**<u>Reply to Reviewers</u>**
Manuscript ID WES-20224-122

**General overview**

We sincerely appreciate the three reviewers for their time and thoughtful feedback on our manuscript, as well as the editor for providing us with sufficient time to address all the comments. We recognize that the initial submission contained substantial language errors and lacked clarity in some aspects. In response, we have carefully revised the manuscript to thoroughly address these issues. We believe the changes have improved the overall quality and resolved the concerns raised in the initial review.

Reply to comments from Reviewer #1

***General review:***

1. *Add in the introduction and possibly also in the conclusions a better elaboration on what the added value of this research is in the larger wind energy science context.*

**Reply**: We thank the valuable suggestions from the reviewer. In the revised manuscripts, we have mentioned the added value of the current work to the wind energy community.

<u>In the second last paragraph of the introduction section</u>:
"To address these gaps, this study systematically investigates the effects of a **broader range** of **rotor asymmetries** on **the onset of leapfrogging instability** and evaluates the robustness of this phenomenon under **both laminar and realistic turbulent inflow conditions**. Blade length differences ranging from 0% to 30% of the rotor radius are considered. Furthermore, the investigation aims to link the local effects of rotor asymmetry on tip vortex behavior to global wake dynamics. Parametric studies on vortex core size and flow diffusivity are also conducted, demonstrating that the key conclusions remain robust within the tested parameter range. The primary objectives are to **provide insights into whether rotor asymmetry can serve as a viable passive strategy to accelerate wake recovery and to assess both its potential benefits and limitations**. However, practical considerations such as the impact of asymmetry on structural loads, rotor imbalance, and durability are beyond the scope of this study and remain topics for future research."

<u>In the second last paragraph concluding section</u>:
"In general, this study provided critical insights into the aerodynamic behavior of two-bladed asymmetric rotors, particularly regarding their influence on **the onset of leapfrogging instability and wake recovery under both laminar and turbulent inflow conditions**. By systematically exploring a broad range of rotor asymmetries, it was demonstrated that **asymmetry accelerated the onset of leapfrogging** when subjected to both laminar and turbulent inflow conditions. However, **a shorter leapfrogging distance did not necessarily**

**translate into faster wake recovery**. These findings not only addressed **existing gaps in the literature concerning the behavior of asymmetric rotors under realistic turbulent inflow conditions, but also highlighted the possible limitations of rotor asymmetry as a passive control strategy for enhancing wake recovery**. "

2. *Clean up the language of the manuscript. I marked some sentences that are hard to read or understand, but there were plenty more that I did not mark, especially in the second part of the paper. I would seriously consider having a writing specialist go through the paper for this purpose, as I feel the level of English is currently sub-par and at some moments simply sloppy.*

**Reply**: Thank you for pointing out the issue and your commitment to aid us improve the manuscript. We appreciate your valuable feedback. We have carefully reviewed the manuscript and made improvements to the sections you highlighted, as well as other parts of the paper. Specifically, we have:

(1) Addressed the language-related comments and made necessary modifications to enhance clarity and readability.

(2) Removed irrelevant information and restructured sentences and paragraphs to improve the flow and ease of reading.

3. *Improve quality of the captions; right now, none of the figures are comprehensible without reading the accompanying text.*

**Reply**: Thank you for your valuable suggestion. To improve clarity, we have largely revised the captions for most of the figures. The important mathematical symbols are now explicitly mentioned in the captions to help readers interpret the figures independently. Additionally, irrelevant information has been removed from the figures to enhance the overall reading flow and focus on the key aspects.

4. *Clean up the mathematical nomenclature used in the manuscript. Parameters are regularly not used consistently or not defined at all, making it hard to understand or reproduce the work.*

**Reply**: Thank you for your valuable feedback. We have carefully reviewed the mathematical nomenclature used in the manuscript and made sure they are now in consistency throughout the article. Additionally, to improve the readability, when representing quantities that are non-dimensionalized, instead of using asterisks in the previous version, we have now explicitly written out the non-dimensionalized expression. For example, instead of using $\Delta r^*$ to represent the normalized blade difference, we have changed the notation to $\Delta R/R_0$ (or $\Delta R/h_0$ in some occasions) in the revised manuscript.

5. *Consider moving/renaming some sections/paragraphs around as suggested in the PDF.*

**Reply**: Thank you for your suggestion. We have considered the proposed changes and reorganized the manuscript accordingly. Specifically, the section describing the blade truncation is moved closer to the section describing wind turbine parameterization, which are now in Section 2.2 and Section 2.3, respectively. Additionally, the section reporting the wake quantities (Section 3.3) has been moved under the Result and Discussion section (Section 3).

***Specific comments****:*

**Section: Introduction**

1.‘’’

*Additionally, van der Hoek et al. (2022) showed the potential of the concept of dynamic individual pitch control*

’’’

Comment: *This is not the original paper showing the potential of this concept. Consider instead citing the original, Frederik et al (Wind Energy, 2020). Furthermore, the referenced paper does not use dynamic IPC, instead it uses dynamic induction as first proposed by Yilmaz and Meyers (2018). If the authors want to focus on the first time the concept of dynamic IPC was tested in the wind tunnel, Van der Hoek et al (2024) should be cited instead.*

‘’’

*Brown et al. (2022) advanced further to apply the oscillation on both rotational frequency and blade pitch.*

’’’

Comment: *This paper also does not use IPC. If the authors meant to refer to collective dynamic pitch control, these citations are correct, but I'm assuming this is not the case as this is much less comparable to the earlier references, and therefore the comparison to dynamic IPC makes more sense*

**Reply**: Thank you for pointing this out and providing the extra reference. Indeed, *van der Hoek et al. (2022)* applied dynamic induction instead of dynamic IPC. The work of *van der Hoek et al. (2024)* and *Frederik et al. (2020)* on the dynamic IPC are included in the paragraph discussing the active approaches in accelerating wake recovery now.

2. Comment*: What I'm missing in the introduction is what the application of blade asymmetry in wind turbines/wind farms might look like. Are you suggesting that we deliberately start building asymmetric turbines? Because if so, I think you should also elaborate on what that mean for turbine performance (power production, structural loads). And if not, and this study is purely for academic purposes, you should extend more on what the value of this study is: what knowledge do you hope to gain from this study that might benefit wind energy or our understanding of wind energy?*

**Reply**: Thank you for the valuable comments. Indeed, "the primary objective of this work is to provide insights into whether rotor asymmetry can serve as a viable passive strategy to accelerate wake recovery and examine both the potential benefits and limitations." As we can see from the results that "despite the earlier onset of leapfrogging triggered by rotor asymmetry, leapfrogging was found to have minimal impact on the large-scale breakdown of the helical vortex system and the subsequent wake recovery." This is contrasted with some previous literature. Furthermore, we found out that "the inflow conditions were found to have minor effects on the near-wake tip vortex dynamics of an asymmetric rotor, as both the leapfrogging distance and growth rate remained relatively unchanged compared to those observed under laminar inflow conditions. However, inflow turbulence played a dominant role in the wake recovery process, overshadowing the influence of rotor asymmetry even at turbulence levels as low as those found in controlled laboratory environments. Specifically, for the current setups, turbulent fluctuations consistently promoted the wake breakdown process to a similar extent across different levels of rotor asymmetry, highlighting that ambient turbulence, rather than the induced perturbations by the rotor asymmetry, governed the large-scale wake evolution and recovery. " In general, we conclude that "A shorter leapfrogging distance did not necessarily translate into faster wake recovery. These findings not only addressed existing gaps in the literature concerning the behavior of asymmetric rotors under realistic turbulent inflow conditions, but also highlighted the possible limitations of rotor asymmetry as a passive control strategy for enhancing wake recovery. " All these aspects are added to the introduction and conclusion sections.

