Dear Alessandro Croce and Sandrine Aubrun,

We have the pleasure of submitting our revised paper “A scaling methodology for the Hybrid-Lambda Rotor - Characterization and validation in wind tunnel experiments” (wes-2024-168) for consideration in the journal Wind Energy Science.

We are very grateful for the constructive feedback with lots of valuable suggestions from the editorial team and the reviewers which helped to improve our paper. In short, we want to highlight the major changes and additions:

- We structured chapter 2 “Scaling methodology of the Hybrid-Lambda Rotor” with multiple sub-section-headings. The order now follows the design work-flow.
- We revised Fig. 1 (Design Flow-Chart), added a legend and introduced all variables.
- We added contour lines to the wind speed hot-wire measurements (Fig. 13) to highlight the outer annulus with reduced wake deficits.
- We clarified the explanation of the control schedule.

Furthermore, we have made all the necessary requested changes and have addressed all comments of the reviewers (printed in black) in the detailed response below.

Our responses to the referees are written in green.

Reformulated or added phrases for the revised manuscript are cited with blue fonts.

Line, figure and table numbers in our answers are according to the revised manuscript. Line, figure and table numbers in the referees’ comments are according to the initial manuscript. All updated figures are appended to this authors’ response.

We feel that based on the reviewers comments our paper has been sharpened and improved, especially in terms of clarity, presentation quality and additional considerations, and now meets the required standards to be published. If any responses are unclear, or if you wish for additional changes, please let us know.

Sincerely,

Daniel Ribnitzky

- On behalf of all authors –

Referee 1:

Dear Authors, I have reviewed your article and provided my comments below.

General comments

The topic of the article, which focuses on the design and experimental characterization of a scale model of a low specific-rating rotor, is significant for the research community and aligns with the scope of WES. The methodologies outlined in the article are valuable for the design of scale model wind turbines, and the results obtained from the testing campaign enhance the understanding of the operational characteristics of low specific-rating rotors. This study can contribute to their further development and commercial viability.

The research objectives and hypotheses are clearly defined. The discussion of the methodology and results is supported by sufficient detail. The article is generally well-structured.

For these reasons, I believe the article merits publication in the WES journal. Prior to publication, I request the authors address the comments below. Specific suggestions to enhance the effectiveness of the article are included in the "specific comments" section. Technical corrections, including typos and improvement suggestions for presentation quality, are provided in the attached PDF.

We appreciate the detailed comments and would like to thank the referee for the positive feedback.

Specific comments

Introduction. I recommend that the authors include a discussion on the potential impact of the article, particularly in relation to the development of low-specific-rating rotors and the scale model testing of wind turbines in general.

We thank the referee for this suggestion and added a paragraph about the impact of the paper:

The study presented here, gives valuable insights on aerodynamic effects and design methods for low-specific-rating wind turbines. This includes the understanding of the flow in the rotor area as well as in the near-wake of rotors with non-uniform axial induction distribution. Further, the study can serve as an example for various complex scaling problems in wind energy research and provides guidelines and inspirations on designing scaled model turbine blades for aerodynamic investigations in the wind tunnel.

Scaling methodology of the Hybrid-Lambda rotor. This section is hard to follow as scaling requirements are introduced gradually. I recommend restructuring it into: 1) Scaling constraints, 2) Scale factors, 3) Blade design algorithm, 4) Results of the blade design. Essentially, divide it into inputs, methodology, and outputs.

We improved the readability and understandability of this section by adding headings for the subsections. We further moved the paragraph about the choice of airfoils to be included in the inputs section. The added headings are:

2.1 Geometric scaling vs. aerodynamic redesign
2.2 Scaling objectives
2.3 Adaptation of design TSRs
2.4 Design and scaling constraints
2.5 Inputs to the aerodynamic redesign
2.6 Deriving the control schedule and time scaling factor
2.7 Verifying the constraints and closing the design loop

By doing so, we follow the chronological order within our design workflow which aligns well with the flowchart in Fig. 1. Starting with theory and background knowledge, moving on to the scaling objectives which form the basis for the adaptation of the design TSRs. We then introduce the design flowchart and explain the scaling constraints followed by the design inputs. Continuing, we can derive the control schedule and only with that being calculated, we can define the time scaling factor. Finally, the constraints are verified and the design loop is closed.

