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Validation of the near-wake of a scaled X-Rotor vertical-axis wind turbine predicted by a free-wake vortex model

Adhyanth Giri Ajay, David Bensason, and Delphine De Tavernier

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The authors would like to thank the reviewers for their time, and valuable comments. Their inputs significantly improved the quality of the paper. The comments of each reviewer are addressed separately. The explanation for each question/comment is marked in blue while the actual changes in the manuscript are marked in red with the updated line numbers.

Reviewer 1

1. Comment (line 2 of the abstract): The phrase "to minimise the levelised cost of energy" is somewhat broad. If possible, please clarify in one sentence how the proposed X-Rotor specifically contributes to lowering the LCOE (e.g., through reduced manufacturing costs, improved efficiency, easier maintenance, etc.).

Thank you for highlighting it. We mentioned it in the introduction section. The X-Rotor specifically aims at reducing the capital and operational/maintenance expenditures through its design. We shall adjust the abstract to reflect this.
As part of this interest, a novel X-shaped VAWT (X-Rotor) has been proposed to minimise the levelised cost of energy by minimising capital and operational expenditures.

2. Comment (line 36, Introduction): It would be helpful to clarify whether the study by Morgan et al. (2025) also employed a BEM-based approach, and whether it included a quantitative comparison of power coefficients between the classical and X-type rotor configurations. This information would support the claim of performance benefits associated with the X-type design.

Thanks once again. Morgan et al. (2025) used a 2D Actuator cylinder approach to come to the conclusion. Their work did not focus on the X-type rotor configuration, but rather showcased the strengths of having a coned blade geometry through various inclination angles. Therefore, their study is not a standard comparison of H-type to the X-type design over the same rated power or frontal area.
Later, Morgan et al. (2025) demonstrated the power gains achieved by coned blades compared to non-coned blades for a given blade span using a 2D Actuator Cylinder approach.

3. Comment (Section 2.1): The manuscript mentions that the scaled model was derived from a large turbine design. Please clarify whether the scaling was purely geometric, or if similarity parameters (e.g., Reynolds number, tip-speed ratio) were also considered in the design process. Additionally, it would be useful to specify whether the blades were clean or if any flow tripping devices were used to promote separation. Finally, please provide details on the blade mounting configuration — specifically, was the mounting point located at the quarter-chord (c/4) position from the leading edge?

The scaling was purely geometric based on the diameter. The blades were clean and did not have any taper or any vortex generators. The chord-mounting point was c/2. In the paragraph below, please find the bold texts to highlight the changes we made. The paragraph is also modified to account for the comments from Reviewer #2 as well.

The test geometry is a **purely** geometrically scaled model of the full-size primary rotor, reduced by a factor of $\frac{1}{100}$. The top and bottom blades have a tip diameter of $D = 1.5$ m and cone angles of 30° and 50°, respectively, resulting in upper and lower spans of 1 m and 0.65 m, respectively. Aside from scaling, the primary difference between the full-scale turbine and the scaled model is that the latter consists of four straight NACA0021 airfoils with a constant chord of $c = 0.075$ m, attached to a stiff crossbeam of length 0.5 m with the same profile and chord. **The blades are clean, without any vortex generators and are mounted at $c/2$.** The rotor is supported by a tower with a diameter of 0.06 m. The model operates at a constant tip-speed ratio $\lambda = 4.0$ **at U_∞ = 4 m/s**, yielding a chord-based Reynolds number of $Re_c = 8.1 \times 10^4$ at the tip. **The operating conditions are determined to obtain a thrust coefficient to be as close as possible to the optimal value of $C_T = 0.7$, without compromising the structural integrity of the rotor.**

4. Comment (line 153): The authors state that the simulations were run for 10 revolutions to ensure convergence. Please clarify whether 10 revolutions represent the total simulated time. Is this duration sufficient to obtain both converged blade loads and a fully developed wake structure?

We present to you a plot on the convergence of the power and thrust with respect to the number of revolutions in Figure 1. Beyond the 8th revolution, the errors begin to oscillate between the 3rd and the 4th order of precision. This, in our opinion, is sufficient enough to capture the lower order effects observed in this study, as we are not modelling turbulence. This has been added to Appendix A, as a short subsection.