**<u>Methodology</u>**

3. Comments on Eq. (1) and (2) of the original draft:

$$\frac{\partial u_i}{\partial x_i}=0, \qquad \frac{\partial u_i}{\partial t}+u_j\frac{\partial u_i}{x_j}=-\frac{1}{\rho}\frac{\partial p}{\partial x_i}+\frac{\partial}{\partial x_j}[(\nu+\nu_{\text{sgs}})(\frac{\partial u_i}{\partial x_j}+\frac{\partial u_j}{\partial x_i})]+\frac{f_{\text{body},i}}{\rho} \tag{1}$$

$$\nu_{\text{sgs}}=C_k\sqrt{\frac{C_k}{C_\epsilon}}\Delta^2\sqrt{2S_{ij}S_{ij}}, \qquad S_{ij}=\frac{1}{2}(\frac{\partial u_i}{\partial x_j}+\frac{\partial u_j}{\partial x_i}) \tag{2}$$

*I'm sure these equations are considered general knowledge, but if you are going to write them out in your paper, you should also define the parameters that are used in them, and define the coordinate system (x, y; i, j). If you really literally copied these equations from the references, you can also consider just removing these equations from the manuscript.*

**Reply**: Thank you for pointing this out. We acknowledge that the parameters and subscripts were not clearly introduced and defined in the text. To maintain the completeness of the documentation and to support the subsequent introduction of the body force term exerted by the actuator line, we have decided to retain the current set of equations in the paper. However, to enhance readability and clarity, the variables are explained in section **2.1 Large eddy simulation**, and we have explicitly stated that the coordinate system used is Cartesian. We also fixed some mistakes, such as the missing "partial" for the advection term. Below is a screenshot of the updated version.

$$\frac{\partial u_i}{\partial x_i}=0, \quad \frac{\partial u_i}{\partial t}+u_j\frac{\partial u_i}{\partial x_j}=-\frac{1}{\rho}\frac{\partial p}{\partial x_i}+\frac{\partial}{\partial x_j}\left[(\nu+\nu_{\text{sgs}})\left(\frac{\partial u_i}{\partial x_j}+\frac{\partial u_j}{\partial x_i}\right)\right]+\frac{f_{\text{body},i}}{\rho} \tag{1}$$

$$\nu_{\text{sgs}}=C_s^2\Delta^2\sqrt{2S_{pq}S_{pq}}, \quad S_{pq}=\frac{1}{2}\left(\frac{\partial u_p}{\partial x_q}+\frac{\partial u_q}{\partial x_p}\right), \quad C_s=0.168 \tag{2}$$

4. Comments on Effective diameter, including Fig.1(a), Eq. (6) and the arrangement of subsection **<u>2.4 Blade truncation and effective diameter</u>**

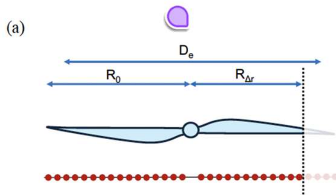


Fig.1 (a)

$$D_e = D_0\sqrt{1-\Delta r^*+\frac{\Delta r^{*2}}{2}}$$ Eq. (6)

Comment(1): *So D_e = R_0 + R_{\Delta r}? Please specify*

Comment(2): *Consider moving this section into or closer to Sect. 2.2. You are explaining some necessary information about Fig 1 here, but you moved on from this by talking about the simulation setup in the meantime.*

Comment(3): *Consider moving the equation (6) to where it's referred to in the text*

**Reply**: Thank you for the valuable suggestions. In response to these comments, we have removed $D_e$ from Fig. 1(a), as it was confusing by providing irrelevant information and was only intended as a schematic representation. $D_e$ is now introduced later in the text and properly defined in the equation within the section 2.3 **<u>Blade Truncation and Effective Diameter</u>**, which has been moved upward to ensure better alignment with the flow of the discussion.

5. On Fig.1(b) and Eq. (3) and (4)

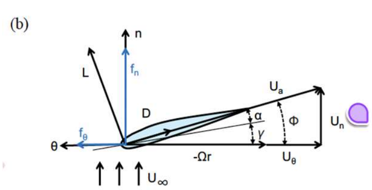


Fig.1(b)

$$\boldsymbol{f}_{2D}=\frac{1}{2}\rho U_a^2 c\left(C_L(\alpha)\boldsymbol{e}_L, C_D(\alpha)\boldsymbol{e}_D\right) \tag{3}$$

$$\boldsymbol{f}_{\text{body},d}=\sum_{k=1}^{N_b}\int_0^R F_1(r)\boldsymbol{f}_{\text{2D}}(r)\eta_\epsilon(d)dr, \quad \eta_\epsilon=\frac{1}{\epsilon^2\pi^{3/2}}e^{(d/\epsilon)^2} \tag{4}$$

‘’ *...F1(r) represents the end effect correction ...* ‘’

Comment(1): *These figures defines a lot of parameters, some of which are not mentioned in the text, without proper explanation. Furthermore, some of the nomenclature does not appear* *to be consistent with equations 3 and 4. Please use consistent nomenclature and consistently define all relevant parameters (this also goes for eqs 3-4).*

Comment(2): *Missing definition of this end correction*

**Reply**: Thank you for pointing out the issue regarding the symbol definitions. Indeed, the parameters were not clearly introduced. To improve clarity, Equation (3) and (4) and Fig.1 are updated accordingly. The used variables and the correction for the tip loading $F(r/R_0)$ are also explained in Section **2.2 Wind turbine model.** , which is known as the Shen correction factor (Shen et al., (2005)).

