

Dear Reviewer,

We sincerely thank you for your insightful and constructive comments. We have tried to implement all suggested improvements and believe that addressing your comments has improved the paper considerably, in particular its readability for a wider audience. We also hope that our responses clarify the points that were unclear.

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.

Best regards,

Hendrik Verdonck and Oliver Hach

Review

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

The paper under review presents a method to verify the occurrence of flutter by focusing on edgewise instabilities of a wind turbine blade section. However, the simplified aerodynamic model included only covers situations involving a attached flow, which means that only a subset of edwise instabilities can be analysed using it. It is essentially limited to 2D flows and 3 DOFs. The authors carefully present the details of their method, compare it against a CFD-based method to recover the aerodynamic transfer functions, and benchmark it with a literature reference from recent years. The manuscript provides a good, comprehensive overview of the current state of research and cites relevant publications. The aims of the work are clearly stated. The authors go into great detail as to identify which aerodynamic effects can be ignored, for their specific case, without significantly impacting the quality of their results. A reduced order model is developed for this purpose that takes into account only the relevant effects. Overall, the paper presents the reader with an extreme ammount of information which makes it somewhat difficult for the authors to adequately cover all that gathered information and understand the multiple perculiarties of each method and to, on top of that, cover the impact of each significant model parameter. The authors notably took great care to cover and discuss most observations that can be made by looking at the various plots, and doing more analysis would make the paper even heavier for the reader, but perhaps some more focused analysis could be given, or some plots abandoned. Nonetheless, the plots and equations are very well presented and easy to read and understand.

We thank the reviewer for this assessment. We are pleased that the paper is assessed positively overall, and we understand the criticism that the reader is presented with a very large amount of information. This makes it difficult for us to explain every detail and for readers to keep track of the overall picture. To address this issue, our main change was to split the paper into two parts. The first part is dedicated to assessing the potential flow model for this instability mechanism. The second part focuses on improving the understanding of why this mechanism occurs in the first place. In our opinion, this split has made the structure of each paper clearer and more digestible, and we hope that it at least partly addresses your concerns.

Apart from splitting the paper, however, we were not able to focus the analysis or remove plots. Most of the reviewers' comments requested additional information and analysis rather than less. Additional verification is therefore documented in a new (optional) appendix document (*“Optional appendices to: Edgewise instabilities of a wind turbine blade section in attached flow conditions – Part 1: Comparison of CFD and potential flow theory results”*).

Also, the authors include an Appendix to discuss the impact of the moving reference frame (namely the impact of torsion along the span on the model's response); it is however confusing because the authors mention that the aircraft's reference frame rotates as the aircraft pitches, but that for the wind turbine airfoil analysis of this paper, the reference frame does not rotate with elastic deformation. So the reader wonders, a) the authors want to say that the ref frame of the wind turbine airfoil remains unchanged regardless of both blade torsion and pitching? and b) the ref frame of the airfoil of an airplane wing will pitch with the airplane but won't rotate with torsion, or will rotate with torsion? The text there should be made clearer.

Thank you for this comment. This difference is not simple to explain. This is also why we tried to break it down as much as possible and exemplify it with the elastic wing vs. rigid-body aircraft comparison. However, your comment points out that our explanation was not clear enough.

The essential distinction is not between wind turbines and aircraft, but between elastic deformations and rigid-body motions. Rigid-body rotations define the orientation of the reference frame, whereas elastic deformations are measured relative to it. Regarding the reviewer's questions:

- **For the wind turbine blade section, the reference frame does not rotate with the elastic torsion of the blade, but it would rotate with a rigid-body pitch of the blade. In this study, blade pitch is not a degree of freedom but part of the steady operating point (the steady angle of attack α_0). The torsional degree of freedom represents elastic torsion only, so the reference frame remains fixed.**
- **For an aircraft wing, the airfoil reference frame rotates with the rigid-body pitch of the aircraft, but not with the elastic torsion of the wing.**

Another way to explain it would be to consider what the structural restoring forces are anchored to. In an elastic structure, the edgewise and flapwise restoring forces are provided by the surrounding structure, and ultimately by the blade root, which does not follow the local elastic twist of the section. The stiffness directions therefore remain fixed. In a moving reference frame, the edgewise stiffness would instead rotate with the local section, so an elastic twist of the section would change the direction in which its edgewise mode vibrates, which is not physical for an elastic blade. The classical wind tunnel typical section, a rigid airfoil on springs attached to the tunnel walls, illustrates the same principle: the translational springs act in fixed directions, regardless of the pitch angle of the airfoil.

We have rewritten the first part of Appendix C accordingly. It now states this distinction explicitly, treats both the aircraft and the wind turbine case in this way, and adds the classical wind tunnel typical section as an intuitive example. We have also extended the caption of Figure C1.

There are also a few points we believe should be addressed by modification/clarifications to/in the text of the paper.

As the mathematical derivations and stability analyses fall somewhat outside my primary area of expertise, I was not able to verify every step of the analysis in full detail, and I therefore cannot rule out the presence of subtle technical errors in the modeling or in the conduct of the stability analyses. Based on my overall assessment of the manuscript, however, I recommend acceptance subject to minor revisions.

I hope those comments can provide a basis to help the authors improve the paper for its final version.

Detailed comments

- *General:* the terms "low-fidelity" and "high-fidelity" do not seem entirely appropriate here; these are simply different approaches based on different assumptions

Thank you for this comment. We agree that the ‘low-fidelity’ vs. ‘high-fidelity’ terminology is not the best choice here. We exchanged it throughout for ‘potential flow theory’ vs. ‘CFD’, or the plots ‘Theodorsen’ vs. ‘CFD’.

- *General:* "to adjust some model parameters" (Page 7, line 179) is very unspecific; colloquial expressions of this kind appear repeatedly throughout the text and clarity would be advisable

Thank you for this comment. We agree with this assessment and have made the wording more precise. In this specific sentence, “some model parameters” was replaced by “the steady airfoil properties”, which is immediately followed by a list of the specific properties.

Furthermore, we went through the entire paper and used more precise and direct wording wherever possible.