Figure 1. The diagram is hard to follow due to undefined variables. It should be simplified and the comparison with MoWiTO1.8 might be removed.

We explained all abbreviations and variables that are used in Fig. 2 in the caption and sorted them alphabetically. We further corrected for the mistake that we used β as a variable for both, twist and pitch. The latter is now abbreviated with β_{pitch} throughout the whole paper. We added a legend to the flowchart, explaining the shapes of the symbols.

11: “both rotors”. It's not clear if the two rotors mentioned here are the two scale models (Hybrid-Lambda and conventional) or the scale model of the Hybrid-Lambda and its full scale version.

We clarified this by adding the adjective “scaled”:

The measurement data is supplemented with free-vortex-wake simulations of both scaled rotors.

62: “the large geometric scaling factor”. Can you recall its value here?

We added the exact scaling factor:

First, the large geometric scaling factor of 1/181, second [...]

71-76: this paragraph is a bit disconnected from the rest of the introduction. Consider to remove it and merge its content to the rest of the text.

With this paragraph we aim on addressing the impact of this study, as you also kindly recommended in line 51 of this author response letter. We merged the content with the additional description of the potential impact.

140-141: This sentence should clearly connect with Eq. 1-4, as it forms the basis of the methodology described in the article.

We added a sentence to the beginning of the paragraph to explain the purpose of Eq. 1 to 4:

By using Eq. 1-4, we will explain why a reduction in design TSR will increase the Reynolds number.

161: “fulfil the constraints … number”. Can you recall the constrains, i.e. the minimum allowed chord and Re?

The constraints are introduced in the next subsection, so we followed your advice to recall the exact numbers up front.

We found that a design TSR for the light-wind mode works well in order to fulfil the constraints of minimal chord length (3 cm) and Reynolds number (70000).

169-171: “Second, … in the wind tunnel”. Please clarify with examples or explain in more detail.

We reformulated and added an explanation:

Second, the transition from the light-wind to the strong-wind mode at constant rotational speed will expand over a wider range of wind speeds, if the design TSRs are further apart. […] This opens up more opportunities for control-related investigations in the wind tunnel that focus on the transition between the operating modes.

192: “first tower eigenfrequency”. Report the value.

We added the value:

It is constrained by the first tower eigenfrequency (6.7 Hz) at the lower end, […]

210-211. Can you briefly explain how the control strategy works?

We added an explanation on how the control strategy is derived:

Now, the constraint of flapwise RBM is applied and the wind speed is calculated at which the limiting loads are reached (u_{ts}) which is defined as the start of the transition region. The control strategy, i.e. rotational speed and pitch angle over wind speed, as shown in Fig. 4 for the final scaled design, is derived as follows. For wind speeds greater than u_{ts} the rotational speed is kept constant until λ_{sw} is reached. Then the rotational speed follows λ_{sw} until rated wind speed. The pitch angle is set in order to constrain the RBM below rated wind speed with the given operational TSRs.

215-216. This statement is unclear. There is only one time scale. It can be stated that the maximum rotor speed is not scaled in relation to the maximum rotor speed of the full-scale turbine.

We clarified this and reformulated:

Now, the wind speed at which the transition to the strong-wind mode starts, u_{ts}, and when it ends, u_{te}, are known, as well as the respective pitch angles. Since most of the measurements are performed at those two wind speeds and with the respective rotational speed, ω_{tran}, we define the time scaling factor as […]. Note, that the maximum rotational speed of 600 rpm is set by hardware constraints and is not exactly true to scale compared with the full-scale model.

226-228. To provide clear guidance, it is recommended to avoid asking open-ended questions and instead offer definitive answers.

We added a sentence to emphasise that those questions form the design check at the end of the iteration process. Additionally, we added to each question a reference to the respective figure in order to provide the answer to the questions.