6. *'''*
*While this modification impacts induction and overall performance, the tip vortex pairing motion analysis remains valid as long as key parameters related to the leapfrogging instability, such as vortex separation distance and circulation, are controlled.*
*'''*

Comment: *What is this statement based on?*

**Reply**: Thank you for pointing this out. The statement refers to the relevant properties of leapfrogging motion (or vortex pairing motion) which depend on vortex separation distance and circulation, and this has been experimentally demonstrated by ***Quaranta et al. (2019)***. The original sentence lacked clarity and proper referencing. It has now been revised into "While this modification (modifying the NREL 5MW from a three-bladed rotor to a two-bladed one) impacts induction and overall performance, the tip vortex pairing motion analysis remains valid as long as relevant parameters, namely vortex separation distance and circulation, are controlled (Quaranta et al., 2019). "

7.. *'''The rotor is set to operate at a tip speed ratio of $\lambda = \Omega R_0/U_\infty = 7$, and no controller is applied for simplicity.'''*

Comment :*I suggest you move this sentence to the previous section where the turbine model is described.*

**Reply**: Thank you for the valuable feedback. This sentence has been relocated to Section 2.2 **Wind turbine model**, where the rotor model (NREL 5MW) is introduced, to improve contextual relevance and clarity.

8. *'''To better approach a realistic scenario, besides the laminar inflow condition, inflow turbulence has been set using the divergent-free synthetic eddy method (Poletto et al., 2013), implemented by turbulentDFSEMInlet function. '''*

Comment*: Please consider rephrasing this sentence as it is not very clear now what you are trying to say.*

**Reply**: Thank you for the valuable feedback! We agree that the original sentence was confusing. The sentence is revised into "For the cases subjected to turbulent inflow conditions, the divergent-free synthetic eddy method (Poletto et al., 2013) is applied to introduce inflow turbulence, which is implemented through *turbulentDFSEMInlet*. "

9. '''
*the turbulence intensity Ti is determined by mean velocity u and its standard deviations σi*
'''

Comment: *You use subscript i here twice for different reasons. What does this subscript refer too? As mentioned earlier, please specify and be consistent.*

**Reply**: Thank you for highlighting this issue. You are correct that the subscript *i* was overused, leading to confusion with the Einstein notation, as noted in Specific Comment #3. To address this, the symbol for turbulence intensity throughout the paper, including in text and figures, has been changed to "TI" for clarity. Additionally, the standard deviation has been explicitly written out. Note that the standard deviation is mentioned only once in this paper. The revised sentence reads "The definition of TI is given in Eq. (6), where $\sigma_u$ ,$\sigma_v$ , *and* $\sigma_w$ are the standard deviations of u, v, and w with respect to time. ".

10. '''
*The overall computational domain is set for $12.5D_0 \times 5D_0 \times 5D_0$ , the Cartesian coordinates definition for the laminar inflow case*
'''

Comment: *This seems on the smaller side of things. Have you run simulations in a bigger domain to guarantee that you're not seeing any blockage effects?*

Comment: *I don't understand what you are trying to say here, please rephrase.*

**Reply**: Thank you for your valuable comment. The impact of the cross-sectional blockage ratio is added to section **4.2 Cross-sectional blockage ratio**. The blockage ratio of the standard mesh is 3.1%. "As shown in Table 4, reducing the blockage ratio from 3.1% to 0.7% results in changes to rotor performance of less than 0.5%, indicating that a blockage ratio of 3.1% is not excessive. " The text in section **2.4.1 Grid layouts** is also rewritten.

11. '''
*At the most refined level in both mesh layouts, the grid size $\Delta/R_0 \approx 1/40$ within the range suggested by Jha et al. (2014).*
'''

Comment: *I'm assuming the level 1 grid cell size is 10m, and then refines to 1.25m around the rotor, for both cases? Please include this in your text.*

**Reply**: Thank you for your insightful comment. We appreciate the opportunity to clarify the grid resolution. This detail has now been added to the text for completeness and clarity.

*12.*

*‘’’The blended scheme was carried out by the Gauss fixedBlended with 95% cubic and 5% upwind. ‘’’*

Comment: *You refer to specific functions of OpenFOAM on multiple occasions in the text. While I appreciate the efforts to make the research reproducible, this effort is not particularly useful to me as a researcher who has never used OpenFOAM. Please consider elaborating how these functions work.*

*‘’’*

*At each time step, the Navier Stokes equations are iteratively solved by pimpleFoam and march with a blended scheme using 90% of Crank-Nicolson and 10% of Euler, and the tolerance is set to be 10−6 for both pressure and velocity fields.*

*‘’’*

Comment: *Consider rephrasing this sentence*

**Reply**: Thank you for your valuable feedback. We understand the importance of providing clarity for researchers unfamiliar with OpenFOAM while ensuring the methodology remains reproducible. Now, the conventional names of these interpolation schemes (e.g., central difference, upwind, and Crank–Nicolson) are explicitly documented in the text. Considering these schemes are widely used in the CFD field, this clarification enables readers to apply these methods even with other solvers or software. The paragraph has been revised *into* “The time step $\Delta t$ is set at $1.4 \times 10{-}2$ s, corresponding to a one-degree rotor rotation. This ensures the distance traveled by the rotor tip is below a grid size per time step, which is around $0.7\Delta$ with the current setups. Note that this $\Delta t$ results in a Courant-Friedrichs-Lewy number of 0.09, which is well below 1. In the simulations, the pressure-velocity coupled system is solved iteratively with PISO (Pressure Implicit with Splitting of Operators) algorithm. The time marching scheme employs the Crank–Nicolson method with a coefficient of 0.9 (*Crank-Nicolson 0.9*)*.”.*

13.

*‘’’ implemented in the ALM by reducing the number of blade elements to control the spacing Δb ‘’’*

Comment: *I don't understand what \Delta b is*

**Reply**: Thank you for the feedback. The entire paragraph has been rewritten to be consistent with Fig. 1. To clarify, “~~to control the spacing Δb~~” in the original text should be omitted.

14.‘’’

*Based on the current discretization of the actuator line, the blade length difference $\Delta r* = \Delta r/R0$ is imposed to vary from the baseline 0% to 30% in increments of 2.5%, with each step corresponding to the removal of one blade element.*

‘’’

Comment: *Does this mean you simply "cut off" part of one of the blades, instead of making it shorter but maintain the same shape? I know from BEM theory that the highest thrust is near the tip of the blade, so by simply cutting this part of the blade off, you significantly change the thrust profile of the blade. Surely, that must have a substantial effect on the aerodynamic properties of the blade. Please elaborate on why you chose to do it this way regardless, instead of for example making the elements of the truncated blade smaller in size, or reduce the number of elements but keep the same aerodynamic properties. Also discuss the consequences of your choices.*

**Reply**: Thank you for your thoughtful comment. Indeed, the asymmetry in blade length is introduced by truncating part of one blade, which was a decision after careful assessment. Modifying the rotor configuration naturally changes the aerodynamics, both locally and globally. First, it includes changes to rotor thrust and power. To account for the resulting global aerodynamic differences, the performance and integral wake characteristic of a modified rotor is analyzed using an effective diameter, as introduced in Subsection 2.3 and conducted in Subsection 3.3. As shown in Table 1, this effective diameter remains consistent across all rotor configurations and is therefore used as a length scale for wake analysis.