- *line 96:* "Similarly, most investigations on edgewise instabilities in nominal operating conditions have been done with models of this level of fidelity. An exception are the recent studies by Hermes (2024); Hermes et al. (2025), who compared the stability analysis based on CFD aerodynamics with different low-fidelity BEM methodologies for the IEA 10 MW turbine in operational conditions. The observed differences in the damping for the edgewise modes were relatively small in operating conditions with axially aligned inflow, larger differences up to 30% were shown for yaw misaligned conditions."
- it is unclear whether the quantified differences are between "CFD aerodynamics" and "low-fidelity BEM" or something else; please reformulate

Thank you for pointing out this ambiguity. The quantified differences refer to the edgewise damping predicted with CFD aerodynamics compared to that predicted with the low-fidelity BEM methodologies. We have reformulated the passage to state this explicitly. The important modification to the original is “... *The differences in edgewise damping between the CFD-based and BEM-based predictions were relatively small ...*”

- *line 111:* "This presented work serves a couple goals." - grammar

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

- *line 154* starting with "with the uncoupled edgewise, flap" should be unindented

Thank you for this correction. This was the case around equations in many locations of the paper. This has been corrected throughout.

- *line 174: "Secondly, stability analysis results with a much lower angle of attack of 0° will be shown, to investigate the effect of the steady load. " - this doesn't fit well with previous sentence, would suggest rewrite the subparagraph*

Thank you for this comment. We agree that the flow of this paragraph was not good. We have rewritten it to present the two investigated angles of attack in a clearer and more parallel structure, with the purpose of each case stated directly. The modified paragraph is then:

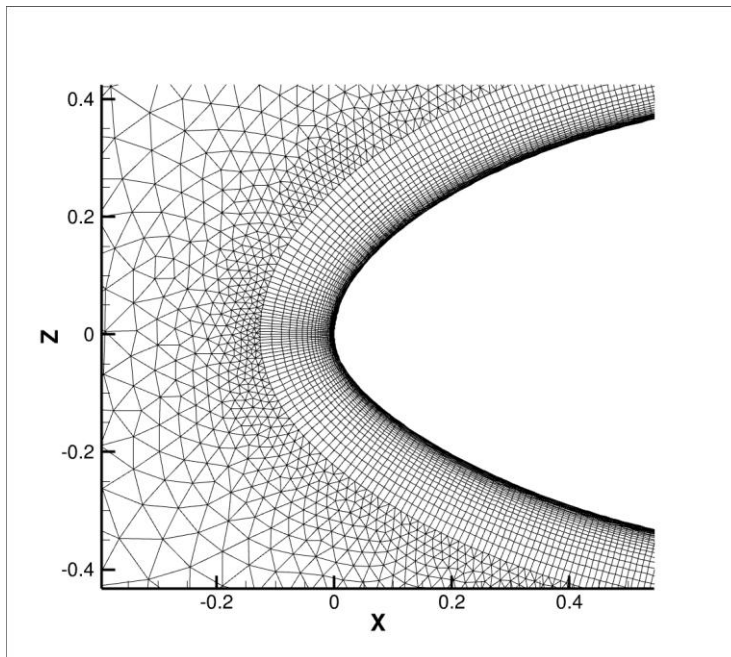
"Two operating angles of attack are investigated in this work. The first, 7° , is representative of a design angle of attack in the outboard section of a wind turbine rotor blade. For the DTU 10 MW reference turbine, for example, it approximately corresponds to the angle of attack at 75% blade radius for a wind speed of 8 m/s and an axial induction factor of 0.3. The second, 0° , serves as a case with a considerably lower steady aerodynamic load, in order to investigate the influence of the steady load on the stability results. Note that, because the airfoil's zero-lift angle of attack is negative, the steady lift at 0° is reduced but not zero."

- *Figures 1 and 2: It is somewhat confusing to see two figures defining the same model but using slightly differing positions of the axes.*

I think the two figures should be presented as subfigures instead, so that the reader can quickly see the differences between both. Alternatively, Fig. 1 could also be removed, since it seems it's only purpose is to present the "ba" parameter as a positive value before making it negative in Fig. 2. Would it not make more sense to also give e_{cg} in non-dimensional form (e_{cg}/b) in the captions of those figures? Neither b nor c is given here, so the reader cannot directly picture where the centre of gravity is located. The length a is given non-dimensionally as well.

Thank you for this comment. Note that a comment by another reviewer prompted a complete revision of this figure. We understand that it is confusing to see two figures defining the same model. In our opinion, the first drawing is essential for clearly and unambiguously defining the positive directions of all involved parameters. We have therefore decided to remove the second figure. We have also adopted your valid suggestion that it is better and more consistent to define the center of gravity in non-dimensional form.

- *Fig 3: can you explain why the nose boundary layer quadrangle mesh is not complete? (i.e. why does it switch so quickly to triangles?*
- **Thank you for this question. Unfortunately, we cannot give a fully satisfactory answer. We use the commercial meshing software Centaur (<https://home.centaurosoft.com/>), which generates the mesh starting from the body surface and building outwards: first the structured boundary layer mesh is created, and then the unstructured farfield mesh is grown from it. For the chosen boundary layer settings, the program did not extend the structured boundary layer mesh fully, presumably because the corresponding cells would have been too strongly stretched. When a larger number of boundary layer cells is used (75 instead of 54 layers), this effect disappears:**



- However, the CFD mesh sensitivity study showed that this effect has no influence on the transfer functions.
- and also, can you explain/justify why was the O-Type boundary layer mesh not extruded until reaching a circular farfield mesh instead of switching to unstructured mesh?

Thank you for this question. It is a practical solution. Our flow solver, Tau, is a hybrid solver. In this setup the structured boundary layer mesh provides a good resolution of the boundary layer, while the unstructured growing towards the far field ensures there are no highly stretched cells in the farfield.

Note that we relied strongly on the experience of the CFD experts in our institute for the setup of the CFD mesh and solution parameters, as also mentioned in the acknowledgements. In their experience, this topology provides the most robust and accurate results for the combination of Centaur as the meshing software and Tau as the flow solver.

Additional CFD convergence studies were performed; their outcome is summarized in Sect. 2.2.1, and the detailed appendix (Opt. Appendix A) is available separately. The aerodynamic transfer functions were insensitive to the mesh discretization, which might not be surprising for the relatively simple aerodynamic flow conditions simulated in this work.

- line 395: this should read F_{yy} , $F_{y\theta}$, $F_{\theta y}$, $M_{\theta\theta}$ in order to be consistent with the transfer function of the coefficients (Equation 6). This is presumably just a typo

Thank you for this correction. It was indeed a typo; it now reads F_{yy} , $F_{y\theta}$, $M_{\theta y}$, $M_{\theta\theta}$, consistent with the moment row of the transfer-function matrix (Eq. 6).