[…] and we can verify the scaling objectives as follows. Is the axial induction distribution from the full-scale rotor reasonably matched (Fig. 6a)? Is the change in the angle of attack distribution reasonable when switching the operating modes (Fig. 6b)? Is the constraint for the Reynolds number achieved (see Fig. 5)? If one of these objectives is not met, the design optimization loop is closed by re-tuning the input parameters and calculating the next design iteration.

Figure 6. Can you add the constrains to the plots with horizontal lines?

The objective for the axial induction is to match the spanwise distribution from the full-scale model in the two operating modes. Consequently, there is no constraint. For the angle of attack, it is not possible to match the distribution exactly, since the low Reynolds number airfoils operate at lower angles of attack. However, the characteristic tilting when switching between the operating modes should be replicated. The constraint would be the stall angle. However, this constraint is not active, since we operate at low angles of attack.

288: “Here”. In this study or a cited one?

The statement is valid for the cited reference and for our study. We replaced “here” with:

According to this method, the axial and tangential velocity components are probed in the bisectrix of two blades […].

394-395: “as we aimed … blade design”. How does this goal influence the tuning of viscous diffusion in OLAF? Please explain.

By reducing the core spread eddy viscosity, we can simulate a wake that convects freely for multiple diameters downstream. The results give a better representation of the impact of the blade design, because it is not overshadowed by viscous diffusion. We reformulated:

We aimed for a representation of an undisturbed wake, focusing on the effects that result from the aerodynamic blade design. Consequently, no tower shadow model is used and the core spread eddy viscosity (viscous diffusion parameter) is set to 100, in order to allow for a free convection of the wake for multiple diameters downstream distance.

409: “only a small portion of the results”. How did you choose which results to plot and which to discard?

We chose to plot the pitch angle for the highest power coefficient and additionally an equidistant spacing of pitch angles, covering the entire measurement matrix. For each pitch angle always the entire range of TSRs is plotted.

415: “with a considerable offset”. Could this offset be due to thrust being estimated from the tower base bending moment and requiring corrections for nacelle and tower drag, making it less certain than the torque (power) measurement?

We explain the correction model for the aerodynamic tower drag in Sect. 3.3. These correction models are already applied to the data, however the offset presented in Fig. 8b remains. We added a note about the correction model to the results section:

Although correction models for the tower drag are applied, as explained in Sect. 3.3, an offset in the thrust coefficient of about 13% is present for the operating point with maximal c_p.

Eq 14: “My”. Could you please clarify what "My" refers to? Is it the bending moment of individual blades that have been measured using strain gauges?

Yes, this is correct. We missed to introduce this variable. Thank you for pointing us at this. We added:

The measured flapwise RBM (M_y) are in good agreement with the simulations.

420: “the measured flapwise RBM are in good agreement with the simulations”. Have you estimated rotor thrust from blade-root bending moment? It might compare better with simulations than the tower base moment estimate.

We tried the proposed method without seeing major improvements. To do so, we need to know the lever arm of the resulting bending force on the blade. To estimate the lever arm, we used the LDA measurements of the axial induction distribution, but those are only available for the LW and SW operating modes (i.e. two combinations of TSR and pitch). A further drawback of the LDA measurements is that not enough measurement positions are available close to the root and the blade tip and the data needs to be extrapolated. The thrust can then be calculated by either summing up the discrete axial force distribution (derived from the axial induction measurements), or by dividing the measured RBM by the derived lever arm. As an alternative, the lever arm can be derived from the BEM simulations.

In summary, the confidence in the suggested analysis is not very high, since we need to extrapolate the axial induction distribution towards the blade root and tip and we can only probe two operating points. Further, it is questionable if the openFAST simulations should be the absolute reference, since openFAST assumes independent blade elements and the effects due to the non-uniform loading are not captured. We therefore don't believe this analysis will add much clarification to the paper and decided not to include it. We clearly point out the offset in the thrust measurements in the results section, line 427-429.

