We would like to thank the referees for the constructive feedback, which certainly contributes to strengthening the article.

In the following, we would like to address your comments and suggestions individually.

We mark our responses in red, the reports of the referees in black. Our line numbers are based on the updated latexdiff version of the manuscript.

RC1:
Remarks:

1. The motivation is not fully clear. The study aims to perform similar investigations as performed on smaller rotors on a larger one? If so I would expect more parallels to be drawn in the conclusions.

   The motivation in lines 58 to 83 has been revised to clarify that we analyze a yaw maneuver used as a control strategy to reduce rotor loads in the event of a pitch motor failure. In addition, it now explicitly states that we aim to assess whether this maneuver introduces unwanted side effects, with a particular focus on the onset of blade vibrations. So, it becomes clearer that it's not the main aim to perform similar investigations as performed on smaller rotors. Additionally, we have extended the conclusion in lines 508 to 514 to draw a parallel to a smaller rotor that our approach could be seen as an alternative approach of mapping the influence of the inflow angle (Horcas et al., 2022) via a continuous sweep, but that a direct comparison of results is difficult due to the different range of inclination angles (lines 512-514). We now also state in the conclusion that the findings of incoherent vortical structures at low inclination angles align with the findings made on a smaller blade (lines 520-521).

2. I'm not quite sure how inflow turbulence is accounted for in the simulations. A boundary condition on turbulent eddy viscosity is listed in table 1, but very little other detail is mentioned. This is relevant as the inflow turbulence may interact with the turbulent structures generated by the blades and alter the aeroelastic response.

   The given turbulent viscosity corresponds to a turbulence intensity of 0.1% in the rotor plane. We chose this almost "laminar" inflow to get a conservative estimate on the occurrence of VIV. (To clarify this, we added this statement on lines 111 to 113.) The influence of turbulence might reduce the VIV but this is to be studied in a future paper. This has been added to section 2.2 "General case setup" for more clarity, as well as to the outlook in the end, where we state: "Further investigation should also address the influence of turbulence and its interaction with wake structures on blade vibrations, as this enhances the physical depth of the problem."

3. One detail that is again not clear is whether drivetrain and yaw bearing flexibility are accounted for. Since edgewise vibrations are the main concern, drivetrain flexibility (and structural damping) may impact the observations herein. I would expect this aspect to be discussed.

   We have added a statement in the "Structural model and discretization" section, stating: "Structurally, the rotor is modeled using three isolated beams, without taking into account the drive train, yaw bearing or the tower flexibility to purely focus on the wind-blade interaction". In a similar comment by RC2 we stated that this study investigates isolated blade setups to isolate the fundamental flow mechanisms and their role in vibration growth. The goal is to understand the driving flow processes at the blade level that lead to vibration buildup. We focus on different stages, from the onset of vibration to how flow quantities lock in with the blade structure and drive vibrations. Exploring the full machine, where interactions between components can occur, would be a logical next step to be studied in a future article, but most probably will make the effect more complicated as there might be an interaction between different structures. This has been addressed on lines 536-538.

4. The quality of writing is ok. Some paragraphs appear too long/convoluted. I've highlighted some inconsistencies below.

   We improved the article based on the comments given.

Detailed comments:

1. Table 2: tilt angle of the IEA 15 MW should be 6°.

   That is true. This was a mistake in the table. All simulations have been performed with a tilt angle of 6°. The table has been updated.

2. Table 2: How are the pitch angles and azimuth positions chosen? It is obviously impossible to simulate lots of conditions with high fidelity methods but some motivation should be provided. Also, why is the pitch angle of blade #2 different from the others?

   In accordance with the reformulated motivations, we have simulated a storm condition in which one blade fails to pitch to feather and remains at 60°. We assume a control strategy in which the rotor is yawed in such a way that the yaw misalignment is -90°. In this way, the flow reaches the rotor directly from the side and the loads are minimized. This has now been clarified in the motivation (lines 65 to 72) and Sect. 2.2 "General case setup" (lines 120 and 123-124).