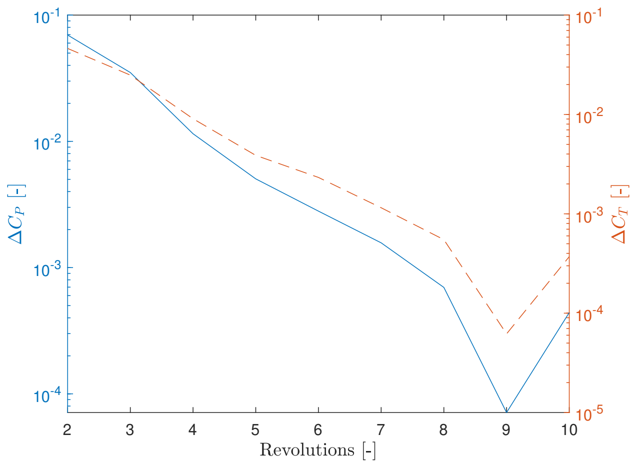

Figure 1: Convergence plot of power and thrust. The Y-axes are in logarithmic scale

5. Comment (starting around line 167): I am not sure whether the figure numbering is consistent starting from line 167. The paragraph beginning at line 162 discusses Figs. 5 and 6, but, if I'm reading correctly, there appears to be no reference to Fig. 7, and the text jumps directly to Fig. 9. Moreover, Fig. 9 already refers to the case with pitched blades, suggesting that an intermediate figure (possibly Fig. 7 or 8) might be missing or misnumbered.

Thank you for pointing it out. We have now fixed the Figure numbers. You can find the reflected changes in the marked up version of the manuscript.

Overall, the velocity predictions within the rotor volume show good agreement with the experimental results, as observed in **Figure 5**, except for the wake of the tower . . .

A similar trend is observed in the vortex structures (**Figure 6**), . . .

6. Comment (around line 175): The authors rightly note that the numerical approach fails in certain regions. I believe two factors may contribute to this discrepancy: the accuracy of the airfoil characteristics at such low Reynolds numbers, and potential limitations of the dynamic model used. A brief discussion of these aspects would strengthen the interpretation of the results.

Thanks for your suggestions. We did consider the airfoil characteristics at low Reynolds numbers and mentioned it in the conclusions, however neglected to highlight it as an obvious answer to all our validation cases present here. We already referred to the Melani et al. (2019) while choosing our polars, citing significant variation even at the Reynolds number of our choice.
With regard to the dynamic stall model, we could not compare multiple dynamic stall models to be able to scientifically address the limitations of using the Leishman-Beddoes model, specifically for the X-Rotor. While we did try to run a case with the Boeing-Vertol model (pre-packaged with CACTUS) it resulted in diverging solutions and we did not present them here.
Notably, as highlighted in Section 3.2, the difference could also arise from the polars due to the inconsistency shown between experimental and numerical lift coefficient profiles. This holds true for the rest of the results discussed in this study.

Reviewer 2

1. There is significant uncertainty in the polar data and overall aerodynamic modeling, particularly in both pre- and post-stall regimes. Assuming similar airfoil behavior at Re = 80k and 150k is a strong simplification, especially considering the chosen airfoil and the experimental turbulence intensity. Using XFOIL with Ncrit = 8 is also questionable, as the experimental TI suggests a lower Ncrit ≈ 4. This likely introduces notable errors in force predictions and may account for discrepancies with the experiments such as the reduced wake expansion. Additionally, the treatment of post-stall behavior is unclear—was any extrapolation method used?

Thank you for your very thorough insight into the paper. We shall address your comment in reverse order of your comments, as each point is dependent on the other. We performed a Viterna extrapolation Viterna and Janetzke (1982) to obtain post-stall behaviour of the polars. We shall mention this in the numerical setup section. Regarding the chosen value of Ncrit, the reviewer is indeed correct that using an Ncrit = 4 would be more suitable based on the experimental TI. This was a critical oversight on our part. We further agree with the reviewer that performing the simulations at Re = 80k as opposed to Re = 150k would improve the reliability of the results, especially with the updated Ncrit value. Therefore, we have repeated our study with Re = 80k and an Ncrit = 4 for all the pitch cases. After a very thorough inspection, we noticed that the results do not change significantly at all - often remaining extremely similar to our previous result. This is attributed to the very close agreement between the load profiles generated by using the two polars (see Figure 5 and Figure 3 below). Therefore, there appears to be no significant changes to our observation throughout this study, however we have used a more reasonable polar for the validation.

