Dear Prof. Zhang,

Thank you for giving us the opportunity to submit a revised version of the manuscript titled "Experimental analysis of a horizontal axis wind turbine with swept blades using PIV data" to *Wind Energy Science*. We appreciate the time and effort that you and the reviewers have dedicated to providing valuable feedback on our manuscript. We have been able to incorporate changes to reflect the suggestions provided by the reviewers. Please find a point-by-point response to their comments below.

Reviewer 1: Dr. Alessandro Fontanella

1. As a general comment, I would like to see (at the beginning of the methodology or in the introduction) an explanation of what is expected to happen in a swept blade from the aerodynamic point of view, especially if compared to a straight blade. Authors decided to measure and present in the article some quantities (velocities, flow angles, section loads) and I ask them to explain why these quantities are important to study (e.g., because they see a significant change passing from a straight blade to swept blade, or because they are difficult to predict with current engineering models for rotor aerodynamics, . . .).
Thank you for this comment. Previous numerical investigations have shown that blade aerodynamics are affected by sweep. In particular, sweep leads to a misalignment between the airfoil orientation and the local inflow in the swept part of the blade. Additionally, the trailed vorticity system is displaced in azimuthal direction, and the now curved bound vortex (thinking in lifting line terms) induces a velocity on itself. The relevance of the experimental dataset presented in this study then lies in enabling the validation of numerical models in terms of quantities such as induction, flow angles and blade loads. We have added this line of argumentation in the introduction section, see Page 2, Line 44.

2. The scale model blades show small pitch offsets and a bend-twist coupled elastic response. At line 146 it is said that blades were designed to be stiff, thus I suppose that the bend-twist deformation is unwanted, but it seems to affect results. I ask authors to clarify this aspect and explain which effects in the results are wanted and which are not, but are a consequence of manufacturing difficulties (that I think are normal at this scale).
You are right, the bend twist coupling was unintended but does affect the results. We have clarified our motivation to built stiff blades in the description of the scaled model, see Page 4, Line 85. Furthermore, we confirm that it is indeed challenging to discern between the purely aerodynamic effects of blade sweep and the additional changes in local aerodynamics due to the aeroelastic blade response, see Page 12, Line 236. We'd be happy to hear about your experience with these challenges at a given opportunity.

3. "Such values would be unrealistic on a full-scale, operational wind turbine". Can you provide typical values for an operational wind turbine?
We have added a reference to the STAR project, see Page 3, Line 67, which is the only publicly reported research project on a full-scale wind turbine with swept blades. Here, blades with 8 %R tip sweep were tested in the field. It should be noted, that this experiment was conducted on a sub-megawatt rotor with a tip radius of 28 m. In unpublished work, the authors have investigated swept blade designs for a multi-megawatt machine. From this work, it seems more realistic that swept blades would have a tip sweep below 5 %R on modern wind turbines.

4. "To maintain the same tip radius as the unswept reference blade, the swept blade axis coordinates are scaled by. . . ". I think this is not clear. You should explain what happens if you do not scale the blade axis coordinates.
Thank you for pointing out that this needs further clarification. The scaling of the blade axis coordinates is applied to ensure that both sets of blades have identical blade tip radii and, thus, also rotor disc areas. We have added an additional sentence to clarify this, see Page 3, Line 74.

5. "was mounted rigidly on a traversing system". I suppose the traversing system moves the PIV plane in a radial direction. Please add this information for clarity.
We have adjusted the text to be specify the movement direction, see Page 5, Line 109.

6. Figure 4. This figure is not explained clearly. Please explain the difference between the green shape and the blue shape. The meaning of the different line styles is explained in the figure caption. I think it would be better to explain it inside the figure with a legend.
We can see how this was not entirely clear and have made adjustments aligned with your suggestions. We hope that the new version of Figure 4 is more intuitive.

7. *Additional comment from the technical correction attachment referring to line 114 (in the original submission):* What is the implication of this distortion? A few lines before, you said that measurements are set up to have comparisons with a straight blade. Is it possible to make comparisons if the PIV plane is not aligned with the airfoil?
Upon rereading this section, we feel the two sentences at the end of this subsection add more confusion than that they are useful. We have removed them, see Page 6, Line 132. The implication of the misalignment of airfoil orientation and measurement plane is that care needs to be taken in the analysis of the derived quantities. The forces and induction terms are defined in the two directions that span the measurement plane. Thus, no special treatment of the derived values is needed here. The inflow angle and the angle of attack, however, are defined in the plane of the airfoil orientation and are, thus, a function of the global and local sweep angles, see Equation (4).