3. Section 2.3: The set-up is validated referencing simulations where the rotor is operating. The grid sensitivity study also appears to be performed in such conditions. These conditions are very different from those simulated herein, where the rotational speed is zero and the blades are vibrating. I'm not sure that given these differences the verification of the model is fully convincing.

   The verification is performed with the only available data. No information of the rotor standing still with vibrating blades is available. This has been added to line 137.

   Simulations within IEA Wind TCP Task 47 are performed for the rotor in operation. This reference has been given as additional source to support the applicability of the mesh. We have added "[...] for a turbine in operation [...]"(lines 155 to 157)

   The main grid sensitivity study has been performed on standstill conditions similar to the one described in case FLEX37 but with a forced motion solver enforcing a pure, dynamic edgewise mode shape oscillation, so that all meshes experience the same unsteady blade deformation. The rotational speed is zero. Thrust and torque quantities are extracted as a measure of integrated blade quantities. In line 139 we added the expression "in standstill" for clarification. Additionally the sentence in lines 143-144 now reads: "A forced motion oscillation with a maximum ~6.8m edgewise tip deflection amplitude in shape and frequency of the first edgewise eigenmode is applied."

4. Section 2.4: Blade torsion is mentioned when discussing damping factors but I haven't seen it brought up again elsewhere. Are torsional vibrations irrelevant?

   We have now updated Fig. 7 on page 14 in Sect. 3.2.1 to also show the torsional (and flapwise) vibration components. Since all three investigated components are largely driven by the first edgewise natural frequency, we focused the investigation on that component. This has now been addressed in that section (lines 300-304 and 313-315) for clarification. Additionally, in the discussion on the angle of attack in Fig. 15 the contribution of torsion has been addressed (lines 392-393).

5. Figure 4: Is it possible to attribute the observed behavior to one or more modes? Based on the shape of the energy curve, is it possible that during ramp-up the first mode is dominant while during ramp down the second edgewise mode is dominant? Moreover, do torsional modes play a role?

   From a frequency perspective, both the ramp-up and ramp-down are mainly driven by the first edgewise frequency, as well as smaller contributions of the second and third edgewise frequency. Flapwise and torsional do not appear to play a role here. We added a statement that a detailed analysis of the vibration modes is performed in Sect. 3.2 (line 268). In lines 294-295 we now write: "[...] this motion is mainly dominated by the edgewise eigenfrequencies. The flapwise and torsional natural frequencies do not appear to impact the vibration behavior." With the updated Fig. 7, the frequencies of the different components also become clearer (see comment above).

6. L290-295: given the provided explanation, which seems logical, about how vibration amplitude can increase despite total power being negative, the poi of this section is not entirely clear. I.e.: if total power is not directly correlated to the amplitude what is the usefuleness of the analysis in Fig. 11?

   From the results shown in Grinderslev et al. (2022), we would have expected similar findings in our case, i.e., a curve exhibiting negligible power difference for small vibration amplitudes. However, our results show a different behavior, which is an interesting finding and indicates that this method cannot always be applied to vibration cases. Accordingly, we have revised the wording in the manuscript in lines 344-346 to: "[...] Based on the results shown in Grinderslev et al., one would expect the ΔP curve to start and end at 0kW. However, in the present case a different behavior is observed, indicating a non-zero net energy exchange at the beginning and end of the yaw maneuver. [...]"

7. Figure 13: Why was r/R = 0.9 chosen?

   We chose r/R = 0.9 for all investigations of aerodynamic quantities and near wake structures because here we expected a major influence of aerodynamic power on the results (see, for example, Fig. 4). This expectation is based on the outer regions of the blade experiencing higher rates of deformation than sections closer to the blade root. Additionally, the influence of a possible tip vortex remains limited. A short clarification has been added to the beginning of section 3.2.4. There we state now: "To achieve this, all four cases presented in Tab. 4 are examined at r/R=0.9, since this spanwise position is expected to experience major influence of aerodynamic energy (cf. Fig. 4)."