As the reviewer noted, removing the tip region of the blade locally may change the circulation gradient, which is responsible for generating the tip vortex. Ensuring the properties of the tip vortex is the primary focus in this work. Investigating designs that preserve the same aerodynamic properties involves the change in chord, and twist angle besides blade length, which falls outside the scope of this study. It should be noted that simple geometric scaling does not work well since the “apparent tip speed ratio” of the truncated/shrunk blade is actually modified, and tip speed ratio is the main driver for wind turbine blade design. In this sense, directly truncating the blade may be a better option as the circulation along the blade span of a typical wind turbine is usually close to constant except at the tip region. And, with our current setup, the tip-loading profiles are controlled by the tip-correction factor (Shen et al., (2005)). These reasons have led us to modify the blade length by directly cutting the blade. Note that these discussions are not included in the manuscript, as they are more like some educated guesses. The key justification for this method is based on that the most relevant property, namely the circulation strengths across the truncated and un-truncated blade, is preserved with this method, which is commented in the following paragraph.

The effect of blade truncation on the two characteristic parameters of the tip-vortices, the circulation strength and the vortex core size of the truncated and un-truncated blades are compared in table 2 within Section **3.2.2 Characteristic quantities related to the leapfrogging instability**. The results show “ that the relative difference in circulation strength between the truncated and un-truncated blades is within 1% for both mesh resolutions, confirming that the assumption $\Gamma_{\text{truncated}} = \Gamma_{\text{un-truncated}}$ holds fairly well. Additionally, differences between the vortex core sizes are also merely around 3%, further justifying the assumption that the tip vortices shed by the truncated and un-truncated blades are identical."

15. *On table1. The column of $\Delta r*$*

Comment : *These increments are not at 2.5% consistently as mentioned in the text. What is the reasoning behind these specific blade lengths?*

**Reply**: Thank you for pointing this out. Here, the 2.5% value is a compromise for readability. As explained in **Subsection 2.2 (Wind Turbine Model)**, the blade is discretized into 40 blade elements, bounded by 41 actuator points. The innermost blade element starts from the hub exterior boundary. As a result, each blade element is not exactly 2.5% of the radius of the swept area but slightly smaller—approximately 2.5% of the radius.

To enhance readability, this value was rounded to 2.5% to reduce the number of digits. However, as the cumulative percentages increase beyond 10%, the rounding difference between 2.45% and 2.5% becomes more significant. Therefore, the exact values were used for these larger percentages to maintain accuracy. This ensures a balance between readability and precision.

**Section: 2D vortex model**

16. *'''This model consists of two parallel arrays of vortices, separated by an initial radial distance $\Delta r$ in the z-direction and aligned along the x-axis (streamwise).'''*

Comment(1): *Which model, the one you developed? Or one of the models you refer to in the previous section? I recommend you only use "this" or "that" if it is abundantly clear from the previous sentence what you are referring to*

Comment(2): *Are they running parallel along the y-axis? This does not become clear from the text*

**Reply to Comment (1):** Thank you for your observation. To clarify, the model described is the one we have developed. In the revised section **2.5.1 Model definition**, now it reads "The 2D point vortex model….".

**Reply to Comment (2):** Thank you for pointing this out. The arrays are aligned with the x-axis (streamwise direction), and their separation is along the z-direction. To improve readability and ensure clarity, "This self-repeating vortex arrangement is schematically depicted in Fig. 3, where the x- and z-axes correspond to the streamwise and radial directions of the helical system, respectively. " has been added to section **2.5.1 Model definition**.

17.'''*This arrangement models the tip vortices shed from two rotor blades of different lengths, and the smallest unit in this model is illustrated in Fig.3'''*

Comment*: I don't understand what "unit" you are referring to here*

**Reply**: Thank you for pointing this out. The entire subsection **2.5.1 Model definition** and the Fig. 3 has been rewritten and revised.

18. On Eq. (7)

$$\frac{d}{dt}\begin{bmatrix}\delta x_i\\ \delta z_i\end{bmatrix} = \begin{bmatrix}V_{i,x}\\ V_{i,z}\end{bmatrix} = \begin{bmatrix}\frac{\Gamma}{2\pi}\sum_{j=1,j\neq i}^{2N_p}\frac{z_j-z_i}{(x_j-x_i)^2+(z_j-z_i)^2}\\ \frac{\Gamma}{2\pi}\sum_{j=1,j\neq i}^{2N_p}\frac{x_j-x_i}{(x_j-x_i)^2+(z_j-z_i)^2}\end{bmatrix} \tag{7}$$

Comment: *What are delta x and z? The positions of vortex x?*

**Reply:** Thank you for your feedback. Indeed, the symbol was not well defined in the original version. Your understanding for delta x and z are correct. In the revised version, we replaced delta x and delta z with x and z, and their definitions are now clearly stated in the text.

19. *'''*

*Furthermore, the normalized leapfrogging time $t_{LF}$* is defined where the vortex pair swap the streamwise positions, namely when $h_0 = \delta h$, marked by the dashed line in Fig. 4*

*'''*

Comment: *How is leapfrogging time t*LF determined?*

**Reply**: Thank you for the comment. The original text is not clear. Section **2.5.2 Leapfrogging instability** has been rewritten. In the previous version, we use $t_{LF}$* to denote the normalized leapfrogging time, which is denoted as $t_{LF}$. In our current version, the normalization action is explicitly written out, where $t_{LF}$* becomes $t_{LF}/t_{Hel}$, and $t_{Hel}=2(h_0)^2/\Gamma$.

**Section: Result and Discussion**

20. '''*Figure 5 and 6 show the instantaneous vorticity fields for $\Delta r* = [0\%,10\%]$ under a laminar inflow condition and a turbulent inflow, respectively, where an extreme case of $\Delta r* = 30\%$ is also shown in Figure 5.'''*

Comment(1): *This text is more suited to go in the caption of figures 5 and 6*

Comment(2): *I would suggest starting this section with a small introduction into what you are going to show in different subsections.*

**Reply:** Thank you for your valuable feedback. The captions of all the figures in the manuscript have been rewritten, accordingly. Following your suggestion, we add a paragraph introducing the subsections at the beginning of Section **3 Results and discussion**.

21. *Figure 6*

Comment: *To me it looks like the blade length difference, at least in the 0.5% turbulence case, actually slows down the wake breakdown. Is there some measure for the wake breakdown point* *that you could use to quantify the effect of different blade lengths on how quickly the wake breaks down?*

**Reply**: Thank you for the comment. Indeed, it's the case, this is now discussed under Section **3.1.2 Contours of tip vortices under turbulent inflow**: "the coherence of the vortex structures appears to be better maintained in the asymmetric rotor case, with the onset of wake breakdown delayed from approximately $x/D_0 = 4$ to $x/D_0 = 5$ compared to the symmetric rotor case. This observation is further validated by the phase-averaged vorticity magnitude presented later in Fig. 7. " There are measures to quantify the wake breakdown locations. However, this work focuses on quantifying the effects of blade length differences on leapfrogging instability and its growth rates. As we can see from Section **3 Results and discussion**, "By systematically exploring a broad range of rotor asymmetries, it was demonstrated that asymmetry accelerated the onset of leapfrogging when subjected to both laminar and turbulent inflow conditions. However, a shorter leapfrogging distance did not necessarily translate into faster wake recovery. " . Therefore, we don't proceed with measuring the wake breaking down locations.