8. 122-124. It seems contradictory that you remove the induction and then you compute the induction. I suggest explaining briefly how the method works.
We have adjusted the text, see Page 7, Line 141, and hope this makes it clearer. The method only removes the local induction due to the blade cross-section. After this removal, the flow field is the sum of the freestream velocity vector and the velocities induced by the remainder of the blades and the wake. This yields the relative inflow vector and, consequently, the induction terms, inflow angle and angle of attack.

9. Do you have an explanation on why the Noca's method does not work for tangential force? Maybe it is worth to report the KJ's and Noca's methods in the article appendix and use the appendix to explain where the Noca's method fails.
The wind tunnel model runs at a fairly high tip speed ratio and has low torque and tangential force values. It is our understanding that it is very difficult to capture the change in momentum associated with this small tangential force using Noca's method. We have added this information to the text, see Page 8, Line 150. Your suggestion regarding an appendix to discuss this in more detail has already been implemented in our paper on the straight-bladed reference experiment (https://doi.org/10.5194/wes-9-1173-2024). To avoid the reporting of an essentially identical analysis, we have added another reference to this paper in the text so that an interested reader will easily find it.

10. Add a short introduction at the beginning of the Results section where you explain the content of the next subsections.
We have added an introduction to the Results section, see Page 8, Line 173.

11. "The blades used in this experiment were manually manufactured…". I suggest moving this sentence in section 2.1. The orientation of fibers plays a role in the bend-twist coupling of blades.
We agree that it makes sense to move this sentence to the Methodology section. It is now placed at Page 4, Line 85.

12. Explain if this was controlled in the manufacturing process and if you expect it to influence the results.
We could indeed see minor changes in the fibre orientation which were cause by the resin pushing through the layup during the infusion process. We have added a statement along those lines, see Page 8, Line 175.

13. 152-158. Please explain why you correct the airfoil to align it to the illuminated cross section.
The illuminated cross-section represents the blade shape during operation. The overlaid red shape is the original design and, thus, the expected airfoil orientation. The green airfoil shape is obtained by applying a rotation until it better aligns with the illuminated cross-section. The rotation angle between the red and green shapes then corresponds to a deviation from the original blade twist distribution and pitch angle. We have made changes to the paragraphs you indicate and hope that this clarifies our motivation for this approach. Maybe we misunderstand where the confusion came from. If so, we would be happy to answer additional comments.

14. 163-164. “All three blades exhibit twisting behaviour…”. This is measured by the variation of DeltaBeta between at the blade tip and root. DeltaBeta at r/R = 0 is instead the blade pitch offset (right?)
    Yes, this is indeed correct. We tried to express this with the term $\Delta\beta_{tip-root}$, but we can see that we can still be more precise. We have altered this term to $\Delta\beta(r = R) - \Delta\beta(r = 0)$, see Page 10, Line 200.

15. Table 3. I suggest putting the equations in the text and avoid the use of a table.
    We have replaced the table with equations, see Page 10, Line 210.

16. “Non-dimensionalized”. How did you normalize measurements? Can you recall how you computed Vrel?
    The velocity fields are non-dimensionalised using the local relative inflow velocity V_{rel} for which we have now added the equation here, rather than later in the text, as was the case in the original manuscript, see Page 10, Line 214.

17. Figure 7. I suggest to add in the figure a line for reference straight blades.
    We would like to refrain from including the data from the straight-bladed campaign here for two reasons. Firstly, we see the experiment with swept blades as an independent study and we want to analyse the data of both experiments individually before drawing any comparisons in potential future work. Secondly, the straight blades also suffered from pitch offsets and minor twist deformations. Thus, a fair comparison of the straight and swept blade data cannot be ensured without further analyses that we consider outside the scope of this paper. We hope you can agree with our line of argumentation to leave this figure as is.

18. “where the twist deformations vary strongly” and where measurements are more uncertain due to the small dimensions of the airfoil?
    We don’t think the small dimension of the airfoil necessarily introduces higher uncertainties. If that would be the case, this increased uncertainty would also show up in the error bars. We hope you can agree with our decision to not alter this section of the manuscript.

19. 209-210. “This corresponds to the forces in the coordinate system spanning the measurement planes…”. Add a reference to Fig. 4 if you think it’s useful
    Thank you for this suggestion. We have added the reference, see Page 14, Line 256.