8. Section 3.2.4: the 37° case shows significant hysteresis on Cl, while the 60° case does not. Can the reason be reconducted to the different nature of the vibration (edgewise vs flapwise), or are they related to unsteady aerodynamic effects (vortex shedding for example) ?

   It is the combination of deformation and aerodynamic effects. For 37°, we can see that from a rigid setup (Fig. 14a), where only aerodynamic effects play a role and already a circular pattern is visible, to the flexible setup (Fig. 16a) the hysteresis pattern gets more pronounced. We conclude that unsteady aerodynamic effects are the driving mechanism and that a lock-in leads to a clear hysteresis. Since the angle of attack is based local velocity probes around the blade, it gives a first hint that vortex shedding could play a role. We rephrased the concluding paragraph in the end of section 3.2.4 to make this clearer and to link this section towards the following near wake frequency analysis (lines 405-411).

9. L251: At this point..

   It has been added to the text. The sentence has also been extended with the fact that the theoretical frequencies are based on a preceding modal analysis (see comment of RC2). “At this point, a slight shift of the blade's inherent oscillation frequencies towards smaller values can be noted, relative to the theoretical eigenfrequencies that are based on a preceding modal analysis.” (lines 296-298)

10. L375: This phrase is unclear, please rephrase. In addition, the discussion in this and the previous paragraph is also unclear. Why lock-in happens in one case and not the other is not easy to understand, as in both cases one IMF has a peak at the edgewise natural frequency. Is it due to the shift of this peak in the flexible simulations? Pleas eexplain this more clearly.

    The sentences have been rephrased to “This lock-in phenomenon does not occur in the case of -60° yaw misalignment, as shown in Fig. 21. Although IMF-7 has its peak in the vicinity of the first edgewise frequency in the rigid setup (Fig. 21a), the signal's power distribution does not lock into the first edgewise natural frequency during the flexible simulation phase (Fig. 21b). [...] The slight differences between RIG60 and FLEX60 can be explained by the minor change in AoA, when the blade adjusts to the loads.”

    Additionally, these two paragraphs do not aim at describing why lock-in happens or does not happen. The goal of this section is to describe the behavior of the inherent frequencies. An explanation of why lock-in happens in one case and not in the other is given at the end of this section where the influence of the Strouhal number is discussed. There we write: “It is therefore concluded that instability occurs when wake structures lock in with the first edgewise eigenfrequency (FLEX37), whereas higher-frequency correlated wake shedding that avoids this lock-in suppresses energy near the mode and stabilizes the blade (FLEX60). In the FLEX37 case, the observed coherent wake frequencies cannot be attributed to classical Strouhal shedding from a bluff body. Instead, the instability appears to originate from a three-dimensional separated shear layer that develops under yawed and inclined inflow, generating periodic wake structures whose characteristic frequency approaches the first edgewise eigenfrequency. This frequency alignment promotes lock-in and results in negative aerodynamic damping.”

11. Section 3.2.5: The analysis of the velocity probe highlights some interesting findings. It is not clear what phenomenon is triggering the vortex shedding which leads to the critical lock-in phenomenon in the FLEX37 case. Authors motivate

that it is not Strouhal vortex shedding, but I haven't seen an alternative explanation.

The observed coherent structures are not attributable to classical Strouhal shedding from a bluff body. Instead, the instability seems to originate from a three-dimensional separated shear layer under yawed inflow, generating periodic wake structures whose characteristic frequency approaches the first edgewise eigenfrequency. The resulting synchronization leads to lock-in and negative aerodynamic damping.

This has been added to the conclusion of this section (lines 488-491 see comment above).

12. Conclusions: the premise in the introduction is that somewhat similar studies have been performed previously on smaller rotors. The conclusions would strongly benefit from a critical comparison to such studies. Although I imagine they cannot be directly compared, are there any indications that size may play a critical role? Can any differences are similarities be noted? This would go great legths in demonstrating the importance of these approaches.