- line 448: "Divergence implies that for one of the modes the frequency fell to zero and the damping to increased to infinity."
- this needs a grammar check (damping is negative or positive in that case? and must it be infinity or is this just a numerical artefact of calculating it?) Please clarify this in the text,

especially because just before all "flutter cases" are with negative damping and this is then confusing for the reader.

Thank you for this comment. The original statement was both confusing and not entirely correct. The fact of the frequency falling to zero and the damping increasing to infinity is more a symptom than an actual description or check for divergence. Divergence is a static instability. The motion becomes non-oscillatory, i.e. an exponentially growing deflection without any vibration. All results in the paper give the damping as critical damping ratio, defined relative to an oscillation. For an eigenvalue $p = g + ik$ the damping ratio is defined as: $\zeta = \frac{-\Re(p)}{|p|} = \frac{-g}{\sqrt{g^2 + k^2}}$. This was not explicitly stated in the paper, but has now been added to Sect. 2.2 in Part 1 and Sect. 2.5.1 in Part 2. For a non-oscillatory motion, it has no meaningful value, which is exactly why the original formulation was misleading. Besides this, it was also not how the divergence speed was actually computed. The frequency domain flutter methodology is formulated in terms of the reduced frequency and covers oscillatory motion only. It cannot cover divergence. Therefore, the divergence speed was computed separately by inserting $p = 0$ in the flutter equation and determining at which speed the aerodynamic contribution cancels the structural stiffness. This should have been explained better in the paper and has now been added to Sect. 2.2 of Part 1.

So altogether this has been improved in the document by:

- Adding the explanation for the divergence check in Sect. 2.2 of Part 1.
- Adding the statement that all damping values shown in the paper are the damping ratio relative to the oscillation frequency.
- The original statement has been replaced by: *“Divergence implies that the section becomes statically unstable, as determined by the static check described in Sect. 2.2.”*
- line 399: "In the Stäblein et al. (2017a) implementation, the Jones constants in the unsteady aerodynamic formulation could be tuned to [...] Therefore it was decided to stick with the flat-plate Jones constants."
 - can you cite the Jones constant reference? (do you mean The Unsteady Lift of a Wing of Finite Aspect Ratio NACA report from 1940?)
 - does that mean that the "Stäblein et al. (2017a) - corrected" results are all conducted with the $C(k)$ parameters values of Jones? Please clarify.
- Thank you for this comment. That is indeed the original source for the Jones approximation of the Theodorsen theory. We referenced it in the text.
- Yes – all „Stäblein et al. (2017a)“ – improved results are obtained with the standard flat-plate Jones constants (see point 7 of the general response).
- The second question was really important. The initial paper did not properly explain the differences in the stability analysis approach between the g-method used in this work for the CFD and Theodorsen results and the direct eigenvalue analysis approach used to get the Stäblein et al. (2017a) – improved results. Neither did it explain how the aerodynamic transfer functions were computed based on that implementation. A detailed explanation of all these points is added as Appendix F in Part 1. A brief summary of this is given here: The Stäblein et al. (2017a) – improved methodology uses the Jones approximation and linearization to be able to write the

whole system as one state-space system. The stability analysis can then be done by a direct eigenvalue analysis on the state matrix. That methodology does not directly rely on a frequency domain transfer function. The frequency domain transfer function had to be derived to be able to compare the aerodynamic implementation in that work with the two methodologies introduced in this work. This derivation can be done analytically, as explained in Appendix F. It is also shown that this difference in stability analysis approach does not affect the flutter points or the damping of the edgewise mode.

- *line 754: "a low-fidelity aerodynamic model"*
are you referring to the aeroelastic model you extended from Stäblein, if so then it would make sense to name it a such (i.e. maintain the name you used in all the plots)

Thank you for this suggestion. We rephrased this sentence, so that there is a direct connection to the name used in all the plots. The sentence in the conclusion now reads:

"... was analyzed with three aerodynamic models: a CFD reference model, a Theodorsen-based potential flow theory model derived in this work, and the potential flow theory based literature reference by Stäblein et al. (2017a), whose methodology was refined in several respects in the course of this work"

- *line 168: "The model parameters used in this study are for the most part identical to those defined in Stäblein et al. (2017a), and are listed in Table 1. "*
- this is quite vague, can you mention which ones differ so that the reader is not constrained to go read the Stäblein paper to understand which parameters you changed?

Thank you for this comment. This sentence was indeed too vague, especially because all parameters were identical to those used in Stäblein et al. (2017a). The "for the most part" has been removed.

- *line 215: "The DLR in-house tool Tau is used as CFD solver."* - can you give a precise version number and/or literature reference?

Thank you for this comment. To address this, we:

- added a literature reference to the most recent overview paper of Tau,
 - updated the sentence to say that Tau was used in the FlowSimulator environment
 - added a footnote to the webpage of FlowSimulator (the Software integration around Tau) and Tau
 - added the used version number of FlowSimulator.
- can you give a few more numerical parameters used for the simulation (number of iterations, convergence criteria, y^+ value, time step, etc)?

Thank you for this comment. A better overview of the main numerical parameters is given in the main body of the text and a detailed overview is given in Part 1, Appendix B.

To your direct questions:

Number of iterations / convergence criteria: The initial steady simulation was run until the density residual decreased by 10 orders of magnitude. An additional check was performed to verify that there were no visible fluctuations in the aerodynamic coefficients anymore. The unsteady pulsed simulations were computed with 51200 time steps, with each containing 2000 pseudotime iterations.

- The maximum y^+ value was 0.13
- The minimal time step was 1.15×10^{-3} s
- Has a grid convergence study conducted?

Yes, a grid convergence study was conducted, which was extended to address the comments of all reviewers. The results of these convergence studies can be found in Part 1, Opt. Appendix A. To limit the size of the paper, we suggest not to include this appendix in the final version of the paper, and to just mention the conducted convergence studies and their outcome.

- How was the step function for the motions realized? Was the airfoil abruptly accelerated, and over how many time steps did this take place? Or was an unsteady inflow specified? What were the magnitudes of the Δ 's ($\Delta \alpha$, $\Delta x/y$) for the impulse motion?

The pulse motion was realized by a motion of the profile spread over eight time steps. The magnitude of pulse motion was 1×10^{-5} m for $\Delta x/\Delta y$ and $1 \times 10^{-5}^\circ$ for $\Delta \alpha$.

Appendix B in Part 1 contains further information on the CFD setup. We hope that this answers all relevant questions.