436-465. This discussion should precede the LDA measurement results. Condense the FVW simulation results (e.g., using Fig. 11) to show that the BEM model's 2D flow assumption is invalid in the blending region, explaining the poor agreement between BEM and measurements there.

We added a short explanation upfront to the LDA measurement results, referring to the subsequent discussion on the 3-dimensionality of the flow:

However, the steep gradients in the blending region of the blade design are not captured by the LDA measurements. The measurements show a smeared out and less steep gradient at $\frac{r}{R} \approx 0.75$. Since the flow in the rotor plane is 3-dimensional, the assumption of independent blade elements in the BEM theory is invalid, and the gradients can not be captured by the BEM theory. The radial velocity components are further addressed in the next paragraph.

485. You should clarify that the shear layer is between the wind tunnel jet and the still air around the nozzle.

We added a clarification:

The hot-wire measurements are performed in the open jet configuration of the wind tunnel, as explained in Sect. 3.1. This means, there is a shear layer between the wind tunnel jet and the still air around the nozzle. For large distances to the wind tunnel nozzle, there is an unavoidable interaction of the wind tunnel shear layer with the wake of the turbine.

489-490: "A major advantage … operating modes". Can you relate this discussion to the control schedule shown in Fig. 4?

We inserted a reference to the control schedule:

When the wind speed increases and the rotor shifts to strong-wind mode and further to rated conditions, the pitch angle is increased as shown in the control schedule in Fig. 4. Consequently, the thrust coefficient is reduced, the wake deficits are greatly decreased (see also Fig. 14) and […].

496: "leads to an outer annulus with reduced wake deficits". Could you please highlight it in the figure by delineating the edges of the annulus?

We added contour lines to Fig. 13 and mentioned it in the text:

The low-induction design of the outer 30% of the blades leads to an outer annulus with reduced wake deficits which is highlighted in Fig. 13 with the contour lines.

504-505: “to a second shear layer at the boarder to the inner wake core with lower wind speeds” Can you highlight it in the figure?

We added arrows to Fig. 13 pointing at the second shear layer and explained it accordingly:

The outer annulus with reduced wake deficits leads to a second shear layer at the boarder to the inner wake core with lower wind speeds. This is highlighted by the black arrows in Fig. 13, showing the local turbulence intensity, which is defined [...].

504-517. Are two shear layers expected in the Hybrid-Lambda rotor based on prior numerical studies? If so, how do the measurements align with the numerical results?

Yes, the second shear layer was also observed in FVW and LES simulations of the full-scale rotor (Ribnitzky et al., 2023) and in the FVW simulations on the model turbine shown in Fig. 15. This is already addressed in line 561-566. The measurement results are further compared to the FVW simulations in line 552-557.

Figure 13. The text in this figure is very small. Please, increase it.

We increased the font size in Fig. 13.

542. Please add a sentence explaining how the assumptions of the FVW model relate to the experimental conditions. If I understood the text properly, clarify that the results from the FVW model are useful for indicating the location of vorticity but that the strength estimated by the FVW model should not be directly compared to the strength observed in the experiment.

We added a sentence to indicate why we chose a simulation set-up that leads to an undisturbed wake. However, please note that the strength of the vorticity from the FVW simulations cannot be directly compared to our measurement results. We only measured with one-dimensional hot-wires and the vorticity cannot be derived from one velocity component. This is why the turbulence intensity is shown in Fig. 13.

The FVW simulations are set up in a way to produce a wake that is as undisturbed as possible, e.g. using laminar and uniform inflow and a relatively low core spread eddy viscosity. This simulation set-up is chosen to identify a clear representation of the vortex rings and their positions. Consequently, the vortex systems do not break up noticeably and the interaction between the inner and outer ring of vortices is rather low.

Figure 15. Please add titles to the subplots to indicate if they refer to Convention or Hybrid Lambda rotors.

We added abbreviations to the subplots to clarify this.

589: “must be considered critically”. Not clear what you mean.

We adjusted the wording to specify:

[…] but when designing a blade with such strong gradients along the blade span one cannot rely solely on the BEM theory.