We have rephrased the conclusions to make it explicit that the motivation behind this study assumes a storm scenario in which one blade fails to pitch to feather, and a load-mitigation control strategy is applied in which the rotor is instead yawed so that the inflow approaches the rotor directly from the side to minimize loads. The revised conclusions also clearly state that the work investigates the unwanted side effects of this maneuver, the resulting blade vibrations (lines 493-500).

Additionally, we state that our work can be seen as an alternative approach to mapping the influence of inflow angles as done in Horcas et al. (2022) (continuous mapping instead of discrete yaw steps) (lines 508-512). However, a direct comparison of our results with those for the smaller blade in Horcas et al. (2022) is difficult. Although the first edgewise frequencies of both blades are very similar (0.67 Hz vs. 0.68 Hz), the inclination angle in our study (−30°, flow from root to tip) lies well outside the range considered by Horcas et al. (0° to 70°, flow from tip to root). This has been added to lines 512-514.

RC2:

General Remarks:

This work presents a study on vortex induced vibrations on the IEA15MW wind turbine in extreme conditions, where the turbine is in stand still. An instability is found around 40deg yaw-misalignment on one blade with small inclination angles. Subsequently an in-depth analysis of the this isolated blade is carried out.

The content of the article is well presented but the language and flow could be improved in several parts. Furthermore, care should be given to the bibliography and the references as at times references are not found in the bibliography or not rendered correctly in the text. Abbreviations are not always introduced properly.

The implications summarized in the conclusion (Line 439 onwards) are very relevant but are only superficially addressed in the work itself. I recommend detailing specifically how this work helps contribute to, for example, controller design.

Major Comments

The authors mention Grinderslev (2023) and that numerical results are very sensitive to low-inclination angles. It is of my understanding that the core analysis of this paper is also based on a low-inclination case (blade 2). Could the authors comment on the uncertainty related to the results based on the sensitivity that Grinderslev found?

Grinderslev (2023) reported a sensitivity of such cases towards the grid and turbulence model. We based our meshing resolution on the findings made by them. They indicated that a mesh with 512 spanwise cells (around 35M cells total) under similar operating conditions (inclination angles) is sufficient to predict the power accurately. Therefore, a finer grid would not result in any major improvement in prediction. In our case, since the blade is longer (20%), we even increased the total number of cells in the spanwise direction (total 40.2M cells). This leads to an effective resolution within the recommended range by Grinderslev (2023). This mesh sensitivity is due to the behavior of the turbulence model, specifically DDES, which we also use. The hybrid nature of the model leads to URANS-like behavior in coarse mesh regions. Furthermore, we inspected the indication function which marks LES and URANS to make sure that the wake of the blade is well resolved and within the LES region.

The authors also refer to Pirrung (2024) to highlight the importance of simulating a fully coupled rotor. However, as far as I understand, an isolated blade is analyzed for the in-depth analysis, while the coupled approach is mentioned in the conclusion as future research. Could the authors comment on their decision to focus on an isolated blade and the possible implications for the findings of this paper?

To isolate the fundamental flow mechanisms and their role in vibration growth, this study investigates isolated blade setups. The goal is to understand the driving flow processes at the blade level that lead to vibration buildup. We focus on different stages, from the onset of vibration to how flow quantities lock in with the blade structure and drive vibrations. Exploring the full machine, where interactions between components can occur, would be a logical next step to be studied in a future article. This has been addressed in lines 73-75, 161-163, 272-275 and 595-597.

Introduction:

In general, the introduction would benefit from more literature references. Specifically, whenever a new phenomenon is introduced or a statement is presented (some examples are highlighted in the minor comments)

We added references to five parts of the introduction.

Numerical methods and setup:

The edgewise frequency amplitude was set to 5%, which is assumed to be reasonable. Could the authors comment on the basis for this assumption? Perhaps an additional source would be helpful.