The setup of the pulse motion and signal parameters was part of the CFD verification study. The convergence of the pulse amplitude, minimal timestep and number of pseudotime iterations is shown in Part 1, Opt. Appendix A. Besides this, an additional verification with harmonic excitation at 5 individual reduced frequencies was done to compare with the continuous transfer function obtained with the pulse excitation, as also shown in Fig. Opt. A3 in Part 1, Opt. Appendix A.

For more information and dedicated verification of the general procedure, we would like to refer to Kaiser et al. (2015).

- *Figure 5:* - the simplest solution would be to label the unstable regions directly in the plot. It is evident that blue = unstable; simply adding "Classical Flutter", "Edgewise Flutter" and "Static Divergence" would help (as done in Figures 6 and 7)

Note that Figures 5, 6 and 7 of the initial manuscript are now Figures 4, 5 and 6 in the revised version. We refer to the new figure numbers in our answer.

Thank you for this suggestion. We understand the idea and its potential benefit. However, we would prefer to keep the current layout, for two reasons:

- **Figure 4 shows something slightly different from Figures 5 and 6. Figures 5 and 6 show the lowest critical inflow speed and indicate the corresponding instability mechanism. Figure 4, in contrast, shows the actual stability boundaries of the individual mechanisms, which leads to regions where several mechanisms overlap. For example, the top left corner of the left plot lies beyond both the edgewise and the classical flutter boundary, meaning that there are two modes unstable there. This can be seen at high wind speeds in the top left plot of Figure 7. Labeling the regions with a single mechanism would therefore be ambiguous, which is why we prefer to show and label the boundaries of the individual mechanisms rather than label the areas.**
- **The same figure format was used in the original publication by Stäblein et al. (2017a). Keeping it allows readers of both papers to compare the results directly.**

We are nevertheless willing to change the figure if you consider this necessary, but we believe that the current format is the clearest option.

- *Figs. 5, 6, 7:* -In the figure captions where flap-twist coupling and edge-twist coupling parameters are the x-axis, it would help the reader if the variable name was also given in the axis title (e.g. γ_y , γ_x)

Thank you for this suggestion. That does indeed improve clarity. The variable names have been added as suggested (now Fig. 4-6).

- *Line 390:* I would not denote the agreement between the simplified aerodynamic model and the CFD results as good. For example, in the case of F_{yx} , there are significant discrepancies in the real part; that is, the lateral force is predicted incorrectly during edgewise motion, which should potentially affect edgewise vibrations. Although the individual components are discussed subsequently, no attempt has been made to identify the causes of the occurring deviations. I would have liked to see a more in-depth analysis here.

Thank you for this comment, we can understand your opinion. First of all, note that the F_{xx} and F_{yx} components were improved by correcting two mistakes, one in our own formulation and one in the software implementation of Stäblein et al. (2017a). These corrections are discussed in points 4 and 5 of our general response to the reviewers.

Nevertheless, some unexplained differences remain. A considerable effort went into understanding the remaining differences and improving the potential flow models. This is reflected in the several improvements we made to the published literature reference, each of which improved the agreement between the potential flow models and the CFD result. To the best of our effort and knowledge, the potential flow models used in this work have been derived and implemented correctly.

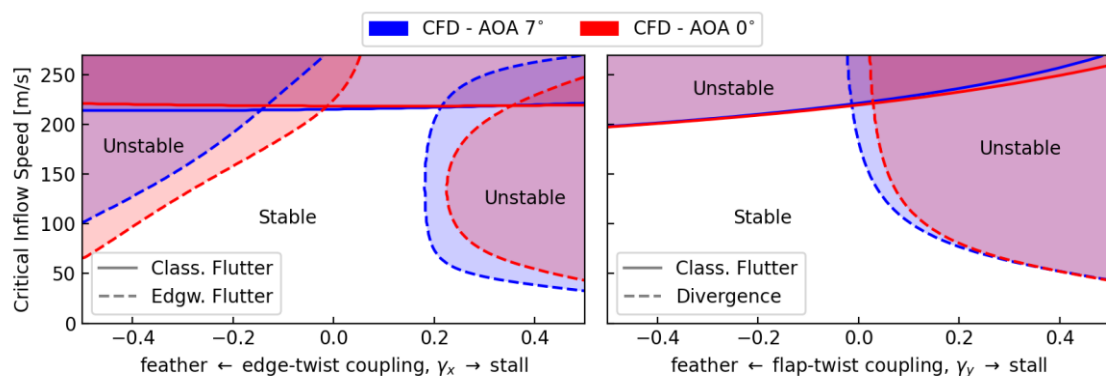
Regarding the possible effect on edgewise vibrations: this is exactly the question the sensitivity study in Part 1, Sect. 4 is intended to answer. We also investigated directly how the individual discrepancies in the transfer functions affect the stability results. However, since the stability results of the potential flow models and the CFD reference already agree very well, this provided little additional insight. The sensitivity study in the paper therefore applies much larger modifications to the aerodynamic model, removing entire groups of aerodynamic terms. Most relevant to your comment is the modification in which all forces induced by edgewise motion are omitted, i.e., the complete first column of the aerodynamic matrix, including F_{yx} . Even this drastic change alters the damping of the edgewise mode and the flutter point only quantitatively, while the qualitative trends remain similar. Other modifications, such as neglecting all pitching moments, the non-circulatory forces or the unsteady lag effect, have an even smaller influence. This altogether shows that the edgewise instability mechanism is relatively insensitive to many of the aerodynamic components.

We acknowledge that a full explanation of the remaining differences in the aerodynamic transfer functions is still missing. We consider this an interesting topic for future research. To reflect your comment, we have also toned down the wording in the manuscript and clearly stated where differences still exist.

- *Line 458f / Fig. 5:* The authors state that the critical inflow speed drops significantly as the steady AoA increases. I cannot see this when comparing Fig. 5(a) and 5(b).

Thank you for this comment. Note that the contour plots (now in Figure 4) changed slightly due to the corrections made to the aerodynamic model implementations, but the overall observation remains the same. The statement in question is: “For edge-twist coupling to stall, the critical inflow speed drops significantly with increasing steady angle of attack.” Comparing the edgewise flutter boundaries for positive γ_x in the left plots of Figure 4 shows that, at 7° angle of attack (top), the unstable region is larger and the flutter boundary lies at lower critical inflow speeds than at 0° (bottom). To make this clearer, we have included below a direct comparison of the CFD results at 0° and 7°.

The decrease in critical inflow speed from 0° to 7° is clearly visible, but we have removed the word “significantly” from the sentence.