Next to these specific comments, the technical corrections suggested in the attachment to your comment have been included in the manuscript. Thank you for the precise and detailed review!

Reviewer 2

1. Overall, I found section 2.2 confusing. It gives a lot of information very densely and it is hard to understand the experimental setup properties. First, as other referee remarked, figure 4 is nor properly explained. Furthermore, I find the perspective of the laser sheet from figure 3 ambiguous, and it does not allow to see the field of view extend and direction.
   In line with your comment and that of reviewer 1, we have made adjustments to Figure 4, which hopefully improve its clarity. We have also added a short explanation of the updated figure, see Page 6, Line 123. Regarding the perspective of the laser sheet, we have added a clarification of its orientation to the caption of Figure 3 to avoid any confusion.

2. I understand the difficulty in explaining the very large amount of SPIV planes covered, but in its present state the reader requires some time to understand them (for example, the so-called blade 1 was tested in 22 planes while 2 and 3 were tested in only 4). The authors may consider adding an extra table better detailing such measurements. I also propose that the laser planes are defined in terms of fixed cartesian coordinates instead of relatively to the blades.
   Thank you for this suggestion. We have added a table more clearly defining the measurement planes in Appendix B and a reference to this appendix in the text, see Page 5, Line 117.

3. Connected to my previous comment, if the authors have a film of the experimental setup running, that can bed added to the public dataset, may help to support section 2.2.

Unfortunately, we do not have a video of the setup running with the laser sheet visible. We will, however, check whether any other media material we collected could help in the understanding of the experimental setup and upload it to the dataset.

4. I also agree with another reviewer about adding to the manuscript which parameters from equation 1 correspond to a realistic shape. While I understand the authors plan to do a further manuscript in this topic, the present work would greatly benefit from testing the BEM correction model for swept blades (Fritz et al. (2022)) to test the sensibility of the lift coefficient from the blade to the parameters from equation 1.
Regarding the realistic values, please refer to our answer to comment 3 of reviewer 1. Regarding testing the sensibility of the lift coefficient, I am not sure I fully understand your comment. In case you mean a sensitivity study of how blade aerodynamics change with different sweep parameters, I would like to refer your to the original publication of the sweep correction model (Fritz et al. (2022)), where we simulated different swept blade configurations. In case you meant a validation of the sweep correction model using the experimental data: We have submitted a paper for the Torque 2024 conference dealing with this, which I hope would then answer your comment. In either case, we think it is best to retain a single focus for the present paper (the presentation of the experimental data) instead of mixing experimental and numerical work. We hope you can agree with this opinion.

5. Related to the last comment, the authors say in line 147: 'Experience from previous experiments taught that the stiffness properties of the three blades can vary considerably'. How much they change? Furthermore, figures 10 and 11 show significant differences in terms of performance between blades. A sensitivity analysis would help to see if this is indeed due to the manufacturing of the current blades or an actual limitation for the application of swept blades in wind energy.
Upon rereading this section, we realise that the sentence you refer to is misleading. We never actually measured stiffness properties and can, therefore, also not quantify their change. What we intended to say was that also with the straight-bladed experiment, differences in torsion deformation were observed. Looking at this section now, we see that referring back to the straight-bladed experiment has little added value since we focus on the pitch and twist offsets of the swept blades here. To avoid confusion, we have decided to remove this sentence, see Page 9, Line 181.
Regarding the second part of your comment: Twist deformations are not a limitation of swept blades but their motivation. The idea is that blade sweep couples bending and torsion deformations and, as such, can serve as a passive load alleviation technique. In the context of this experiment, we wanted to avoid these deformations since we were aiming to isolate aerodynamic rather than aeroelastic effects. In my opinion, a sensitivity analysis would require manufacturing a larger number of blades than the three we have now and then doing a dedicated analysis of their structural properties. While this would be a valuable exercise and would help to produce more consistent experimental results in the future, we consider this outside of the scope of this work.

6. 120 phase-locked images were recorded at each plane to extract the average velocity field and its standard deviation. Can the authors comment about the convergence of the fields?
As also indicated by the very narrow confidence interval present in Figures 7 – 10, the phase-locking worked very well, and the flow conditions were steady. To corroborate this, we checked the convergence of e.g. the circulation value derived from the flow field for varying numbers of PIV images used in the averaging process. The result is shown for three radial locations in Figure 0 as a function of a number of randomly selected images. It is evident that even with much smaller numbers than the total acquired 120 images, a converged result can be obtained.