For a smaller machine (10MW) and lower wind speed (less energy) Heinz et al. (2016) reported at least 2% for an isolated blade, while Pirrung et al. (2024) reached up to 17.7% (full turbine, including tower flexibility). As the forced motion simulation is supposed to serve as a first estimate of a reasonable vibration amplitude, we decided on this amplitude. The two references are now given in this context, and we explain this in the paper now in lines 145-147.

Results and discussion

Some figures (especially in Section 3.2) are misplaced relative to the text making it hard for a reader to focus on the analysis. It is recommended to place the figures as close as possible to the analyzing text.

The positioning of figures has been optimized now.

The results section is structured so that the case described in Sect 3.1 identifies a critical situation in which VIV occurs on blade 2. Sect 3.2 isolates this case and provides an in-depth analysis of the driving mechanism behind this instability. This context could be communicated more clearly as currently a reader expects two test cases to be analyzed that are not necessarily connected based on the paragraph between Lines 86-92. This leaves one with open questions after reading Section 3.1, such as:

“An instability that occurs between 33° and 55° yaw misalignment is shown and the energy exchange between the fluid and structure is analyzed, however the underlying reason for this instability is not discussed.”

These questions are addressed largely in Section 3.2. It is recommended to outline the arc of the paper more clearly before the results section.

The outline and connection of the results is now added to the beginning of the “Results and discussion” section (lines 211-216). It now reads as follows: “The following sections describe the results of the investigated scenarios. In Sect 3.1, the full rotor simulation during a yaw control maneuver from 0° to -90° is described. A critical yaw misalignment region is identified in which blade vibrations occur on one blade. In Sect. 3.2, the critical blade is isolated under stand-still conditions to provide an in-depth analysis of the driving mechanisms of this instability and to examine the underlying aerodynamic characteristics. For that purpose, a frequency analysis is conducted at one representative radial station on the blade and in the near-wake.”

Conclusion:

Line 441:

“Furthermore, it could contribute to incorporating critical rotor-inflow conditions into the controller design process. Actively pitching the blades can readily circumvent these vibrations, because of a fast change in angle of attack. In the event of a pitch motor failure, these critical inflow-turbine scenarios would need to be avoided.” While an interesting aspect, the control aspect is only raised in the conclusion. It is not fully clear how this work might contribute to controller design. Do the authors suggest to identify as many cases as possible where VIV occurs of a specific rotor following the approach presented in this work and letting this inform control design?

This paper does not analyze the technical solutions to the problem but helps to identify potential critical situations in storm conditions. Advanced controller systems can be designed in such a way that severe resonances are prevented in those conditions. Some possible technical solutions include periodic pitching or yawing motions. The analysis of the technical solutions exceeds the scope of the paper.

Minor Comments:

Line 14: (IEA (2023)) reference not formated correctly, should be (IEA, 2023)

The format has been updated on this and other references as well.

Line 14-16: It is stated that the deployment of wind turbines neccessitates a deeper understanding of the complex dynamic whereas the reason is more rooted in the changing nature of the turbines that are driven by the economic pressure (as is stated afterwards by the authors), consider restructuring. Also, some references could be added here.

This statement is now being split, such that it builds a frame around the introduction of larger rotors being impacted by vibrations (see next comment). In lines 15 - 27 it now reads like

- Deployment of wind turbines expands → high economic demands for low prices → therefore, wind turbines grow in size and see influence of vibrations → this necessitates a deeper understanding of the complex dynamics → for example phenomena like SIV and VIV need more attention

Line 16-17: Please provide some background for why larger rotors are beneficial in terms of LCOE, and why this leads to more slender blades. Consider adding sources.

These sentences have been rephrased as follows:

“In this context, the economic demand for low prices in a highly competitive market leads to larger rotor diameters, as larger rotors capture more energy from the wind flowing past the rotor disc, while the cost impact on other turbine components, such as the nacelle, tower or foundation, remains limited (Bolinger et al., 2021). Consequently, slender, lightweight structures emerge as a direct result of rotor diameter growth, provided that the impact on other turbine components remains limited, with higher blade flexibility accompanying this growth. This ultimately leads to reduced levelized costs of energy (Veers et al., 2019).
This has been supported by two additional sources, namely Bolinger et al. (2021) and Veers et al. (2019).