- *line 492:* when commenting figure 8 the authors wrote "A qualitative difference is observed in the damping of the highly-damped flapwise mode. This is expected, as the flutter analysis using the g-method is exact only for small damping values, and is only an approximation for higher damping levels." does this refer to the fact the Theodorsen results strongly differ from the CFD and Stäblein ones beyond approx. 200 m/s where on the plot's bottom row some methods show edgewise flutter and others flapwise flutter. Can you expand on this because it sort of disagrees with "The results from all three methods are in strong agreement regarding the frequency and damping of the flap and torsional modes."

Thank you for this comment. It showed us that we had to elaborate this point better. A very detailed description of this statement is now given in Part 1, Appendix F. We also agree that the original statement was unclear and partly contradicted the preceding sentence. What we meant is that the remaining differences in the damping of the highly damped flapwise mode are mainly caused by the stability analysis approach rather than by the aerodynamic model. The CFD and Theodorsen results are obtained with the g-method, which is exact only for zero damping and an approximation for higher damping levels. The Stäblein et al. (2017a) results are obtained with a direct eigenvalue analysis, which is exact for the underlying model at all damping levels. To support this, we have added Appendix F, in which both stability analysis approaches are applied to the identical aerodynamic model, so that the effect of the solution method is isolated. We have also rephrased the corresponding passage in Section 3.3 (note that Figure 8 of the initial manuscript is now Figure 7). It now states that all methods agree well on the critical flutter speeds, the overall mechanisms and the damping at low damping levels, and that the remaining differences for the highly damped flapwise mode, including its

critical damping in the Stäblein et al. (2017a) results, are due to the stability analysis method.

- *line 578:* " the center of gravity offset relative to the elastic axis was removed" - is confusing because e_{cg} still appears in the M matrix below, can you rephrase?

Thank you for this correction. The text was right, the equation erroneously contained e_{cg} , this was a typo. The center of gravity offset was removed to further simplify the system because it is not essential for the edgewise flutter mechanism. The paragraph is now in Part 2, Sect. 2.1.

- *line 665:* "The effect of the flapwise force induced by torsional motion depends on the phase angle between flapwise motion and torsional motion (subplot (e)). The phase between flapwise motion and torsional motion shifts from out-of-phase towards torsion leading flapwise displacement. This causes the phase between the flapwise force induced by torsional motion and flapwise velocity to shift from out-of-phase to fully in-phase at approximately 150 m s⁻¹ (subplot (i)). "
- it is not entirely clear from this explanation why at about 150 m/s there is a sudden 180° shift of the phase of the "Simplified 3DOF model" (edge) line of plot 14(i), especially considering that the "CFD" (edge) line on the same plot rather simply further decreases its phase. This makes the reviewer think there was a 180° error in the angle unwrapping used to plot 14(i)

Thank you for this comment, which points out something we should have explained more carefully. The explanation is simple: the simplified models contain no aerodynamic term in flapwise direction induced by edgewise motion, so this force component is zero. Since its amplitude is zero, the phase computed by the analysis script for this component is meaningless. This should at least have been mentioned in the text. To avoid any confusion, we have removed this component from the plots for both simplified models and state in the text that it is omitted because it is zero.

- *Figure 9:* - could the caption state that this is a detail view of Figure 8? That would be sufficient; no further information is needed there

Thank you for this suggestion, that will indeed increase clarity. We updated the caption of Fig. 8 (initially Fig. 9) to: "Detail view of Figure 7, focusing on the edgewise mode."

- *Pages 26 and 27:* - could this be reduced to the models that are strictly necessary? It is not clear which aspects of the figures the text is meant to explain

Thank you for this suggestion. The presented selection is already a reduced subset of a larger set of models and sensitivities that we investigated. We believe that each of the remaining modifications provides valuable insight—either because it significantly impacts the stability prediction (e.g., omitting the steady load) or because its impact is unexpectedly small (e.g., omitting the pitching moments altogether).

We have nevertheless tried to make this section (Part 1, Sect. 4) easier to follow, mainly by improving the readability of the figures themselves. Different line styles are now used, the overall size of the second figure has been increased, and the damping subplots have been enlarged relative to the frequency subplots.

We understand that this section remains extensive, simply due to the number of models and modifications presented. However, we believe that it contains valuable information for specialist readers and may anticipate questions they would otherwise have.

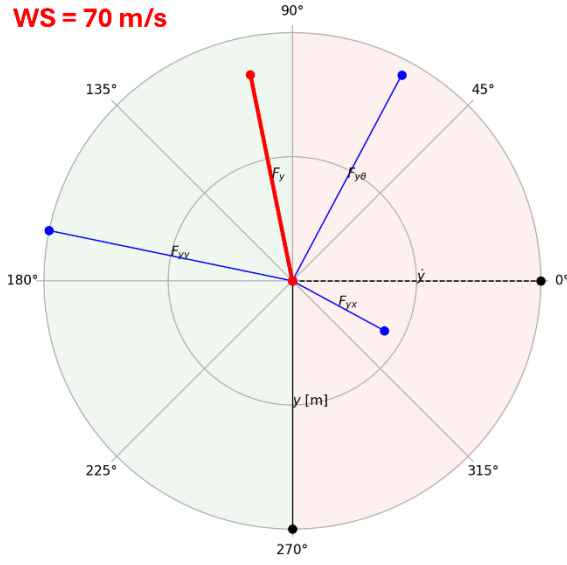
- *Figure 14:* - similarly, can the author comment on whether the "CFD" line (flap) quickly goes from almost 180° phase to -180° because angle unwrapping occurred or whether it actually travels through the zero-point, as what the plot would let one think.

Thank you for this comment. As the reviewer suggests, this is indeed caused by the force passing through zero. To visualize this more clearly, we have added a figure to this response showing the CFD result of Part 2, Fig. 4 in the complex plane. Each diagram corresponds to a different wind speed. All vectors are shown relative to the flapwise velocity and displacement, whose magnitudes are kept fixed. The flapwise velocity therefore always lies at 0° phase and the flapwise displacement at 270° . The total flapwise force is shown in red and its components in blue, with their magnitudes scaled relative to one another. If the total flapwise force lies in the right half-plane, i.e. predominantly in phase with the flapwise velocity, it performs positive work and is destabilizing. If it lies in the left half-plane, it is stabilizing. The diagrams clearly show the total force vector passing through zero. They also illustrate intuitively that the total force is the sum of its three components and that, especially at higher wind speeds, it is determined by the balance between the destabilizing flapwise force induced by torsional motion and the stabilizing flapwise force induced by flapwise motion.