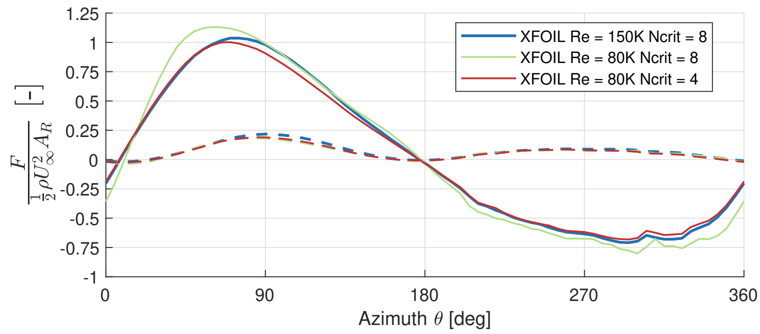

Figure 2: Integrated blade forces F as a function of azimuth θ. The solid lines represent the normal force and tangential force is represented by the dashed lines.

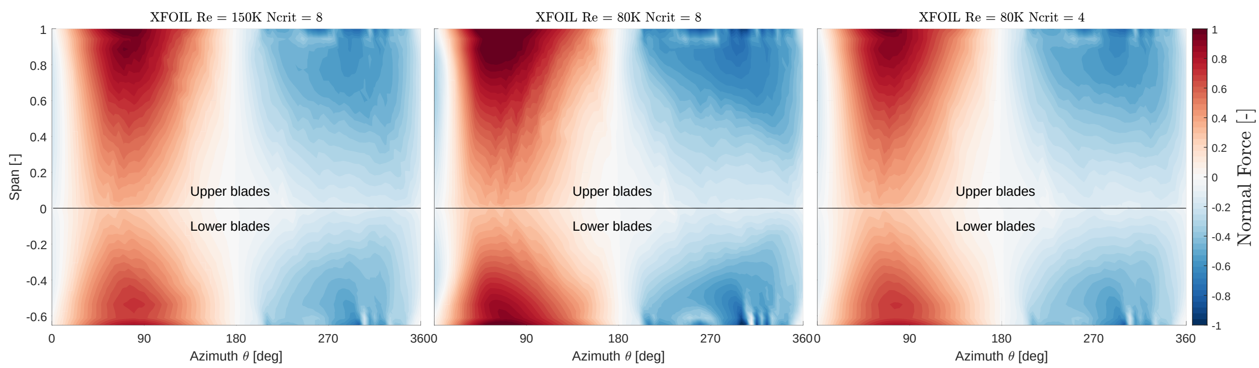

Figure 3: Spanwise distribution of normal blade forces F_N as a function of azimuth θ.

2. Flow curvature, which becomes significant for blade sections with chord-to-radius ratios $c/R > 0.1$, is ignored in the simulations. This is a critical omission, given that most of the blade operates beyond this limit and flow curvature has a strong effect on the lift characteristics at low angles of attack

Yes, the flow curvature model is more pertinent for the X-Rotor than the typical H-Darrieus VAWT. This is because in the X-Rotor, as the reviewer pointed out, most of the blade does operates beyond this limit (especially near the root). While we would definitely have used the flow curvature correction for typical VAWTs with similar chord-to-radius ratios, we omitted it in this study for two critical reasons.
Primarily, as mentioned in the methodology, it would become exceedingly difficult to isolate the differences observed between the predicted values from CACTUS and the experiments with the flow-curvature on for the X-Rotor geometry. Historically the flow curvature has been shown to increase the angle of attack in the upwind half and lowering it in the downwind half (Goude, 2012). This has not yet been verified for the X-Rotor geometry, with a prominent spanwise variation of relative velocity.
Secondly, the flow-curvature model is not built into CACTUS. Any attempts of modifying the source code of the solver is outside the scope of this manuscript. Furthermore, attempting to pre-emptively correct the polars without calculating the relative velocities would invite unwarranted errors in the free-wake vortex model. We believe including the correction is essential, but is not crucial in the context of validation at these low Reynolds number regimes, as the uncertainty of the polars would be the primary point of contention. We have expanded on this in Section 3.3.
Unrelated to this study, in our on-going validation study between an actuator line method (turbinesFoam) and experiments for the X-Rotor geometry, we performed a sensitivity study between the regularisation kernel (ϵ), dynamic stall, and flow curvature (see Figure 4 below). While we definitely need to the tune the inputs more to match the velocity values, we can see that the flow curvature model causes the wake shape to deviate away from the experiments. Without the flow curvature, we can see the wake shape predicted is closer to the experiments. So indeed, modelling the flow curvature is critical. But, its limitations for this geometry are unclear at present as it seems to have a detrimental effect towards the accuracy of the wake. However, due to the limitations of CACTUS, we are unable to provide any scientific insight into the effect of flow curvature in this study.

