

Dear Reviewer,

We are grateful for your detailed and constructive comments, as well as for your positive and supportive feedback. We have tried to implement all suggested improvements and are convinced that the quality of the paper has improved considerably as a result.

Before reading this document, please first read our general response to the reviewers, provided as a separate document. It gives an overview of the main modifications and corrections made to the manuscript, to which several of the answers below refer. Below, you will find our point-by-point response to all your comments, together with an explanation of how each was addressed in the revised manuscript.

Many thanks for your effort and support,

Best regards,

Hendrik Verdonck and Oliver Hach

Review

first of all I would like to say that I think this is a great and extremely detailed manuscript.

However, in my opinion it would be easier to digest if it was split in 2 parts:

- Edgewise instabilities of a blade section part 1: Validation of attached flow unsteady airfoil aerodynamics modeling
 - Sections 2 and 3 of the article
- Edgewise instabilities of a blade section part 2: Analysis of the instability mechanism
 - Sections 4 and 5 of the article

This is of course just a suggestion.

I am not sure how the process for such a splitting would be at WES: reducing the length of the current article and resubmitting the second part?

The article is very clearly written, and the plots are very clear as well.

Apart from the above suggestion of splitting the article, I have some detailed comments for minor revisions below.

Thank you for this comment. We agree that a split into two parts makes sense and would make each paper clearer, with a single research question per paper.

Regarding the process at WES: a similar split was recently made for another WES paper (<https://wes.copernicus.org/articles/11/3273/2026/wes-11-3273-2026-discussion.html>). In that case, the split was suggested during the discussion phase; the revised manuscript contained both parts in a single PDF, was reviewed as a whole, and the actual separation into two papers took place only in the production phase. We contacted the handling associate editor and asked whether a similar procedure would be possible for our manuscript. He considered this meaningful and indicated that he would support it if requested by the reviewers (at the time of this exchange, he had not yet been able to read all comments in detail).

We have followed this procedure. Rather than shortening the current article and resubmitting the second part separately, the revised manuscript consists of two companion

papers that are reviewed together in this discussion and would be separated only in the production phase:

- Part 1: "Edgewise instabilities of a wind turbine blade section in attached flow conditions – Part 1: Comparison of CFD and potential flow theory results" (original Sections 1–4)
- Part 2: "Edgewise instabilities of a wind turbine blade section in attached flow conditions – Part 2: Flutter mechanism analysis" (original Section 5)

The split thus follows your suggestion, with one adjustment: the sensitivity analysis of the aerodynamic model components (original Section 4) was assigned to Part 1, since it directly extends the comparison of the aerodynamic models, while Part 2 is dedicated to the analysis of the instability mechanism.

We believe this split is the right way forward, especially since the revisions made in response to the review comments added approximately 25 pages of new material, which would have made a single paper even harder to digest.

Detailed comments

Responses to the reviewers' detailed comments are provided in bold, and changes made to the manuscript are highlighted in bold italics.

- page 2, line 36 'an edgewise flutter-like instability was observed during overspeed operation in experimental tests': I would probably write 'dedicated experimental tests' to highlight that the tests were targeted towards the instability

Thank you for this suggestion. We agree that "dedicated" clarifies that the tests were specifically targeted at the instability. The wording has been changed accordingly in both introductions (Part 1, Sect. 1; Part 2, Sect. 1).

- page 2, line 53-54 'They furthermore showed that the aerodynamic lift and moment caused by harmonic edgewise motion did not significantly change the flutter speed.': This description is not fully clear to me, can you extend it a bit?

We thank the reviewer for pointing this out. What we meant is that Kelley and Paquette (2020) extended their aerodynamic model so that the lift and moment also depend on the edgewise motion of the blade section, i.e., on the edgewise velocity and acceleration, in addition to the usual Theodorsen contributions for flapwise and torsional motion. The relevant equations from their paper are shown below:

$$L = L_{Theodorsen} + \frac{C_{l\alpha}}{2} \rho b^2 \alpha \ddot{y} + C_{l\alpha} \rho b W \alpha (1 + C(k)) \dot{y}$$
$$M = M_{Theodorsen} - \frac{C_{l\alpha}}{2} \rho b^3 \alpha \ddot{y} - C_{l\alpha} \rho b^2 W \alpha (a + \frac{1}{2})(1 + C(k)) \dot{y}.$$

In their notation, the y degree of freedom is in edgewise direction. In the terminology of our paper, these additional terms correspond to the Greenberg terms; in the formulation of Stäblein et al. (2017a), the same effect is captured by the edgewise velocity entering the velocity triangle.

