to an underestimation of both mean advection gradients (vU/y, wU/z) and turbulent transport deriva tives (uv/y, uw/z). If finer spatial measurement grid data are not available (see next comment on PIV), consider restricting the full budget quantification to regions where spa tial gradients are well-resolved (e.g., far-wake regions where profiles approximate smooth Gaussian shapes) and explicitly acknowledge the truncation errors near the rotor. • Particle Image Velocimetry (PIV) measurements: As both the temporal and spatial resolution of the LDV measurements might be low, I wonder if PIV measurements with a higher spatial (and maybe also temporal?) resolution would not have been a better dataset for the presented analysis. On page 9, it is men tioned that PIV measurements have been carried out under the same experimental setup. Results of the comparison are then presented in Appendix B, which would be nice to have in the main body of the paper. However, any details about the experimental setup of the PIV measurements are lacking. It would be nice to have at least some information about the resolutions and a justification why the LDV data were chosen for the analysis over the PIV data.

This key issue was also raised by Reviewer 1. The answer is set out in detail in section 12 of the responses to Reviewer 1.

2.3 Technical corrections

• Line 40: Unfortunately, ... didn't lead to a faster wake recovery. Unfortunately is judgemental and not fitting here. Also, didn't is informal language. see line 44
• Line 144: wich sentence changed

• Line 181: compares see line 180
• Figure 10 Mean vertical transport instead of turbulent kinetic energy see figure 14
• Figures C1- C6: On all these plots, the left hand side (low turbulence case) and right hand side (high turbulence case) look exactly appendix removed and placed in the main body, figures 8 and 9.

References

[1] Ayoub Talamalek, Mark C Runacres, and Tim De Troyer. Experimental investigation of the wake replenishment mechanisms of paired counter-rotating vertical-axis wind turbines. Journal of Wind Engineering and Industrial Aerodynamics, 252:105830, 2024.

[2] Fernando Porté-Agel, Majid Bastankhah, and Sina Shamsoddin. Wind-turbine and wind-farm flows: A review: F. porté-agel et al. Boundary-layer meteorology, 174(1):1–59, 2020.

[3] Maelys Magnier, Nina Delette, Philippe Druault, Benoit Gaurier, and Gregory Germain. Experimental study of the shear flow effect on tidal turbine blade loading variation. Renewable Energy, 193:744–757, 2022.

[4] E Fuentes Noriega and Nicolas Mazellier. Scaling analysis of the swirling wake of a porous disc: application to wind turbines. Journal of Fluid Mechanics, 1003:A34, 2025.