22. '''

*As a result, the tip vortex core becomes less distinct.*

'''

Comment: *Would it be possible to average the results of a larger number of simulations to account for the randomness caused by the turbulence, and subsequently see what the effect on the average vortex positions/interactions is?*

**Reply:** Thank you for this valuable feedback. The contours of the phase averaged vorticity field are added in Fig .7 now for comparison with the instantaneous vorticity field provided in Fig. 5 and 6.

23.'''

*Specifically, $\sigma*$ deviates from $\sigma_{2D}^{\star}$ by 17% at $\Delta r* = 2.5\%$, and this deviation decreases to 5% at $\Delta r* = 19.5\%$.*

'''

Comment: *Could you explain why the deviation becomes smaller as the blade length discrepancy increases?*

**Reply**: Thank you for this insightful feedback. This motivates us to check the procedures to obtain the growth rate. We performed a sensitivity study and found out that the growth rate can vary depending on the time range selected to determine the growth rate, especially with the LES data. Based on this sensitivity study and for consistency, both $\sigma_{2D}$ and $\sigma_{LES}$ are determined based on the time interval $0.6 \leq t/t_{Hel} \leq 0.8$, during which the $||\eta||_1$ curves exhibit the most stable exponential growth phase. Using this updated criteria, as we can see in Fig. 12, the relative difference between the LES-derived growth rates and the model predictions increases slightly as the blade length difference increases. This is now described in Section **3.2.3 Leapfrogging instability growth rate**. However, we cannot precisely pin-point why the relative differences slightly increase with larger blade differences, especially that the absolute differences between the two remains very similar. Detailed investigation may be the topic of future works.

24. On **Section5. Results and discussion on wake characteristics**

Comment: *Section 4 is already called "Results and Discussion". Either combine them and make different (sub-)subsections, or rename Section 4 and possibly also this section.*

**Reply**: Thank you for the valuable suggestion. We agree that the structure was somewhat repetitive. To make it clearer and less confusing, the entire structure of the results part has been re-organized. Specifically, the revised structure is as follows:

- Section 3. Result and discussion
    - Subsection 3.1. Qualitative assessment of the tip vortices behavior
    - Subsection 3.2. Quantification of leapfrogging instability
    - Subsection 3.3 Wake quantities

25.‘’

*At 5% Ti, the increase is even more pronounced, but no significant difference is observed between the symmetric and asymmetric rotor configurations.*

’’

Comment: *This once again makes me wonder what the value of using different sized blades is, if it's not to decrease wake deficits.*

**Reply**: Thank you for your comment. As discussed in literature, we are not the first ones to propose using rotor asymmetry to decrease wake deficit. It has been reported in literature that asymmetry could accelerate wake recovery and earlier leapfrogging can accelerate wake recovery. The primary objective of this work is to “provide insights into whether rotor asymmetry can serve as a viable passive strategy to accelerate wake recovery and examine both the potential benefits and limitations. ” That is, further investigating whether there are values of using the strategy of asymmetric rotor can be considered one of the goals of this work.

26.

‘’

T*his suggests that while the leapfrogging distance and growth rate are primarily governed by inviscid effects, which the vortex model can sufficiently capture—as demonstrated in this study—accurately quantifying the flow quantities in the complex vortex dynamics of an asymmetrical rotor still requires the use of a high-fidelity model.*

‘’

Comment: *You also ran a number of LES simulations though. Why did you not look at the wake velocities in these simulations? I would very much recommend including these here to support this hypothesis.*

**Reply**: Thank you for pointing out this confusion. The original sentence may be misleading. In the previous version, what this sentence meant to describe is as the following. Although several vortex models—including the 2D vortex model used in this study—can predict leapfrogging behavior with reasonable agreement to LES results due to the dominance of inviscid processes, accurately quantifying flow properties such as wake velocity still requires high-fidelity simulations.

In our work, we did include the wake velocities predicted by LES, which is now in **Section 3.3 Wake quantities**.

In the revised version, after some careful considerations, we have decided to remove this statement. In the previous version, we were attempting to say that the discrepancies between the wake recovery rates of our LES results and the results of Abraham et al. (2023b) may be attributed to the model used, since they used a vortex model. However, after some careful research, we found one major difference between our and their setups, which is described as the following: “that their setup included a floor, which may have influenced on the vortex dynamics by breaking the helical symmetry.” (written in the final paragraph of Section 3.3). Therefore, the statement was discarded.

**Conclusion:**

27.Comment: *In the previous section you seem to conclude that your model is not equipped to accurately estimate the wake recovery. Why does this conclusion not return here?*
**Reply**: Thank you for the comments. There is misunderstanding, which is probably due to the unclear description of the previous version. The LES model used in this work is with higher fidelity, which is considered more accurate. And we have added a dedicated section, Section **4 Sensitivity tests on the selected key parameters**, to examine and quantify the sensitivity of key parameters used in the LES model on the final results as well. Hopefully, the revised version will clean up the confusion.

28. Comment: *Similar to my comments in the introduction, I would like to see a "bigger picture" here: what is the impact of your results on the implementation of asymmetric rotors? What did this paper teach us that we did not already know, and how are we going to profit from it? Is the conclusion that asymmetric rotors are perhaps not as relevant as other research indicated? If so, that would be a valuable conclusion, but are you sure you can say your results are more reliable than previous studies?*

**Reply**: Thank you for this insightful comment. This is indeed the importance of this work. One of the most important conclusions is that “asymmetry accelerated the onset of leapfrogging when subjected to both laminar and turbulent inflow conditions. However, a shorter leapfrogging distance did not necessarily translate into faster wake recovery. ” This is different from the expectation and other literature reported. To gain confidence in this conclusion, we have conducted a series of dedicated parametric studies to examine the sensitivity of the model parameters on the final results. With the revised version, we have full confidence in the conclusion we have drawn.

Comments from Reviewer #2

***General Comment:***

*"Near wake behavior of an asymmetric wind turbine rotor" discusses the effects of rotor asymmetry on the tip vortex behavior and velocity in the wake. The main novelty lies in the investigation of the impact of inflow turbulence and its contribution to wake recovery relative to that from the rotor asymmetry, which is a crucial question that must be addressed to justify further investigation of asymmetric rotors. However, I have some concerns about the numerical methodology used to obtain the results, particularly the resolution of the simulations. Please see below for specific comments.*

**Reply:** We sincerely thank the reviewer for recognizing the importance of addressing the impact of inflow turbulence on the wake aerodynamics of an asymmetric rotor. We also fully acknowledge the reviewer's concerns regarding the numerical methodology, particularly with respect to mesh resolution which affects the smallest vortex core size that can be resolved. In the revised manuscript and this document, we have taken actions to address these points in detail, providing additional clarifications, validations, and supporting analyses. Lastly, we are grateful for the reviewer's constructive feedback, which has significantly contributed to improve the quality and rigorousness of this work.