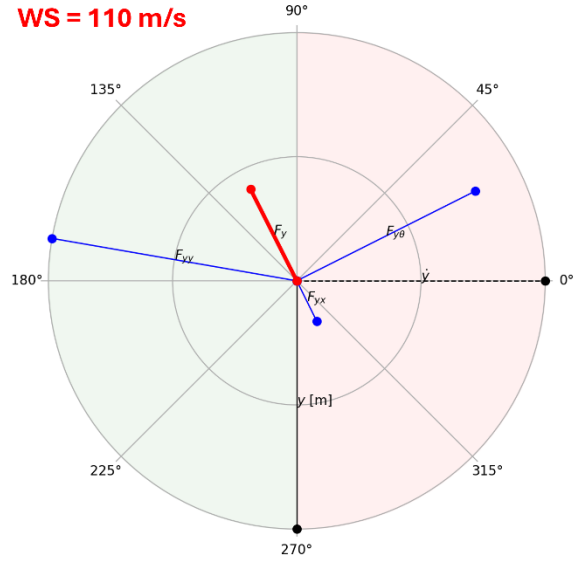
Although this kind of visualization is valuable, it makes it difficult to show the variation with relative wind speed without animating the figure. We therefore decided to keep the representation used in the paper, which shows all components more comprehensively and directly shows their dependence on the relative wind speed.

The revised 2DOF model shows a very similar behavior in almost all figures: a zero crossing of the flapwise displacement amplitude causes a 180° phase shift in most of the phase plots. We have added a remark on this to the text in Part 2, Sect. 4.

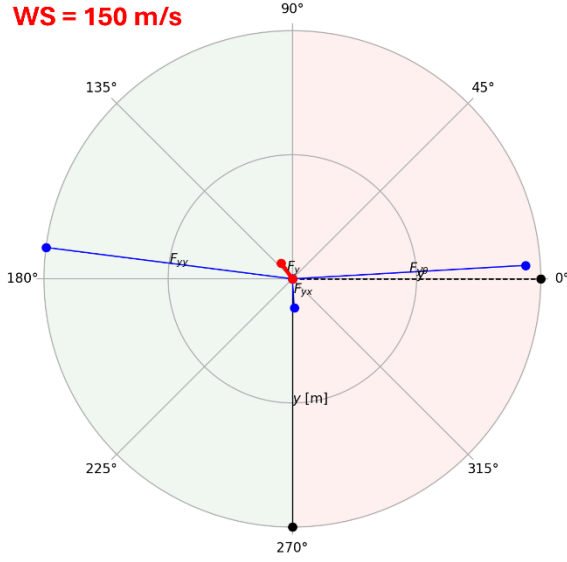
WS = 70 m/s



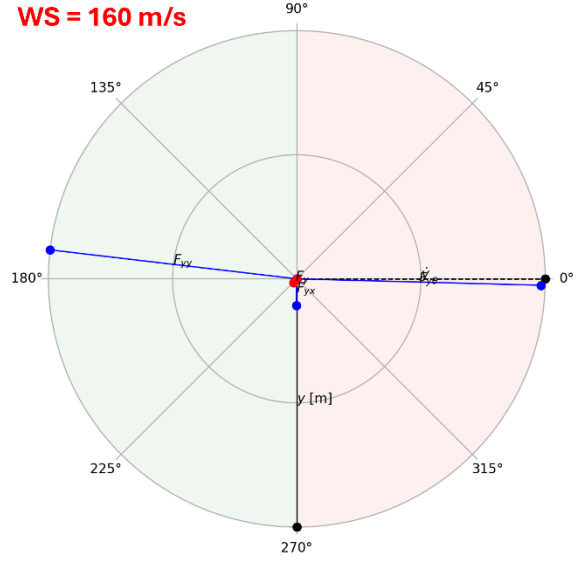
WS = 110 m/s



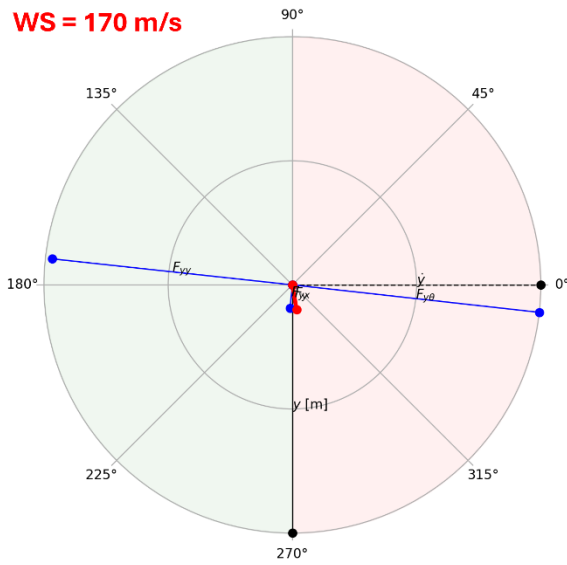
WS = 150 m/s



WS = 160 m/s



WS = 170 m/s



WS = 200 m/s

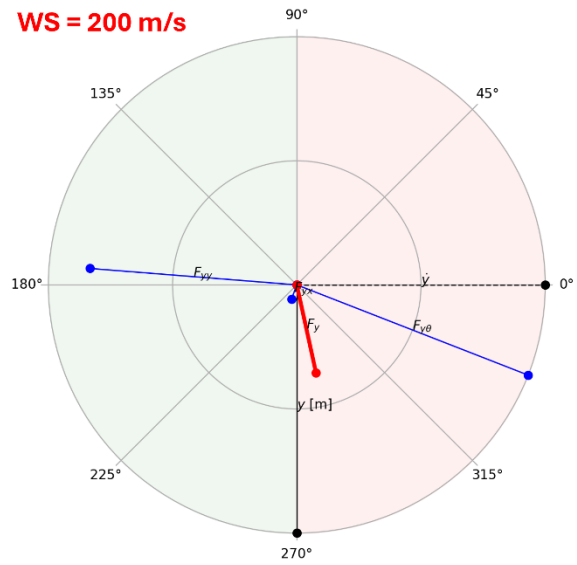


Figure 1: Complex plane visualization of the flapwise velocity and displacement, as well as the total flapwise force and its components. CFD result for $\gamma_x = -0.3$, $\gamma_y = 0$, $\alpha_0 = 7^\circ$

- *Figures 16 and 17:* - similar suspicious angle unwrapping are also visible in figs 16 and 17 (g and i); can the authors verify this and otherwise mention/justify their phase angle unwrapping strategy in the paper?