Lines 20ff: SIV and VIV are introduced. While the latter is defined afterwards, a definition of SIV is missing. Please consider adding foundational references for both.

A short description of SIV has been added to the introduction stating that stall may lead to negative aerodynamic damping resulting in lift force being in phase with blade motion and therefore supporting the growths of vibration. Both VIV and SIV, are now referencing literature (lines 27 and 31).

Line 31: ... turbulence modeling and grid characteristics “in CFD analyzes” of a 10 MW wind turbine

The sentence has been rephrased to “[...] turbulence modeling and grid characteristics in CFD analyses of a 10MW turbine.” (line 40)

Line 34: For clarity it would help if aerodynamic power was defined by the authors e.g., power injected into the structural system by aerodynamic loads.

Sentence has been rephrased to: “This sensitivity is connected to the influence of wake vortex structures on the power injected into the structural system by aerodynamic loads.”

Line 36: Horcas 2020 is cited with a description of the work done in the paper but no conclusion concerning the influence of blade tip geometry with regards to changes in the wake is given.

Two sentences have been added, stating that “It has been found that bending the blade tip out of the rotor plane leads to an earlier breakdown of the vortex tubes shed from the tip. Additionally, depending on the tip geometry, wake lock-in can be shifted towards higher inclination angles or mitigated completely.”

Line 39; FSI was not introduced yet

The introduction of FSI in line 50 is now added to the sentence.

Line 42: “In this configuration, substantial blade deflections considerably affect power injection near the blade tip.” What is the driving difference between isolated blade set-ups and fully coupled rotors. I would assume that for an isolated blade, blade deflections also affect power injection to a similar degree near the blade tip.

We agree that for both setups, isolated and fully coupled, the power injection near the blade tip is affected similarly. The difference in power injection/extraction is more driven by the vibration amplitude. As Pirrung reported, fully coupled simulations might lead to larger deflections, due to e.g. a weakly damped tower torsion, than the isolated setup and therefore showing a different behavior of aerodynamic power near the blade tip. We have rephrased these sentences to “However, Pirrung et al. (2024) demonstrated that in a structurally fully coupled turbine, tower torsion drives substantially larger blade deflections, which markedly affect power injection near the blade tip. By contrast, such vibration amplitudes are not reached in isolated blade setups and the resulting power injection differs from that of the fully coupled system.”

Line 44-55: The gap in existing literature is not fully clear to an independent reader at this point and is only detailed afterwards. The paragraph could be restructured for clarity, first pointing towards the current gap in the literature and then mentioning in which way this work addresses this gap. Moreover, the necessity of this analysis compared to previous analyses of the 10MW rotor could be further explained. Drawing a comparison to the IEA10MW could also be an interesting discussion point for the conclusion.

This motivational section has been restructured to emphasize that, in addition to the larger rotor size, our study differs by analyzing yaw misalignment through an assumed continuous yaw-sweeping maneuver intended to reduce loads. It now reads: “Existing studies on inclined inflow and wake-vortex-induced vibrations have so far mainly considered turbines up to a 10MW scale and typically focused on fixed or discretely stepped yaw angles, rather than on yaw-based load-mitigation strategies. This leaves a gap in understanding for larger, more representative turbines in the 15MW class and for storm-fault scenarios in which a dynamic yaw sweep, as assumed in this study, is used to reduce extreme loads. To address this gap, this paper presents a comprehensive high-fidelity CFD analysis of the IEA 15MW turbine under an extreme storm-fault scenario, in which one blade fails to pitch to feather and remains at a fixed pitch angle of -60°, while a dynamic yaw maneuver drives the rotor toward a misalignment of -90° so that the inflow approaches the rotor from the side and the mean aerodynamic loads are minimized. The present work focuses on the unintended side effects of this control strategy, namely the aeroelastic blade vibrations that may arise during the yaw sweep. [...]"