Kelley and Paquette found that including or neglecting these additional edgewise terms had no significant influence on the predicted flutter speed. We have rephrased the corresponding passage in the manuscript (Part 2, section 1).

- page 3, line 74 'but additionally showing that this sensitivity depends strongly on the operating point.' Is that due to geometric edge-twist coupling due to the flapwise deflection?

Thank you for this question. Your assumption is plausible and likely contributes, but the operating-point dependence of the sensitivity cannot be attributed to the geometric edge-twist coupling alone.

The sentence you refer to contains two statements: first, that the damping of the first edgewise mode depends strongly on the structural edge-twist coupling – found in Stäblein et al. (2017a) and confirmed in Stäblein et al. (2017b) – and second, that this sensitivity depends strongly on the operating point (Stäblein et al., 2017b). Your question concerns the second statement, which is based on the following finding in Stäblein et al. (2017b):

"Damping increases for edge-twist to feather coupling when the pitch angle is close to zero {...} and reduces for edge-twist to stall coupling. For wind speeds where the blade is pitched, the damping of the edgewise mode reduces for edge-twist to feather and increases for edge-twist to stall coupling."

This shows that the effect of a given edge-twist coupling on the edgewise damping – and thus the sensitivity itself – can reverse with the operating point.

The difficulty is that several quantities change simultaneously when the operating point is varied: 1) the geometric edge-twist coupling due to the steady flapwise deflection changes, as you suggest; 2) the pitch angle changes; and 3) the steady angle of attack, and with it the steady aerodynamic load, changes. It is therefore very difficult to isolate the contribution of each individual effect, which is presumably why no conclusive statement was made in Stäblein et al. (2017b).

For this reason, the statement in our manuscript is deliberately kept general: it only states that the sensitivity depends on the operating point, without attributing it to a single mechanism.

- page 3, line 81: 'If the torsional motion leads flapwise motion, the flapwise force induced by torsional motion will perform positive work': maybe for clarity this could be extended to 'the flapwise force induced by torsional motion will be in phase with the flapwise velocity and thus perform positive work'

Thank you for this suggestion. We agree that this makes the mechanism easier to follow. The sentence has been changed as suggested in the introduction of Part 2.

- page 3: line 85 'in edgewise flutter, the torsional motion is introduced by the structural edge-twist coupling': I would think there is also - at least for a full blade - an aerodynamic coupling where edgewise motion causes the steady state flapwise force to move, which causes a torsional moment.

Thank you for this interesting comment. We agree that such an aerodynamic coupling exists, at least for a full blade. However, the intended meaning of the sentence is that the torsional motion in edgewise flutter is *primarily* caused by the structural edge-twist coupling. This is one of the main outcomes of this work: the edgewise flutter mechanism

persists even when the aerodynamic torsional moment is removed entirely (see Section 4 of Part 1 and the simplified models in Part 2). This differs fundamentally from classical flutter, which requires the aerodynamic torsional moment as a coupling mechanism. We have rephrased the sentence to convey this meaning more clearly; it now reads (Part 2, Sect. 1):

“In classical flutter, torsion is an independent dynamic degree of freedom that interacts with the flapwise mode through structural and aerodynamic coupling. In edgewise flutter, by contrast, the torsional motion is primarily introduced by the structural edge-twist coupling; additional aerodynamic couplings may exist but are not essential to the instability mechanism.”

- page 4, line 96: 'potential theory' => potential flow theory

Thank you for this correction. The terminology has been changed to "potential flow theory" throughout both manuscripts.

- page 4, line 111: 'This presented work serves a couple goals.' => The present work serves a couple of goals.

This part of the conclusion had to be rewritten completely due to the split in two papers.