***Specific Comment:***

1. *My primary concern about the results presented here is the observed merging of the vortices (e.g., Fig. 5). As discussed by Ramos-García et al. (2023), vortex merging has not been observed experimentally and is likely due to insufficient mesh resolution in the simulations. The experimentally measured vortex core shed from the rotor blades is very thin, making it difficult to capture accurately using LES. In experiments (e.g., Quaranta et al. 2019; Abraham and Leweke 2023), the tip vortices do not merge, and separate helices are still observed after leapfrogging. In the results presented here, the tip vortices merge after leapfrogging, forming a new stable vortex system. It is therefore unsurprising that the current results show wake mixing is not enhanced after leapfrogging occurs and that rotor asymmetry has no significant impact on wake recovery. However, this merging behavior is likely a numerical artifact, and casts doubts on the subsequent analyses.*

**Reply:** We thank the reviewer for this insightful comment. Indeed, within the current numerical framework, using actuator line model, the minimum resolvable vortex core size is mainly governed by the mesh resolution. In response to the reviewer's concern, aspects related to the vortex core size and mesh resolution are carefully investigated in the current revised manuscript.

To begin with, additional cases with denser mesh are introduced, which are the cases with a postfix "D" listed in Table 1. The grid size of level 4 for these cases (see Figure 2) has been decreased from $D_0/80$ (the standard mesh) to $D_0/160$ (the dense mesh), which allows the simulations to resolve smaller vortex core size.

Next, in the revised manuscript, we have quantified the vortex core size $r_\omega$ obtained with the two mesh resolutions, which are found to be $0.076R_0$ (standard mesh, case Lam10S) and $0.044R_0$ (dense mesh, case Lam10D) based on the tangential velocity profile presented in Figure 18 (note that the circulation strength is largely maintained). Indeed, for the standard mesh (the mesh we exclusively used previously), a vortex core size of $0.076R_0$ is slightly above the higher end of an industrial rotor. Based on the overview provided by Abraham et al. (2023a), they documented a range of $0.01 \leq r_\omega/R_0 \leq 0.05$ (we have provided this information in **Section 2.4.4**). However, $0.044R_0$ resolved by the dense mesh falls within the range, indicating the vortex dynamics solved by this resolution should be reliable for the current application. Besides, we note that the rotor Ramos-García et al. (2023) used was specifically designed to generate highly concentrated tip vortices, as they are using a Joukowsky rotor (now mentioned in **Section 2.4.4**). On the other hand, the blade of the modified NREL 5MW reference turbine currently used features a more gradual transitioning tip loading (this is mentioned in the second paragraph of **Section 4.1**).

Then, effects of varying $r_\omega$ are investigated. With the vorticity contours for the cases subjected to laminar inflow conditions, we find that indeed vortices for the case with higher mesh resolution (Lam10D) is smaller than that with lower mesh resolution (Lam10S), which can be clearly observed by comparing Figure 5(b) with Figure 8(b) visually. Additionally, with smaller vortex size, merging is indeed delayed and subsequent leapfrogging events are observed for the case with denser mesh. These shows that $r_\omega$ indeed influences the detailed vortex dynamics. However, it is important to notice that wake breakdown is still absent for the cases with higher mesh resolution. On the other hand, based on the outcome presented in Figure 10, it is found that the trajectories of the vortex centroids before the first leapfrogging event is actually largely unaffected by the values of $r_\omega$. Furthermore, we have also demonstrated that the leapfrogging growth rate and leapfrogging distance are also not affected by varying mesh resolution ($r_\omega$) in Figure 13 and Figure 14(a). Additionally, the integral wake velocity ($<\underline{u}>_{disk}$) presented in Figure 17 again shows that switching to higher mesh resolution does not affect the impact of rotor asymmetry, that is, it has only little to no impact on the wake recovery rate, especially when the inflow conditions are turbulent.

Besides showing the fact that $r_\omega$ has minimal impacts on the conclusions drawn from this work, we also like to bring up that the wake breakdown process mentioned by the reviewer is likely due to the very low turbulence perturbations. This is supported by comparing the cases with strictly laminar inflow conditions (Figures 5(a) and 5(b)) and those with very low inflow TI (Figures 6(a) and 6(b)), demonstrating that even with TI= $0.6\%$, the wake breakdown process is triggered (see Section 3.1.2 for more detail). This statement is provided in the fourth paragraph in the concluding section.

Lastly, we have also investigated other simulation parameters that affects the vortex core size, which are the smoothing parameter $\epsilon$ and Smagorinsky constant $C_s$. Again, it is found that varying these parameters does not alter the conclusions drawn from this work. All these details are able to be found in **Section 4 Sensitivity tests on the selected key parameters**.

2. *The novelty of the discussion about tip vortex trajectories and the 2D point vortex model is not clear to me. The vortex trajectories and instability growth rate and eigenvectors for an asymmetric two-bladed rotor have been presented in previous studies (e.g., Selçuk et al. 2017, Quaranta et al. 2019, Delbende et al. 2021). The 2D point vortex model was also presented in Abraham et al. (2023) for any number of vortices. Please clarify the differences between the current results and those obtained in the previous studies.*

**Reply:** We thank the reviewer for raising this point. We acknowledge that the 2D vortex model itself is not considered a novel contribution. The 2D vortex model we used follows the formulation provided by Delbende et al. (2021). In the current revised manuscript, the reference is properly cited (see **Section 2.5.2**).

The outcomes of the 2D vortex model are used to compare with the LES results and to show the similarities and discrepancies between the predictions from theoretical frameworks and numerical results (see Figures 12 and 13). This enables a systematic evaluation of the robustness of the simulation results and facilitates the identification of the sources and nature of the discrepancies between simplified models and high-fidelity simulations.

3. *Page 1, lines 16-17: Lundquist et al. (2018) refers to wind farm wakes, not individual turbine wakes. Individual turbine wakes tend to persist ~10 rotor diameters downstream (e.g., Porté-Agel et al. 2020).*

**Reply:** Thank you for pointing out the inaccurate reference and statement regarding individual turbine wake. We have addressed this by updating the reference to Porté-Agel et al. (2020) and restructuring the sentence to align with the meaning of the new reference, as follows: <u>*The wake could persist for up to 10 rotor diameters, leading to downstream turbines often operating in the far-wake of upstream turbines (Porté-Agel et al., 2020)*</u> (the first paragraph of Section 1).