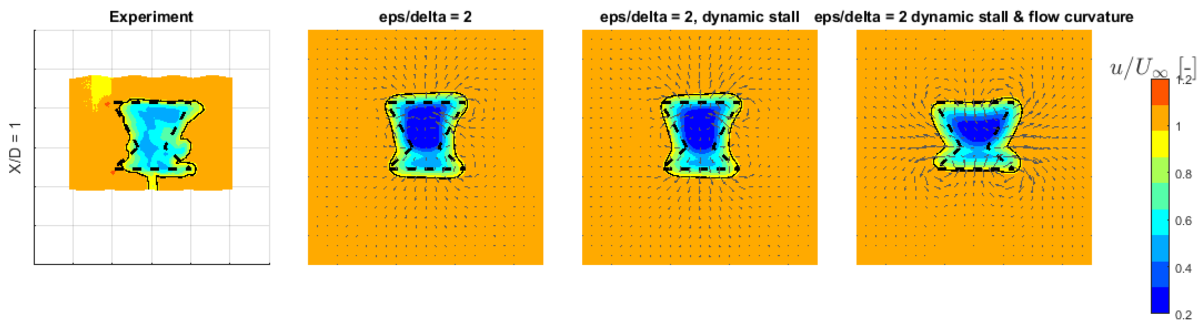

Figure 4: Time-averaged normalised streamwise velocity components for a 1:250th scaled X-Rotor geometry at X/D = 1.

While this provides a more accurate depiction of airfoil motion in VAWTs, we opt not to include it here for two critical reasons. Primarily, implementing the flow-curvature model (such as Goude (2012)) to a rotor geometry with a spanwise relative velocity distribution would make it exceedingly difficult to isolate the differences observed between CACTUS and experimental results to other factors. Secondly, as CACTUS does not come inherently with a flow curvature correction model, implementing it ourself in the source code would be outside the scope of this study. Furthermore, attempting to pre-emptively correct the airfoil polars without calculating the relative velocities would result in unwarranted errors in CACTUS. We believe this model is essential, but would bring uncertainty to the results when the behaviour of the rotor with and without flow-curvature has not been tested for this geometry.

3. The chosen core radius for the LLFVW simulations appears excessively large. While the authors justify this choice on the grounds of numerical stability, this compromises the fidelity of tip vortex resolution—a crucial aspect of wake recovery. It is difficult to draw reliable conclusions about the wake behavior when tip vortex dynamics are under-resolved.

Thank you once again for your critical points. The vortex core size, in principle, indeed has a strong effect on the resolution of the tip-vortices. However, we invite you to consider the updated Figures A2 and A3 in Appendix A (the updated figures presented below). The update was to introduce further analysis on the core radius sensitivity. From here, we can clearly observe that a 50% drop in the vortex core size has a very minimal impact on the load profile. A further reduction in core-vortex size results again in very minor impact in the downwind half of the rotor. This is also observed in the spanwise variation of loads with more noticeable instability of loads at the tips. If the tip-vortices are not resolved, then there would be a considerable difference in the loads near the blade tips - which is not observed here between the updated simulations and the simulations with the 50% and 10% bound and trailing vortices. Therefore, as there is no discernable difference in the loads between the different core sizes in our study, we believe our choice of vortex core size does not affect the wake characteristics. We have also updated figure A1 to exclude the tip-vortex sensitivity (as it is better explained with forces), while retaining the sensitivity to dynamic stall and increased C_L and C_D. Furthermore, Appendix A has undergone significant modifications to reflect on both this comment as well as comment #1 from Reviewer #2. You can find the detailed changes in the marked-up document.