- page 5, Figure 1: I think the collocation point and aerodynamic center should be defined somewhere (name and location)

Thank you for this comment. A clearer explanation of the model parameters and parameterization was also requested by other reviewers. The figure of the model has been completely revised. The collocation point and aerodynamic center are drawn in the figure, mentioned in the text and further elaborated in Appendix A of Part 1. Note one specific point here: the collocation point is located at the three-quarter-chord point, but the aerodynamic center is determined based on the steady CFD result. Because the airfoil is cambered, the aerodynamic center is not exactly at the quarter-chord point. The exact location is determined based on the steady CFD results and taken into account in the computation of the circulatory moment, as explained in Appendix A of Part 1.

- page 6, line 159: 'For realistic, full turbine systems, the edge-twist coupling is mainly driven by the out-of-plane deflection of the blade, either by prebend or by flapwise deformation under load.': I think this could also be mentioned in the introduction, to clarify where the edge-twist coupling could come from.

Thank you for this suggestion. We agree that clarifying the origin of the edge-twist coupling is helpful. The explanation has been added to both introductions (Part 1, Section 1; Part 2, Section 1).

- page 7, line 182: 'As the model is not perfectly linear': Maybe write 'airfoil properties are not perfectly linear' instead.

Thank you for this suggestion, it has been introduced as suggested.

- page 8, Section 2.2.1: What timestep was used?

Thank you for this comment. The other reviewers also requested more information on the computational setup, highlighting that this aspect was not sufficiently discussed in

the initial paper. Part 1, Sect. 2.2.1 now states the key settings, and the new Part 1, Appendix B provides a detailed overview of all CFD simulation parameters.

The minimal timestep is defined by the maximum reduced frequency to be accurately resolved. The maximum considered reduced frequency was 1, which results in a minimal timestep of 1.15×10^{-3} s. The convergence study of the transfer functions as a function of the maximum reduced frequency (and therefore minimal timestep) is shown in the CFD convergence studies (Part 1, Optional Appendix A).

- page 10, line 259 'effects due to the airfoil camber through both the tuned CL_α and the effective angle of attack': how is the lift slope tuned?

Thank you for this comment. It ties into the previous question on the position of the aerodynamic centers. As mentioned, a dedicated appendix (Appendix A in Part 1) has been added to explain exactly how the profile parameters are tuned based on the steady CFD results.

With respect to the lift slope specifically: a steady polar was computed with CFD between -5° and $+16^\circ$ angle of attack at 1° intervals (Fig. A1). As the airfoil properties are not perfectly linear, the local lift slope was evaluated at the two steady angles of attack used in this work (0° and 7°), giving the values summarized in Table 2 ($C_{L_\alpha} = 7.24$ at 0° and 6.60 at 7°).

- page 13, section 2.3: I propose to slightly change the title to '2.3 Summary and corrections of the methodology by Stäblein et al. (2017a)

Thank you for this suggestion. To address some of the concerns of the second reviewer, the formulations in this section have been rephrased slightly. The title has been changed to: '2.3 Summary and improvements of the methodology by Stäblein et al. (2017a)'

- page 15, line 395: I believe the components should be F_{yy} , $F_{y\theta}$, $M_{\theta y}$, $M_{\theta\theta}$

Indeed, thank you for that correction.

- page 16, Figure 4: In modern wind turbines, I believe the reduced frequency range for outboard parts of the blade would be roughly in the range of 0.05 to 0.1 for a first edgewise and maybe 0.1 to 0.2 for a second edgewise mode. Maybe it would make sense to mention this in the description of the figure, and to provide a plot ranging only from 0 to 0.2 in the appendix, where this very relevant area is visible in more detail?

Thank you for this suggestion. We agree and have implemented it as suggested: the reduced frequency ranges are now mentioned in the main text (Part 1, Sect. 3.1, now Figure 3) and Part 1, Appendix D (Fig. D1) shows a detailed zoom-in view of the transfer functions in the range $0 < k < 0.25$.

- page 16, Figure 4, $F_{x\theta}$: Might the increasing difference in the imaginary part for high reduced frequencies be due to neglecting the pitch rate squared term in the tangential force? (Equation 13 in <https://doi.org/10.5194/wes-7-1341-2022>)

Thank you for the comment. We can unfortunately not conclusively answer this question. We have checked this term, but we do not believe it can explain the discrepancy: the pitch-rate-squared term is second-order in the perturbations, so its linearization about the steady operating point vanishes, and it cannot contribute to the

first-order harmonic transfer functions (it would contribute only at the second harmonic).