*4. Page 21, lines 399-402: Lignarolo et al. (2015) also triggered the zero-wavenumber perturbation using a small difference in blade pitch angle between the two blades. Although, I agree that the reason for the sudden increase in kinetic energy flux that they observed is still unclear.*

**Reply:** We thank the reviewer for pointing out this. Indeed, this statement appeared in our previous version was not entirely concise and may cause some ambiguity and mis-understanding. In response, the paragraph has been largely rewritten. Specifically, the fact that Lignarolo et al. (2015) used an asymmetric rotor is now clearly mentioned and some sentences are adjusted to make the context clearer. However, the goal of the paragraph is not changed. The aim of the paragraph is to elaborate that solely with leapfrogging instability will not result in enhanced wake recovery rate and accelerated wake breakdown progress. In the revised version, the corresponding paragraph is in the fourth paragraph of Section **3.3.3 Area-averaged streamwise mean velocity** $\boldsymbol{< u >_{disk}}$. We have ensured that the text written clearly addresses the points precisely.

Comments from Reviewer #3

***General Comment:***

*This paper presents results from a numerical study of the wake of an asymmetric two-bladed rotor (one blade being shorter than the other), characterising the tip vortex dynamics (leapfrogging, merging) and the overall wake evolution and recovery. The novelty with respect to similar previous studies is the consideration of large asymmetries and the additional effect of inflow turbulence on the behaviour induced by the asymmetry. The topic of wind turbine wake control is of interest to the wind energy community and the particular approach studied in this paper is in principle suitable for the WES journal. However, I find that the work shown here presents a number of shortcomings, listed below, which put into question the validity of the results and the relevance of the conclusions, and which make me recommend against its publication.*

**Reply:** We sincerely thank the reviewer for acknowledging the relevance of investigating the impact of large rotor asymmetries and inflow turbulence on the wake dynamics of an asymmetric rotor. Also, we are grateful that the reviewer recognized the importance of this topic within the wind energy community. At this point, we fully understand the reviewer's concerns regarding the validity of the results and the relevance of the conclusions, particularly in light of the numerical methodology. In response, we have carefully addressed these points in the revised manuscript and in this response document. We have provided detailed clarifications, additional validation efforts, and additional sensitivity analyses to ensure the robustness of our findings. We are grateful for the reviewer's constructive feedback, which has helped us significantly improve the quality, clarity, and rigorousness of this work, and we believe this has made our conclusions become more convincing.

***Specific comment:***

1. *Numerical method*
   - *Has the code been validated at all, by a convergence study for temporal and spatial resolution and domain size, or by comparison with other codes and/or experiments?*
   - *The Actuator Line Method requires a smoothing of the induced forces over a distance ε, which is dictated by numerical stability and is therefore related to the resolution of the actuator line discretization. As shown in various studies, this unphysical parameter also determines the size of the tip vortices. With ε chosen here as 5% of the rotor radius, the resulting core size is unrealistically large, which then leads to the observed merging behaviour. In real wind turbine wakes (as in small-scale rotor experiments, by the way), the core sizes are found to be significantly smaller, and merging is generally not observed.*

**Reply:** We thank the reviewer for raising these concerns, especially on the second point, which have motivated us to run a series of additional cases to ensure the conclusions drawn are reliable and persuasive.

- **Regarding the model verification:**

  The numerical setups and code has generally been validated by Li (2023) and Li et al. (2024). They have shown that the outcome obtained with the current setups agree with the experimental measurements and numerical results of the other independent works fairly well. This is now documented in the beginning of Section 2.4.

  In terms of temporal statistics, it is specifically mentioned in Section 2.4.2, which has also been validated by Li et al. (2024).

  In terms of domain size, which has not been validated by Li (2023) and Li et al. (2024), an additional case with four times cross-sectional size was carried out (case Lam10S_LD in Table 3.) in Section 4., showing that enlarging cross-sectional size has no impact on the conclusions drawn (see Section 4.2 for more detail). Additionally, we have conducted a sensitivity test about the selection of spatial discretization schemes in **Appendix A**.

  In terms of mesh resolution, it is together addressed with the second point later on.

- **Regarding the sensitivity of the model setup:**

  We sincerely thank the reviewer for raising these critical points, motivating us to scrutinize over our numerical setups and to perform several additional simulations. We believe with the discussions based on the additional cases performed, the conclusions drawn from this work are further consolidated. As the Reviewer #2 also raised similar concerns, to avoid repetition, we would like to kindly ask the reviewer to see the reply on Reviewer #2's 1st specific comment, where all the concerns raised here are also addressed there.

2. *2D point vortex model*
   *(a) 2D point vortices can represent 3D vortices that are perpendicular to the 2D plane. Here, the helical vortices are not (locally) perpendicular to the x-z plane of the model, they are inclined in the x-y plane. Therefore, the distances h and δh should be corrected to take this inclination into account (see the discussion in Abraham et al. 2023a).*
   *(b) The evolution of the vortex positions in this simplified model can be expressed in a compact form summing over two vortices (see Aref 1995). No need to sum over the two infinite rows.*
   *(c) The point vortex model neglects the effect of vortex curvature, which induces an additional self-induced negative velocity in the x-direction. Since in the present paper large differences in the radii of the two helical vortices are considered, this leads to noticeably different self-induced velocities, which would result in larger relative motion than predicted by the 2D model.*

**Reply:** We thank the reviewer's concerns and comments on the 2D vortex model we used (described in Section 2.5).

For point (a), as the reduced helical pitch $\hat{L}$ of the current work is considered quite small ($\hat{L} \equiv h_0/\pi R_0 \simeq 0.12 < 0.3$, Delbende et al. (2021)), the effects of inclination angle can be neglected (the correction factor will be $1.0018$ with $h_0 = 0.19D_0$).

For point (b), we have now implemented the closed algebraic form documented in Delbende et al. (2021) for the governing equations of the 2D vortex model (Equation (8)), making the expression concise.

For point (c), in order to quantify the effects of inclination angle (the first point), curvature, and the length of the vortex filaments, an analysis based on the vortex filament method was carried out in **Appendix C**. In that appendix, it has been shown that with the parameters for the two-bladed rotor currently used, the effects of the above aspects are rather minor, where the growth rate varied within $\pm 4\%$, which follows the predictions made by Delbende et al. (2021).

3. *Tip vortex behaviour Delbende et al. (Phys. Rev. Fluids 6, 084701, 2021), not cited here, have analysed in detail the dynamics of two interlaced helical vortices. For the present configuration (low pitch, radial offset of one helix), their results predict a periodic overtaking of the smaller helix by the larger helix. This is basically what is observed here, except for the interference of the (unphysical) merging at small offsets. This rather unspectacular behaviour is linked to the choice of a two-bladed rotor. For asymmetric 3-bladed rotors (Abraham et al. 2023a), the non-linear evolution is non-periodic and considerably more complex.*

**Reply:** We thank the reviewer for bringing out these aspects, especially for pointing out the reference that was missing in the previous version. That reference is now cited and their findings are included in the discussions.