598: “are very similar”. Could you please clarify the parameters? Are we discussing velocity, radial extension, or another aspect?

We reformulated to clarify this:

The outer wake annulus behaves very similarly across the simulations and the measurements, considering the wind speed distribution and turbulence intensities.

624-629: I think this paragraph belongs to the conclusions.

We moved the outlook to the conclusion section, as suggested.

Data availability. Can you provide any measurements or numerical models? This is encouraged by the journal.

We added the section data availability. The link to the repository will be added during the typesetting process.

For the Hybrid-Lambda model turbine, we provide the blade geometry as CAD-file and the turbine simulation model for OpenFAST. We further provide the mean wind speeds and turbulence intensities from the hot-wire measurements in the wake as shown in Fig. 13.

Technical corrections

See attached pdf file.

We thank the reviewer for the many detailed and helpful technical suggestions and implemented the changes where applicable.

Referee 2:

Review of the manuscript WES-2024-168 “A scaling methodology for the Hybrid-Lambda Rotor -Characterization and validation in wind tunnel experiments”, by Daniel Ribnitzky, Vlaho Petrovic and Martin Kuhn.

The paper aims to develop a scaling methodology for the Hybrid-Lambda Rotor and validate it through wind tunnel experiments. The Hybrid-Lambda Rotor is designed to increase power output in light winds and limit loads on long, slender rotor blades in strong winds. The rotor concept is scaled to a wind tunnel size (1.8 meters in diameter) and tested under controlled inflow conditions. The experiments involve measuring axial induction in the rotor plane and characterizing the wake using a Laser-Doppler-Anemometer and a hot-wire rig. The results show that the rotor's low-induction design reduces load overshoots in gust events and reveals unique flow patterns, such as increased radial flow components and reduced wake deficits. The study provides valuable insights into the aerodynamic performance of the Hybrid-Lambda Rotor and its potential benefits for offshore wind turbines. The experimental data supports the effectiveness of the rotor design in both light and strong wind conditions, making it a promising concept for future wind energy applications. The manuscript is well-written, and the arguments are well presented. I do not have any major comments regarding the manuscript. Please find some specific comments below:

We thank the referee for the very positive feedback on the manuscript.

Page 7, Line 170: First, we … in the wind tunnel. Could you please also show the angle of attack redistribution for 7.5->6.7 and 7.5->6 in a plot. In addition, please report the wind speeds for LW to SW transitions for both TSR shifts.

The changes in the inflow angle for the mentioned TSR reductions (7.5 → 6.7 and 7.5 → 6) are already plotted in Fig. 2. We further added the corresponding wind speeds to the description.

For instance, with the start of the transition region at $u_{ts} = 6.3\, m\, s^{-1}$, the tip-speed ratio in the strong-wind mode of $\lambda_{d,SW} = 6.7$ would lead to the end of the transition region at $u_{te} = 7.0\, m\, s^{-1}$, whereas a lower tip-speed ratio $\lambda_{d,SW} = 6$ would result in a wider transition region with $u_{te} = 7.9\, m\, s^{-1}$.

Page 8, line 188: This is the…steady-state assumption. Could you please justify the load level of 10.6 Nm for root bending moment or add a reference?

It is derived by scaling the maximum value of the flapwise blade root bending moment from the full-scale Hybrid-Lambda turbine. We added a clarification:

In theory, the maximum flapwise blade root bending moment (RBM) scales with n_l^3. With a maximum value for steady inflow of 62.9 MNm for the full-scale model this would result in a value of 10.6 Nm for the wind tunnel model.

Page 20, line 430: the Measurements show… outer 30%. Can you please elaborate more on discrepancy between the measurements and the simulation at the outer 30%. Is the tip loss factor affecting outer 30% in the simulations?

This is correct, the tip loss correction drives down the angle of attack close to the tip. We added this information and restructured the explanation:

In close vicinity to the blade tip (0.9 < r/R < 1) the modelling of the tip loss effect leads to deviations between the measurements and the BEM simulations. The two outermost measurement points should be treated with care since the tip loss effect influences the bisectrix method by Herráez et al. (2018).