Additionally, in lines 508-514 we have extended the conclusion to highlight that our approach could be seen as an alternative approach of mapping the influence of the inflow angle (Horcas, 2022) via a continuous sweep. However, a direct comparison of the results is difficult to make as our inclination angle lies outside the range investigated by Horcas (2022). (see also comment of RC1)

Line 57: "To investigate the blade deformation response of a large wind turbine to yaw misalignment during storm conditions," Is blade deformation resonse the correct expression here?

It was changed to "blade oscillations" for more clarity (line 85).

Line 58: "generic" is not necessary

It has been removed from the text (line 86).

Line 60: TURBINIA, reference is not correct. I can't find the entry in the bibliography. I assume this should be Scherpers et al. 2025.

Schepers et al., 2025 is the correct reference. It has been corrected in the document. The report is now published, wherefore the statement "future publication, under review" has been removed from the list of references.

The "et al." has also been added to all other references, wherever necessary.

Line 60: It is mentioned that several numerical studies have been carried out but only one reference to TURBINIA is provided. Is it that Turbinia contains multiple studies or might references be missing here? Please clarify.

Context has been added. It is now stated that the cited reference belongs to the work of an IEA Task in which these studies have been performed. (lines 88-89)

Line 90: IEC not introduced

The introduction of IEC is now added to the sentence. (line 121)

Table 3. Absolute values for thrust and torque of the reference (fine) might be helpful here. Also, could the authors comment on the computational cost difference between the different mesh configurations?

Total values for thrust and torque, as well as the computational costs for a total simulation time of 64 seconds have been added to table 3 on page 7. Additionally, sentences have been added to the text, describing the time span which is being averaged and that the medium case is 30% cheaper than the fine case (lines 148-149 and 153).

Line 124: IEAWindTask37b (2020), no entry found int the bibliography. Also, the formating in the text is not correct.

It has been corrected in text and bibliography

Line 139: IEAWindTask37a (2020), no entry found int the bibliography. Also, the formating in the text is not correct.

It has been corrected in text and bibliography

Figure 2d: Could the authors comment on why around r/R 0.6 the phase offset reduces to zero and back to 180° again for further outboard regions. For this region of the span the maximum deflection in edgewise and flapwise DOF seem to be in phase.

The reason behind this is that the graphs on the left-hand side (a,c,e) always show the tip displacements of both components in positive direction, while the phase graphs on the right-hand side (b,d,f) express how the actual phase shift between these components is. This was done to tighten the layout and to prevent the graphs from interfering with the legend. We agree that this could be misleading, therefore we have updated the graph (Fig. 2 on page 8) and positioning of the legend, so that plots on the left-hand side show the actual phase shift between the dominant and respective other component.

Line 150: "Empirical Mode Decomposition" usually, no quotation marks are used when an abbreviation is introduced. Same for IMF.

Both quotations have been removed from the text.

Line 186-188: The description of the the instability in 3e could be rephrased for improved clarity. The word "lead" might be confusing in an instability context.

The description has been rephrased in the text. In lines 226 to 230 it now reads: "A closer look at the tip oscillations of blade 2 (cf. Fig.3e) reveals a vibration ramp up between -30° and -43° yaw misalignment. This ramp up can be visually split into two segments: from -30° to -33°, where amplitudes show a minor increase and from -33° to -43°, where the growth rate of the tip displacement is larger, reaching a maximum tip displacement amplitude of 3.5m at the end of the segment."

Table 4: The blade position should be added, I assume this would be 330° matching blade 2 of the previous subsection.

That is correct. For clarity the 330° azimuth position has been added to the text in line 281 and table 4 on page 13.

Line 251-252: “A slight shift of inherent oscillation frequencies in comparison to the theoretical eigenfrequencies of the blade towards smaller values can be noted”. Were the structural frequencies matched (validated) in a modal analysis of the blade structure previously? Could it be related to how the drive train is modeled or the lack of a flexible tower?