Based on the observed curves, our own hypothesis is that a term proportional to ik is missing from the potential-flow model: the discrepancy in the imaginary part of $F_{x\theta}$ grows approximately linearly with the reduced frequency. Of particular interest in this regard is the newly added comparison with the NACA 0006 profile (Part 1, Appendix E, Fig. E1): the discrepancy is substantial for the thick, cambered FFA-W3-241 airfoil and considerably smaller for the thin NACA 0006 profile. The remaining offset for the NACA profile, however, also appears to be linear in k . Note, however, that the discrepancy is opposite; for the FFA-W3-241 profile the Theodorsen result has a larger negative imaginary part at high reduced frequencies. For the NACA profile the CFD result is larger at high reduced frequencies. These observations lead us to believe that the missing term is linked to the camber and thickness of the profile, which are only partially represented in the potential-flow model.

We cannot conclusively identify the underlying cause, which is why the manuscript states that this discrepancy remains unexplained (Part 1, Sect. 3.1).

- page 16, line 397 'An even better agreement was found for a comparison with a thin, uncambered NACA 0006 profile.': Could an equivalent of Figure 4 for the NACA 0006 be included in an appendix? Also, Bergami et al have investigated changes of the circulatory response due to changing airfoil thickness in <https://doi.org/10.1002/we.1516>
- Thank you for this suggestion. We agree that this additional comparison adds value to the paper. We have therefore added it as Appendix E of Part 1, together with a description of the results; the figure shows a better agreement between the CFD-RANS results and the Theodorsen-based approach for the NACA 0006 airfoil for all transfer functions.
- Thank you also for the reference to Bergami et al. (2013). We agree that there are approaches for a better tuning to get an even better agreement between the potential flow theory-based methods and CFD reference result for thicker airfoils. This has now been noted in Part 1, Appendix E. However, we decided to keep such an improved tuning out of the scope of the present work. The potential flow-theory results are therefore purely the thin-plate unsteady airfoil formulations, improved only with a tuning of the profile parameters which can be derived from the steady CFD result. This work could serve as a starting for future studies investigating the effects caused by the profile thickness and the best possible correction of the potential flow theories in more detail.
- page 19, line 477: 'In the six selected cases': Maybe you could write six selected coupling combinations? I would argue that it is 12 cases, 6 coupling combinations and 2 steady angles of attack.

Thank you for this suggestion. Indeed, the study comprises 12 cases (six coupling combinations at two steady angles of attack), so your formulation is more precise. The wording has been adapted in Part 1 (Sect. 3.2), and the corresponding passage in Sect. 3.3 has been harmonized.

- page 25, table 3: 'Theodorsen - without steady load': maybe you could briefly describe how this is achieved in the model equations?

Thank you for this suggestion. It is achieved by setting the mean steady circulatory lift $\bar{L}_C$ (Eq. 39) to zero; since $\bar{L}_C$ appears only in the circulatory force direction-variation contribution, this sets all terms of the corresponding matrix ($A^{C,direct}$, Eq. 46) to zero. Thanks to the restructuring of the derivation, this contribution is now isolated in a dedicated section (Part 1, Sect. 2.2.2, "Circulatory force direction variation"), which makes the affected equations easy to identify. In the table we mentioned this by a brief statement and reference to the equations.

- page 26, 27, Figures 11 and 12: using different types of lines (solid, dashed, dash-dotted,...) might be helpful in figures 11 and 12 to identify cases where several lines are on top of each other

Thank you for the suggestion. We agree that some of the lines were not well visible. We used your suggestion and used a dotted line for the variation of the CFD result and dashed lines for the variations of the potential flow theory-based results. Additionally, we slightly increased the overall size of the figure and the relative size of the damping diagram compared to the frequency diagram. There are still a lot of lines in one diagram, but it should now be possible to differentiate the individual lines, at least in the damping diagram.

- page 28, line 561: 'edgewise induced forces': I understand what it means, but somehow the term is not totally clear to me. Maybe calling it edgewise-motion-induced forces or forces due to edgewise motion would be more clear?

Thank you for the suggestion. We agree that the word 'motion' makes the term clearer. The terminology has been changed to "edgewise-motion-induced forces" in Part 1 (Table 3, Sect. 4, and the figure labels).