Page 25, figure 14: Please check the caption, both left and right, and (a) and (b) are used for the subfigures.

We removed “right” and “left” from the caption.

Page 26, figure 16: Please make the caption clearer for indicating top and bottom panels of the subfigures (a) and (b).

As also requested by referee 1, we added abbreviations to the figures to clarify the rotor type.

Appendix- updated figures:

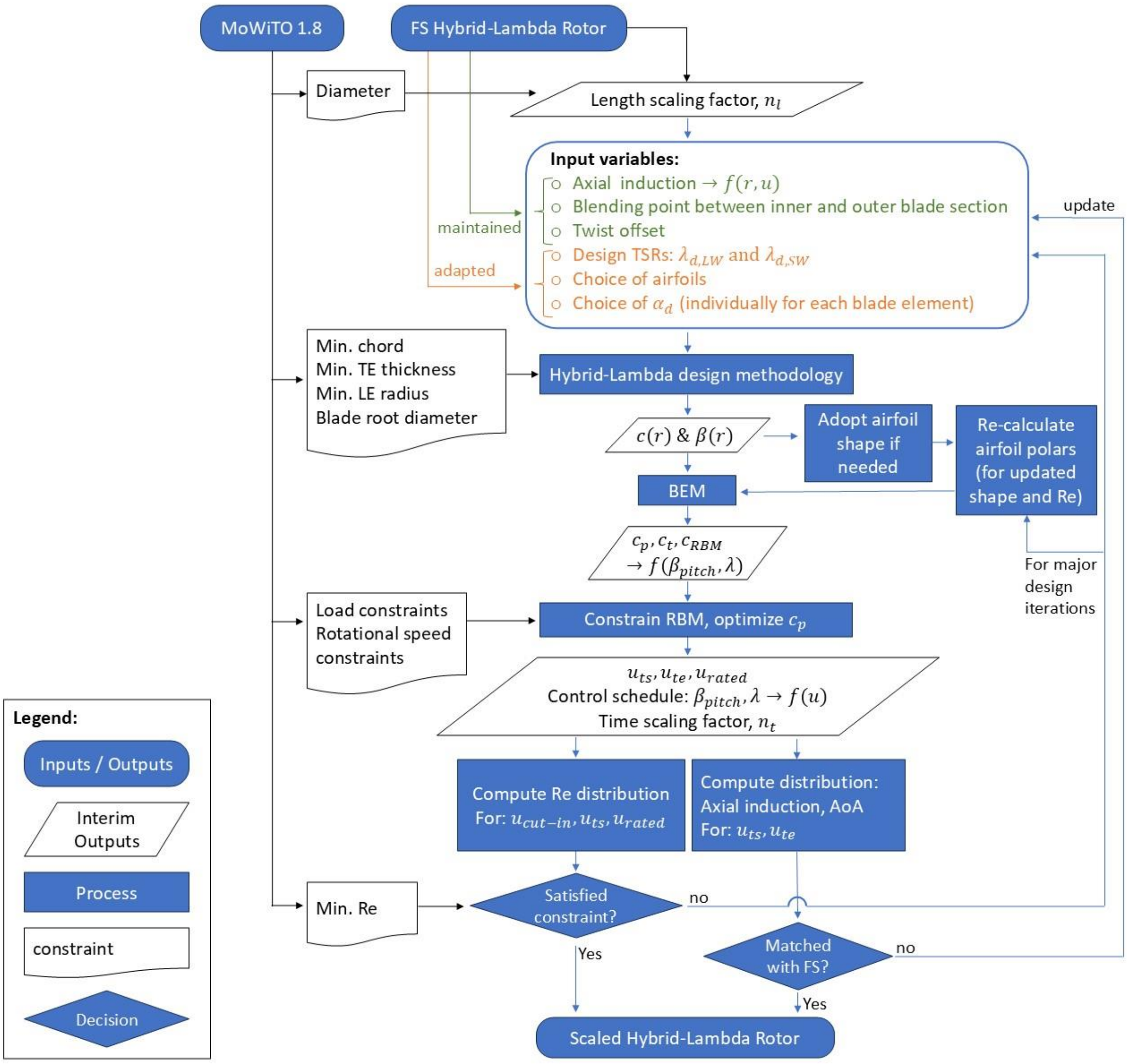

Figure 2: Scaling and design workflow for the Hybrid-Lambda Rotor. […]