In the work of Delbende et al. (2021), they have mostly used an inviscid point vortex model to predict the dynamics of a two-bladed asymmetric rotor. When perturbing in radial direction, which is equivalent to introducing $\Delta R$ in the current work, they predicted a periodic overtaking without precession motion (leapfrogging) and merging. Although the predictions by their inviscid model are interesting and heuristic, both inviscid and vortices with an infinitely small size are unphysical. To address this, they have introduced viscous effects to their model, and they showed that the overtaking mode will transition to precession motion (leapfrogging) and likely to merge eventually after adding the effects of viscosity. Actually, their previous work (Selçuk et al. (2017)) has shown that indeed transitioning from overtaking to merging is tightly linked with the vortex core size $r_\omega$, where the dynamics evolves from periodically overtaking to precession motion (leapfrogging) and eventually to merging as $r_\omega$ grows due to viscous diffusion. They also showed that when $r_\omega$ exceeds a certain threshold, modes of periodically overtaking and precession motion may be absent, and the vortices immediately start to merge.

In our revised manuscript, with the additional cases with the higher mesh resolution, the vortex core size decreases without affecting the circulation strength, as mentioned in the reply to Reviewer #2's first specific comment. Indeed, with the standard mesh (coarser resolution), the event of merging almost immediately occurs right after the leapfrogging (case Lam10S, laminar inflow, $\Delta R/R_0 \simeq 10\%$). However, with the dense mesh (finer resolution), where the vortex core size $r_\omega$ is smaller, a second leapfrogging event happens after the first leapfrogging, and the merging is delayed (case Lam10D, laminar inflow, $\Delta R/R_0 \simeq 10\%$). This generally follows the predictions of Selçuk et al. (2017). Specifically, due to the high tip speed ratio, which led to a relatively small $h_0$, threshold of merging (based on the ratio $r_\omega/h_0$) is relatively more likely to be met in our simulations. Additionally, results of our current work agree fairly well with the predictions of Selçuk et al. (2017) quantitatively in terms of the onset of the merging event. See **Section 4.1** for further discussion.

As for the asymmetric three-bladed rotor, it is considered out of the scope for the current work and is included in the recommendations mentioned in the concluding section.

4. *Comparison between simulations and model Figure 10 shows a significant discrepancy between the growth rates determined from the numerical simulations and those obtained from the 2D point vortex model. The proposed explanation is the fact that the latter "does not account for 3D effects, convection velocity, or wake expansion". The effects of vortex curvature and wake expansion are small in the present configuration, and the effect of convection velocity, as presented here, does not exist (see below). A more likely explanation lies in the fact that the two growth rates were apparently not calculated in the same way. Whereas the one from the 2D model is non-dimensionalized by $\Gamma/(2h_0^2)$ – and not by $\Gamma/h_0^2$ , as written in the text on page 9 – the one found in the simulations is nondimensionalised by $U_c/D_0$, if I understand the description on page 14 correctly, where $U_c$ is the mean convection velocity of the vortices. These two quantities are basically unrelated, and it is even surprising that the two results are as close as they are in figure 10.*

**Reply:** We thank the reviewer for pointing out the flawed descriptions, particularly the "convection velocity", that we had in our previous version which may cause unnecessary confusions.

First, we would like to clarify that all the growth rate related quantities are non-dimensionalized against the inverse of the characteristic time scale, denoted as $t_{Hel}{}^{-1} = 2h_0{}^2/\Gamma$. This is now clearly labeled and described in Figure 12. Moreover, to avoid further confusions, we have abolished the usage of asterisks to represent the non-dimensionalization. In the revised manuscript, all the non-dimensionalization actions are explicitly written out.

Next, we will like to address the discrepancies between the 2D vortex model and LES simulations. To start with, we like to thank the reviewer for pointing out that the description of "convection velocity" is flawed. Indeed, in the context of the vortex model, the relative motions of the tip-vortices are completely governed by the induced velocities. In our revised manuscript, we have updated the text and attributed the disagreement to "the neglect of three-dimensional effects, hub vortices, spatial wake development, and finite vortex core size", which is in the fourth paragraph of **Section 3.2.3**. Additionally, we have also provided a quantitative explanation based on the assessments in **Appendix C** and the work of Selçuk et al. (2017), showing that the three three-dimensional effects and the omission of the hub-vortices are indeed the main source for the discrepancy (at the end of the fourth paragraph of **Section 3.2.3**).

5. *Effect of convection velocity In several places, it is argued that the difference in convective velocities of the inner and outer vortices represents an effect influencing the tip vortex evolution which is distinct from the BiotSavart induction. However, there is no externally imposed velocity gradient in this flow. It is the existence of the vortices which generates the velocity defect behind the rotor, as well as the gradient responsible for the different convection velocities. The evolution of their positions is entirely determined by their mutual and self-induction (plus the constant free-stream velocity) and the described 'convection velocity effect' does not exist.*

**Reply:** We thank the reviewer for pointing out our flawed description. Indeed, as already addressed in the previous comment, in the framework of the vortex model, the relative motions of the vortices are completely governed by the induced velocities based on the Biot-Savart formulation. We have removed all the statements related to the "convective velocity" in the revised manuscript. For example, in **Section 4.5** of the previous version, the discussions on the contradiction about smaller growth rate but shorter leapfrogging distance for the cases with larger $\Delta R$ is **now replaced** from ~~attributing it to the competing effects between the vortex induced velocity and convective velocity~~ to explaining it based on Equation (8). In the revised manuscript, we indicate the reason for this contradicting phenomenon is due to the initial conditions (in **Section 3.2.4** of the revised version).

***Technical corrections:***

1. *In figure 1(b), the blade profile should be aligned with the line separating the angles α and γ. As it is sketched, the angle of attack is zero.*

**Reply:** We thank the reviewer for pointing out the mistake, it has been corrected in the revised version (see Figure 1(b)).

2. *On page 6, it is stated that lengths are normalised by the rotor diameter. However, the blade length reduction Δr appears to be normalised by the rotor radius. This leads to some confusion in the presentation of the results.*

**Reply:** We thank the reviewer for pointing out what may potentially confuse the reader. In the revised version, instead of using $\Delta r^*$ to represent the normalized blade difference, we have changed the notation to $\Delta R/R_0$ throughout the article, aiming to make the expression clear.

3. *What are $\delta x_i$ and $\delta z_i$ in equation (8)? Should they not be xi and zi?*
**Reply:** We agree with the reviewer about the formulation, where $x_m$ indeed is more concise than $\delta x_m$, and the additional $\delta$ may cause confusion. It is now updated in Equation (7).

4. *Page 9: the only length scale in the 2D model is $h_0$. It is therefore meaningless to consider a $\Delta r^*$, which is based on the rotor radius. It would be more helpful to provide the value $\Delta r/h_0$.*

**Reply:** We thank the reviewer for pointing out this and we agree on this point. Now the L1 norm in Figure 4 is normalized with $h_0$, so as in Figure 11 and Figure 12.

5. *Page 17, bottom: What is a W-tunnel?*

**Reply:** We have modified the descriptions about the supplementary experiment. W-tunnel is a low-speed open jet wind tunnel with a cross-sectional area of 60 cm by 60 cm. This information has been updated in the revised manuscript (see the last two paragraphs of **Section 3.3.1**).