Thank you for this comment. This is indeed caused by a phenomenon similar to the one explained in the previous point, and is not an artifact of incorrect phase unwrapping.

- *line 646:* - when referring to the work for figs 14 and similar, shouldn't the formulation mention "integral of the product over one cycle...." instead "product of the modal force and velocity" ?
- can you also specify there whether you use the convention of work being done **on** the structure (and the as such positive work means flutter)?

Thank you for this comment. The aerodynamic work was indeed computed as the integral of the product of force and velocity over one cycle. As mentioned in point 13 of our general response to the reviewers, we now use the approximate aerodynamic damping instead of the aerodynamic work. A more detailed explanation of the derivation of both the aerodynamic work and the aerodynamic damping is given in Sects. 2.5.3-2.5.4 of Part 2.

- *Figures 14 and 15:* - can the authors comment w.r.t. why the "CFD" (edge) lines of Figs. 14 and 15 (g) show an initial jump in the phase, up to about 25 m/s, and also why this is not seen in the "Simplified 3DOF model" line?

Thank you for this question. To address it, we have added a figure here showing a zoomed-in view of the low wind-speed region for the case $\gamma_x = -0.3$, $\gamma_y = 0$, $AoA = 7^\circ$ (Fig. 4 of Part 2).

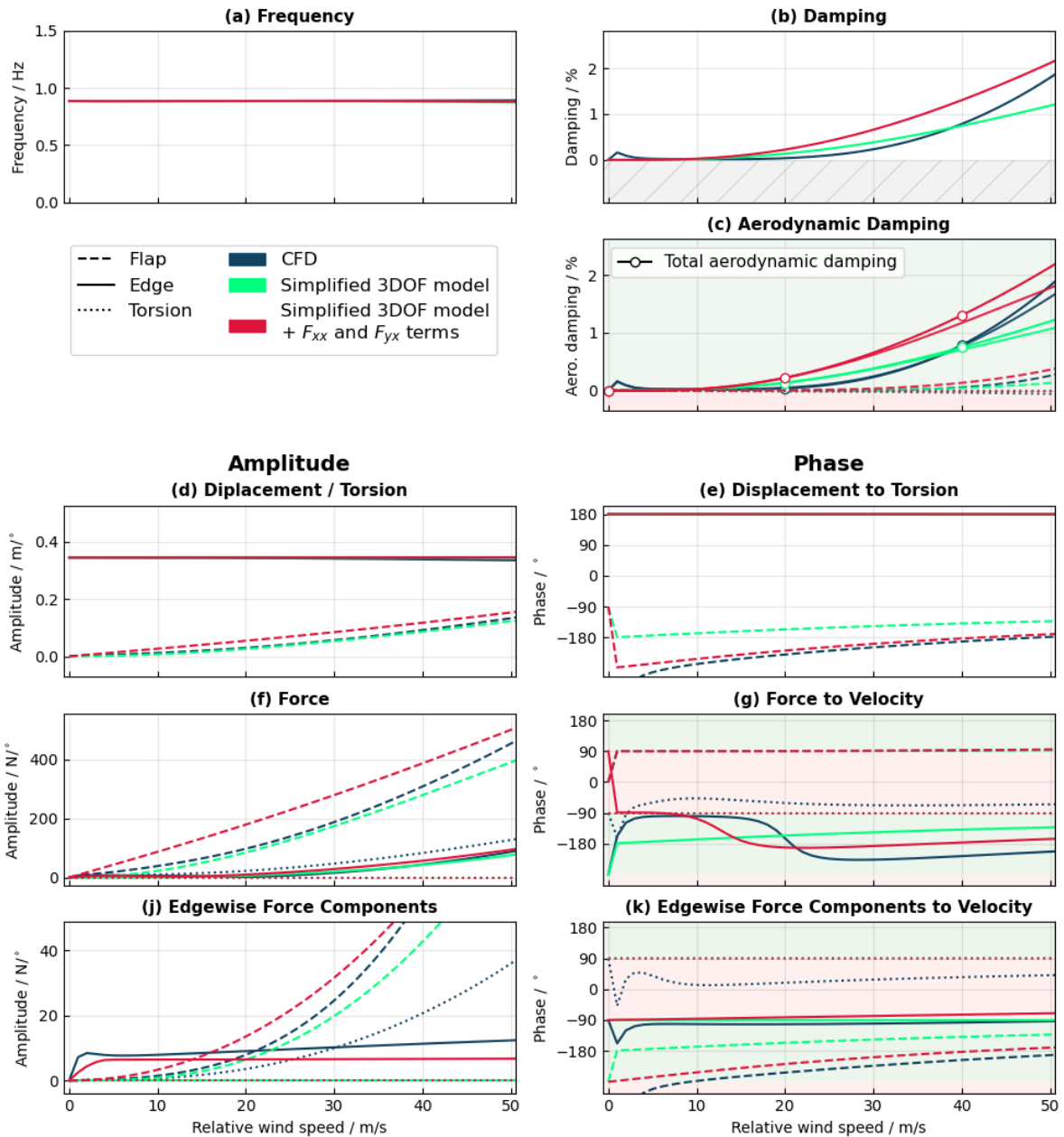
The two bottom axes show the edgewise force components (and not the flapwise force components, as shown in the paper). The blue CFD result and the green simplified 3DOF model are the same as those presented in the paper. The red result corresponds to the simplified 3DOF model, but additionally includes the F_{xx} and F_{yx} components, i.e., the forces in the edgewise and flapwise directions induced by edgewise motion. This figure illustrates quite clearly what causes the distinct trend in the phase of the edgewise force shown in axis (g).

Focusing first on the blue CFD result, the amplitudes of the edgewise force components in axis (j) show that the force component induced by edgewise motion (solid line) dominates at low wind speeds, up to approximately 20 m/s. Towards higher wind speeds, the exponentially growing flapwise-induced component (dashed line) strongly surpasses it. This is precisely what causes the observed trend in the total phase. At low wind speeds, the total phase (solid blue line in (g)) is approximately equal to the phase of the force component induced by edgewise motion (solid line in (k)), which is approximately -90° . Above 20 m/s, the total edgewise force becomes dominated by the flapwise-induced edgewise force. Consequently, the total phase follows the phase of this component (dashed line in (k)), which is approximately -270° at 20 m/s and increases towards approximately -180° at 50 m/s.