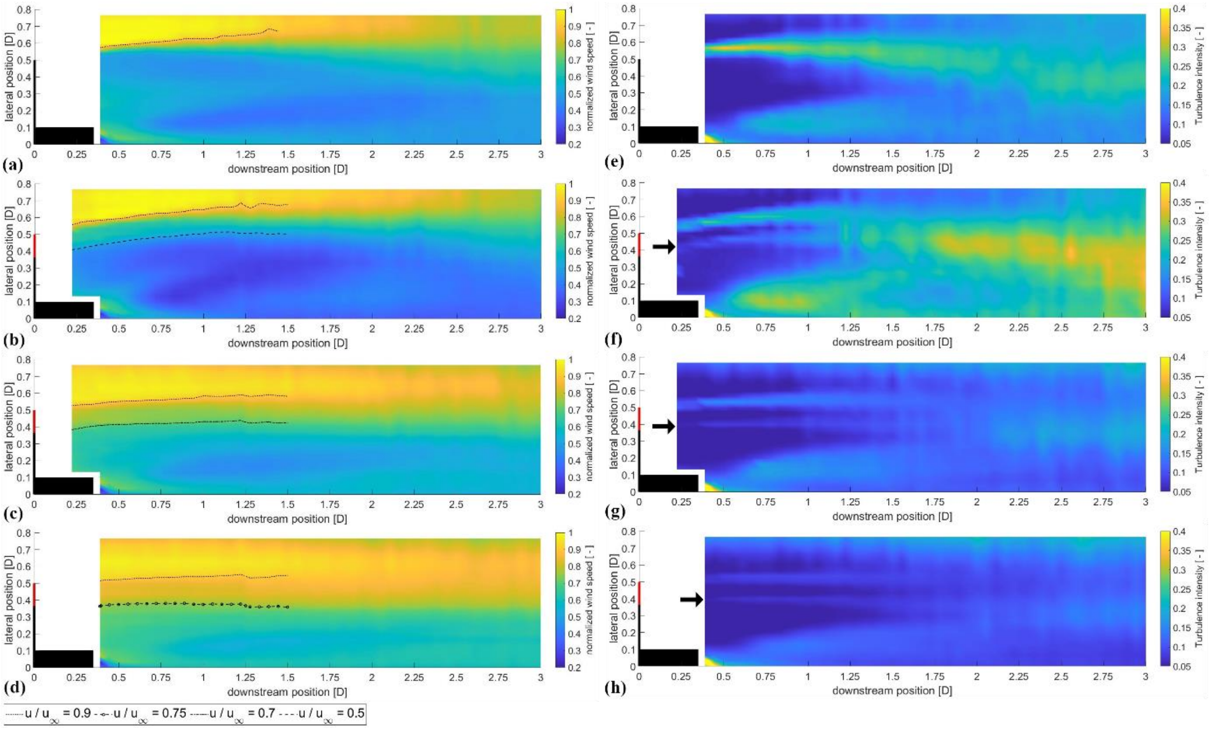

Figure 13. Mean normalized wind speed (a-d) and turbulence intensity (e-h) from hot-wire measurements in the wake of the model turbines. From top to bottom: Conventional rotor (a,e), Hybrid-Lambda Rotor in LW mode (b,f), SW mode (c,g) and at rated wind speed (d,h). Nacelle and blades are displayed in black, the outer 30% of the Hybrid-Lambda blades are displayed in red. The black arrows point at the second shear layer starting at the blade design blending region.