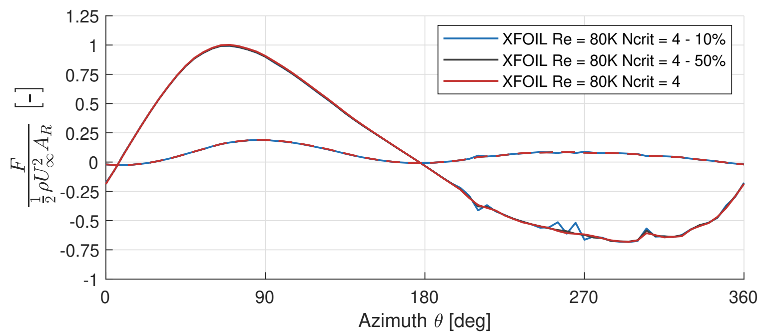

Figure 5: Integrated blade forces F as a function of azimuth θ to show sensitivity to vortex core sizes. The solid lines represent the normal force and tangential force is represented by the dashed lines. Positive normal force is away from the axis of rotation and vice versa. Forces are integrated along both upper and lower halves of B1.

4. The introduction is a bit vague, especially concerning the technological background of the work. What is the idea behind the X-Rotor and what role is it supposed to play in the future offshore wind industry compared to traditional HAWTs or VAWTs? Why is it advantageous to further develop numerical tools to simulate complex shape VAWTs?

The idea behind the X-Rotor is to lower the levelised cost of energy (LCoE) by primarily targeting reduction in capital and maintenance/operation expenditures (CapEx and OpEx) compared to HAWTs. As all the energy generation is done through the secondary rotors, there is no need of heavy gearboxes. Additionally, with the cone angle, it uses less material per surface area compared to other traditional VAWTs which reduces CapEx significantly. Regarding the numerical tools, as the rotor is inherently three-dimensional due to its coned blades, it would be misrepresentative to not account for this using traditional 2D tools such as the 2D Actuator cylinder or the Double Multiple Streamtube. This conclusion is drawn from our earlier work Giri Ajay et al. (2024). To further clarify, the numerical tool CACTUS is not developed by us specifically for this turbine. It is merely used to understand the validity of the tool when compared against scaled experiments. We have now elaborated these points in the introduction section.

line 28-30: which substantially reduces the turbine's capital **expenditures (CapEx)**. Additionally, the low altitude and reduced mass of the generators eliminate the need for jack-up vessels for maintenance, potentially lowering operations and maintenance expenses **(OpEx)**.
line 33-34: demonstrated significant savings in the **CapEx and OpEx** compared to a HAWT.
line 35-36: Furthermore, the coned angles of the X-Rotor blade allows it to use less material per surface area compared to other traditional VAWTs, significantly reducing CapEx costs compared to VAWTs. line 46-47: Therefore, it is essential to use models that are able to capture the 3D aerodynamics of the X-Rotor geometry to be able to predict the aerodynamic behaviour accurately.

5. Section 2.1: in this case, the strategy used to scale the X-Rotor to wind tunnel size plays a crucial role in the soundness of the study. Please provide additional details on this.
The X-Rotor was scaled down purely geometrically. The operating condition of $U_\infty = 4$ m/s and $\lambda = 4$ were defined to be as close as possible to the optimum thrust coefficient of $C_T = 0.7$, within the structural limits of the setup. This resulted in an operating thrust of $C_T = 0.5$. We have clarified this in Section 2.1, also based on one of the comments from Reviewer #1. You can find the adjusted text in the marked-up version or in Comment #2 of Reviewer #1
The operating conditions are determined to obtain a thrust coefficient to be as close as possible to the optimal value of $C_T = 0.7$, without compromising the structural integrity of the rotor.

6. Flow curvature effects do not come from time-varying inflow, as stated by the authors, rather from the variation of the angle of attack along the blade chord, even in quasi-steady conditions, i.e., in absence of significant dynamic effects.
The reviewer is correct. We meant to imply spatially-varying inflow (i.e., over the chord) rather than time-varying inflow. We have now clarified the sentence to be quite explicit.
This pitching component leads to a variation in the inflow (by extension a variation in angle of attack) along the chord of the blades, which introduces flow curvature effects . . .

7. Figures 6 and 9 are difficult to read due to the figure dimensions and adopted colorscale. It is recommended to switch to higher contrast colorscale or make a selection and show fewer 2D views on a bigger scale.
Thank you. We think all planes are required to correlate the flowfield observed in Figures 5 and 8 correspondingly, therefore we have now adjusted the x-axis spacing for Figures 6 and 9 alone to accommodate larger figures. Additionally, we also increased the colour-scale to be between -10 and 10 now, to artificially increase the contrast in the figures. Any higher leads to essential vortex structures being lost from the experimental results.
Figures 6 and 9 have been magnified by 25%.

Other adjustments

We have also corrected some typos and sentence structure in parts of the manuscript that can be observed in the marked-up version of the manuscript.

References

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