- page 29, Equations (42) to (43): could this reduction from 3 to 2 DOF be shown in more detail? for example, how is B chosen for the different cases that will be investigated in the following?

Thank you for this very good question. In answering it, we found that the reduction could be improved substantially; the revised manuscript now uses an improved 2DOF model (Part 2, Sect. 2.4, Eqs. 9–11).

To answer your question directly: the parameter B is no longer required. The original derivation imposed a direct kinematic link between edgewise and torsional motion, $x = B\theta$, with B taken as the edge-torsion ratio of the structural mode shape, while additionally neglecting the structural coupling terms in the reduced stiffness matrix, an overly strong simplification. The new derivation is based on a Guyan reduction (static condensation): it neglects only the torsional inertia, which is well separated from the bending frequencies, and takes all structural couplings into account. The twist is then determined by the static equilibrium of the torsion equation, $\theta = -(k_{x\theta}x + k_{y\theta}y)/k_\theta$ for all investigated cases.

The consequences are as follows. Without structural flap-twist coupling ($\gamma_y = 0$), the new formulation recovers the original direct link between edgewise and torsional motion, and the results are unchanged. With structural flap-twist coupling ($\gamma_y \neq 0$), the improved model agrees much better with the 3DOF result than the original formulation, which failed for these cases.

We refer to the main response document (point 12), where a more thorough description of the new derivation and its implications is given.

- page 31, line 635 'moving from -360° (out-of-phase with torsion) to -180° (trailing torsion)': I guess -360° would be in-phase and -180° would be out-of-phase?

Thank you for this comment. You are right, the formulation was not correct. It has been reformulated and detailed to:

“It starts at -360° , i.e. in phase with torsion, passes through -180° i.e. fully out of phase with torsion, at approximately 50 m/s, and then gradually moves back towards being in phase with torsion as the relative wind speed increases, with torsion leading the flapwise displacement.”

- page 32, line 660 'altogether, these diagrams illustrate a straightforward mechanism': I just wanted to say that I think this is a great paragraph!

Thank you for this positive feedback.

- page 33, Figure 14 h): is the flapwise force amplitude due to torsion exactly on top of each other for the 2DOF and 3DOF model? If this is the case, please mention it

Thank you for this observation, which is correct. Because the 2DOF model has been improved in the revised version, the 2DOF and 3DOF curves often lie directly on top of each other. This is now pointed out in the discussion of the first figure, where both results are introduced.

- pages 33 and 35, Figures 14 and 15: maybe the total aerodynamic work line thickness could be reduced to make sure that they don't hide other lines

Thank you for this suggestion. Showing all components in a single diagram was challenging, as we wanted to keep it readable and use the same axis ranges as the other damping diagrams for direct comparison. In the revised figure, the total aerodynamic damping is plotted in the background so that its components are drawn on top of it and are no longer covered by it.

- page 36, line 713: 'The simplified 3DOF model is still a good approximation of the full CFD reference result.': I am not sure I can agree with this. The 'critical speed' in the CFD result is at about 90 m/s, while the 3DOF simplified model predicts maybe 50 m/s (difficult to see)

Thank you for this comment. We agree and have revised the analysis accordingly. The simplified models capture the main destabilizing mechanism but miss the additional stabilizing contribution of the edgewise force component. This contribution raises the total damping and delays the onset of flutter. The revised passage now reads:

“The simplified models capture the most important destabilizing mechanism well. {...}. However, the simplified models do not reproduce the increase in damping between approximately 40 m/s and 90 m/s. In this range, the CFD reference result shows a stabilizing contribution of the edgewise force, visible in the edgewise aerodynamic damping component in subplot (c) (solid line). This contribution is missing in the simplified models, which can be traced back to the difference in the phase of the edgewise force in subplot (g) (solid line). The simplified models therefore underestimate the flutter speed in this case.”

- page 36, line 717 'Edge-twist coupling to feather ($\gamma_x = -0.3$)': I believe this should be: 'Edge-twist coupling to stall ($\gamma_x = 0.3$)'

Thank you, this has been corrected.

- page 48, line 907, reference for Li et al. (2025): Sometimes in the text, I believe you refer to this, when you intend to refer to Li et al. (2022). Please double check.

Indeed, thank you for catching this mistake. We reference now to the correct Li et al. (2022) paper and to the new Li et al. (2026) preprint.