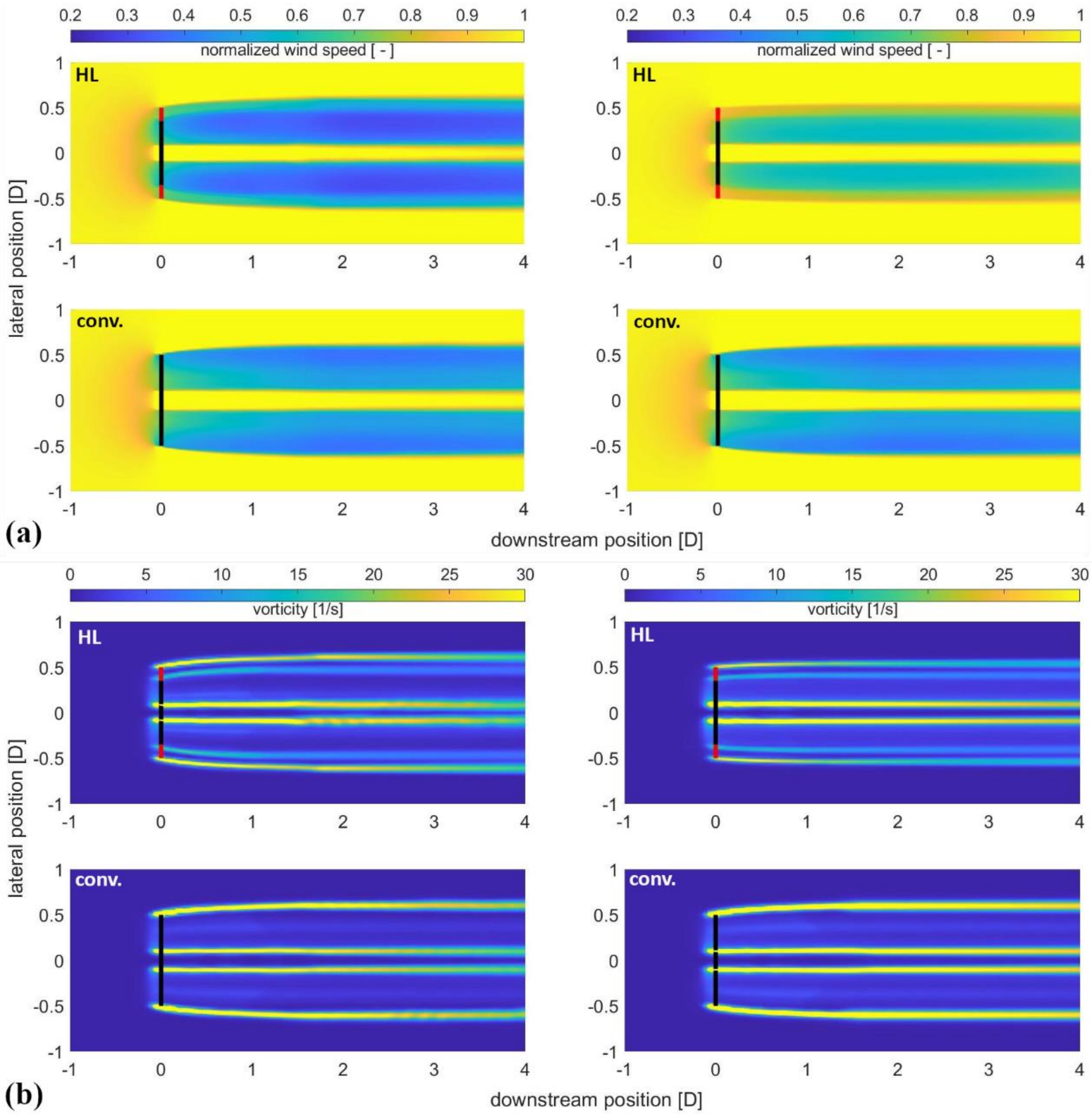

Figure 15. Normalized wind speed (a) in light-wind (left) and strong-wind mode (right). Vorticity magnitude (b) in light-wind (left) and strong-wind mode (right), for the Hybrid-Lambda model turbine (top) and the conventional model turbine (bottom) from FVW simulations.