Turning now to the basic simplified results shown in green and the simplified results including the F_{xx} and F_{yx} force components shown in red, it becomes clear that the basic simplified model does not reproduce this trend because it is missing the force components responsible for it. In contrast, the simplified model including the additional

force components exhibits a qualitatively very similar trend. The same behavior is observed for the case $Y_x = +0.3$, $Y_y = 0$, $AOA = 7^\circ$.

In conclusion, we would like to briefly reiterate that the main objective of this investigation is to identify the aerodynamic components that are most relevant to the overall stability trends of the edgewise mode. The drastic simplification of the aerodynamic model will inevitably lead to differences such as this one, which can, in most cases likely be traced back to one or more of the aerodynamic components that have been omitted. However, given the scope of the investigation, we had to focus our explanation on the main and most relevant differences.



- line 678 (and Figs. 14-17): " The ground for the good agreement of the simplified 2 DOF model can be seen in the amplitude and phase of the edgewise component of the CFD reference result shown in figures 14(d) and 14(e). "

- the "2DOF edge" line mentioned in the text is not visible on those figures. Furthermore, the "Simplified 2DOF model" (edge) line is never visible on any of the Figs 14-17 (d-i), is it always in complete agreement with one of the other curves or was it discarded from the plots? This should be clarified.

Thank you for this comment, which should have been addressed more clearly in the original text. The original 2DOF model indeed showed only two lines in each diagram. However, as explained in point 12 of our general response to the reviewers, the 2DOF methodology has been improved. As a consequence, the motion of all three physical degrees of freedom can now be computed from the reduced degrees of freedom. Each diagram therefore shows all three lines, except for the flapwise force induced by edgewise motion, which is zero, as explained in a previous point.

- *line 717:* the paragraph title "Edge-twist coupling to feather ($\gamma_x = -0.3$), flap-twist coupling to feather ($\gamma_y = -0.3$)" as a sign error since stall should mean positive

Indeed, thank you for this comment, the heading has been corrected in Part 2, Sect. 4.

- *line 823:* " Inserting this in A1" -> in Eq. (A1) ?

Thank you, this has been corrected in Part 1, Appendix C.

- *line: 769:* "Notably, the relevant coupling is not a direct flapwise-torsional coupling, but rather a coupling between the structurally coupled edge-torsional motion and flapwise motion."

- the term "coupled edge-torsional motion and flapwise motion" was not defined before, and as such this sentence could be made even clearer (is this perhaps one of the most important findings of this study?). Can you define more clearly what you mean here, I assume you mean that we have a "chain" of effects such that flapwise motion leads to torsional motion which then leads to edgewise motion, but I am not sure I interpreted the text correctly... Please help the reader.

Thank you for this comment. This was indeed an important outcome of our work, as it confirms the earlier findings of Stäblein et al. with a very simple model.

- **The revised 2DOF model generalizes these findings further. Please refer to the last paragraph of point 12 of our general response to the reviewers, which explains how this original conclusion has been improved and generalized based on the revised 2DOF model. The conclusion in Part 2, Sect. 6 has been revised accordingly.**
- *Conclusions:* The proposed model is a simplified model, limited to 2D flows and 3 DOFs, and which also assumes attached flow. This should be made explicitly clear once again in the Conclusions.

Thank you for this comment. We agree that this should have been emphasized more clearly in the conclusion. We have revised the conclusion, as well as the introduction and the abstract, to state the limitations of this work more clearly.

- And last, but not the least, about the comment about the identification of "mistakes" in the work of your peers.
 - *line 117:* "Finally, in the process of this work some mistakes were discovered in the original publication by Stäblein et al. (2017a). This work therefore intends to serve as corrigendum for the original paper."

- line 333: " but the comparison carried out in process of this work revealed a couple mistakes and modeling differences in Stäblein et al. (2017a). "

I appreciate that the authors openly share the code and highlight the differences they found in Stablein et al. (2017a). However, I think the wording used to describe these differences as "mistakes" could be problematic.

First, the authors themselves say that these issues do not invalidate the original results, so calling them mistakes might be too strong. It could be better to refer to them as adjustments, refinements, or even just differences in interpretation. After all, the corrections were only tested on a single blade section, so how can we be sure they are universally valid? Did the authors consult Stablein et al. to confirm their findings?

Also, using terms like "corrigendum" might make researchers hesitant to share their code in the future for fear of being publicly corrected. We want to encourage openness, not discourage it. Unless there's clear evidence of bad practice, it's better to frame these as improvements or clarifications rather than errors.

In short, the authors should be more careful with their language to avoid implying that the original work was flawed, especially when the differences might just be a matter of interpretation or use-case. Consider naming the model "Stablein et al. Modified" rather than "Stablein et al. Corrected"

We did indeed contact the authors of that paper to discuss our results and findings before submitting our preprint. A. Stäblein, who no longer works in this research field, responded very positively. He was pleased that we took an interest in his work and were able to identify and resolve some inconsistencies in it. We further discussed the most appropriate way forward with his co-authors, who are still active in the field. They also welcomed our interest in their work, our detailed analysis and the improvements we found. Together, we concluded that the best approach would be for us to present our findings, propose improvements to the model used in Stäblein et al. (2017a), and compare this improved model with both our CFD approach and our potential flow approach. As a follow-up, the original authors could then publish an official corrigendum to Stäblein et al. (2017a) that refers to this paper for the suggested improvements and for the results verified against two independently derived methodologies.

We fully share your concern that care must be taken not to discourage researchers from making their work reproducible. On the contrary, we see this as a positive example: because of the excellent work by Stäblein et al., and the effort they made to make their results easily reproducible, we as interested readers were able to build upon their work.

We have addressed your concerns in the paper as follows:

- **The term "corrigendum" has been removed. Instead, the introduction now states: *"Furthermore, in the course of this work, several possibilities for improving the methodology of Stäblein et al. (2017a) were identified. This work therefore also presents an improved version of this methodology."***
- **Many points in Sect. 2.3 of Part 1 have been reformulated to make clear that most of them should be seen as improvements rather than corrections. The first two points, however, are still referred to as 'Incorrect implementation' and 'Sign error'. In contrast to the other points, these are discrepancies between the methodology described in Stäblein et al. (2017a) and its code implementation. We have kept the**

direct wording for these two points so that users of the publicly available code can clearly identify which parts of the implementation were changed and that results obtained with the original code may be affected. The section header has been changed from “Summary and corrections methodology Stäblein et al. (2017a)” to “*Summary and improvements of the methodology by Stäblein et al. (2017a)*”.

- The model has been renamed from “Stäblein et al. (2017a) – corrected” to “Stäblein et al. (2017a) – improved” throughout the paper.