7. Why figures 2 and 3 do not expand the full range of the blade (0<r/R<1)?
The blade root radius is $r_{root} = 0.06\,\mathrm{m} = 0.0667\,\mathrm{R}$ and, therefore, an extension of the blade axis, chord distribution and twist distribution as shown in those figures to $r/R = 0$ would not be realistic. I have clarified this, see Page 3, Line 74.

8. While they can be deduced from available data, the Reynolds number of the blades and rotor should be better specified.
Thanks for this comment. A similar comment was also made in the review of the paper presenting the data from the straight-bladed experiment, which was ongoing in parallel (https://doi.org/10.5194/wes-9-1173-2024l). Therefore, we have adjusted this paper in the same fashion as we did there, see the changes made to Section 3.4. This section now also

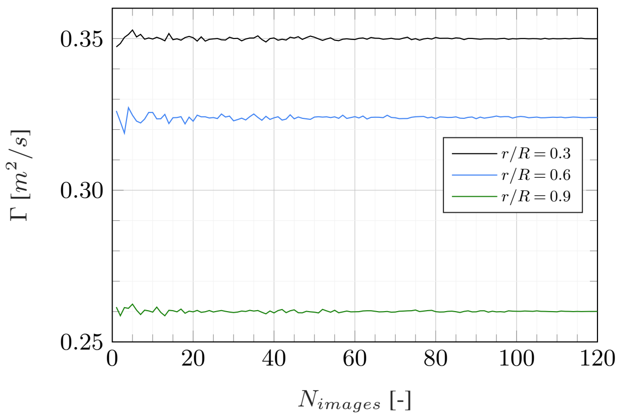

Figure 0. Convergence of the circulation as a function of the number of PIV images used for averaging

presents the chord Reynolds number distribution and an evaluation of the blade lift generation compared to the values based on the design airfoil polars.

9. In line 82, the term ’three-dimensional velocity field’ is confusing. It is the stitching process, detailed later, that allows to reconstruct three dimensional vector fields in space.
   You are right, this is misleading. Thank you for pointing this out. What we meant to say was that the stereoscopic PIV setup allows the measurement of all three spatial velocity components. We have adjusted the sentence accordingly, see Page 4, Line 96.

10. The DOI towards the dataset is the manuscript is outdated (a v1 is missing).
    The 4TU.ResearchData repository reserves multiple DOIs. Each dataset receives one DOI per version and one that always points to the latest version of this dataset. That is the one currently included in the manuscript. Since the dataset has not been altered since the initial upload, the given DOI as well as the one including ".v1" lead to the same data. Should the dataset be updated, the DOI given in this paper would lead to the newer dataset, which we consider to be preferable.

11. In figure 6, the velocity V and the relative velocity V_{rel} are not defined appropriately (the reader has to go to subsequent sections to find definitions).
    Thank you for pointing this out. We now define the relative velocity here, see Page 10, Line 214, rather than in the lift polar section.

12. Do figures 5 and 7 have error bars? It looks like they are within the markers. If that is the case, it should be mention it in the captions.
    You are right, the error bars are within the marker for most measurements (with the exception of the planes close to the blade root). Rather than mentioning this in each individual caption of Figures 7 to 10, we felt this clarification better placed at the beginning of Section 3.3, see Page 12, Line 231. We hope you agree with this assessment.

13. The phrase from line 45 ’ By basing the scaled blade geometry on the aerodynamic characteristics of the IEA 15 MW reference wind turbine (Gaertner et al., 2020), relevance for state-of-the-art wind turbine designs is ensured’, could be better sustained.
    The largest commercial turbines currently built are in the range of 14-16 MW, hence this statement. However, to underline its relevance, we have reformulated this sentence, focussing more on the importance this RWT has in the scientific community. We reference to the IEA task 47, see Page 2, Line 51, in which many research organisations study the IEA 15 MW RWT to gain better insights into its aerodynamics.

We would like to thank the reviewers for their detailed and constructive feedback. Their comments have been very helpful in improving the quality of our manuscript. Please find attached a version of our manuscript highlighting all the changes made. We look forward to hearing from you in due time regarding our submission and to responding to any further questions and comments you may have.

Sincerely,
Erik Fritz, Koen Boorsma, Carlos Ferreira