The structural frequencies in these graphs have been derived from our model using a previous modal analysis. We have added a short statement to clarify that (line 298). Also, the frequencies of the first flapwise and edgewise components agree well with the frequencies reported by DNV-Bladed model 1.1 (Deviations for 1st flap 0.39% and 1st edge 0.88%) and another in-house structural code (deviations <0.9%).

The shift cannot be explained by the drivetrain, since the blades are modeled as cantilever beams from a structural point of view. The influence of other components is neglected, i.e. no drivetrain is modelled. We have added a sentence in the “Structural model and discretization” section to clarify that (lines 161-163).

Figure 8: It might be helpful to add the energy of the yawing case as a reference (as a dotted line perhaps). Making it easier to compare the two.

The accumulated energy within these graphs depends on the simulated time to reach the limit cycle (single blade case) or maximum tip displacement (yawing case). As the energy accumulated for e.g. the ramp up phase in the yawing case is on a different time scale, it is difficult to plot the 2 graphs within one plot. Therefore, the comparison between the yawing and the single blade case is done rather from the shape of the curve than from the absolute values. The goal is to judge on the portions of the blade that contribute either to damping or to excitation.

Line 258: “blade tip of the blade” . remove one “blade” from that sentence.

Sentence has been rephrased to “Only the last monitored section at the blade tip does not inject power into the blade.”

Line 260: Could the authors comment on why the outer blade sections contribute to energy injection into the structure for the case presented in Figure 8 in contrast to Figure 4? Would this be enough to trigger a flapwise instability as well?

For the edgewise component, we added a source describing findings similar to those reported for the energy transition at the blade tip. Regarding the flapwise part, as shown in Fig. 2e and Fig. 12a, the first edgewise mode also contains a portion of flapwise deformation; thus exciting the edgewise mode also drives growth of the flapwise deformation amplitude. In Fig. 8b, there is a positive energy transfer at the blade tip in the flapwise direction. However, the frequency analysis of the flapwise vibration shows that only the first edgewise natural frequency is excited, not the first flapwise one. We therefore conclude that the positive energy in this direction acts only to amplify the flapwise component within the first edgewise mode. A statement was added to that paragraph to make it clearer (lines 313-315).

Additionally, Fig. 7 on page 14 has been extended to display the flapwise and torsional components in the time and frequency domains. We also added a statement indicating that, because the motion is driven by the edgewise frequency, the investigation focuses on that component. (lines 303-304)

Line 393 “by the by the”

The repetition has been corrected in the text.

Line 440: “The study presented describes an approach for predicting blade vibrations of multi-megawatt wind turbines under storm conditions. It is directly relevant to the simulation of extreme load cases, enabling more accurate prediction of peak aeroelastic loads and lock-in behavior under yawed inflow.”

This study seems to present an analysis of the instabilities that can occur while applying tools and methods that are referenced. If so, I am not sure the statement is totally accurate. Otherwise, the approach itself should probably take on a larger role in the paper. Please clarify

This type of load cases is commonly simulated with relative simple methods like BEM, which are actually not suitable for the analysis of complex aeroelastic phenomena.

The approach that we present is based on high-resolution models as described in chapter 2. These models allow the detailed insights gained in this paper.

Line 444: “Although being slower than pitching, yawing the turbine proved being effective in guiding the system to a stable state while reducing vibration amplitudes to roughly half its magnitude and thus offers a mitigation possibility when a fast pitch movement is restricted”

To my understanding, VIV is especially a problem in situations when the yaw motor is broken or deactivated for maintenance. Hence, this control option seems to have limited applicability.

As also stated in other comments, we have revised the motivation and conclusions to clearly reflect that we consider a storm scenario in which one blade fails to pitch to feather, and a load-mitigation strategy where the rotor is instead yawed so that the flow reaches the rotor directly from the side to minimize loads. We investigate the disadvantages of this control approach, which are relevant for both research and industry and have implications for the IEC standard, and highlight—now explicitly stated in the conclusions—that one key takeaway is the need for faster yawing to reduce the exposure time in the critical operating region (lines 530-531).