

Dear Prof. Leishman,

We are deeply grateful for your interest in our work and for your detailed and insightful review of the manuscript. Your comments prompted us to critically re-examine the derivation of the potential flow-based model, to thoroughly document and extend the verification procedures, and to define the scope and limitations of the presented work more clearly. We believe that these efforts have made the revised version considerably clearer and more rigorous, and we sincerely hope that it now meets your expectations.

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 to the manuscript – including its division into two companion papers – several of which are referred to in the answers below. 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.

Sincerely,

Hendrik Verdonck and Oliver Hach

Review

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

- The manuscript addresses a relevant aeroelastic problem, but it is not publication-ready. The main issue is that the paper makes claims broader than the evidence supports, and unresolved issues in the aerodynamic formulation need to be checked before the stability results can be relied upon.

The study is based on a two-dimensional, three-degree-of-freedom typical section, one representative blade section, one airfoil, two angles of attack, and attached-flow conditions only. This is too limited to support broad conclusions about the reliability of classic reduced-order aerodynamic models for edgewise instabilities in wind turbine blades. The reduced-order model may be useful for mechanism studies, but the manuscript does not show that the conclusions transfer to a full rotating blade or turbine. Real edgewise instabilities are affected by radial structural variations, rotation, centrifugal stiffening, gravity, controller effects, blade twist, prebend, sweep, three-dimensional aerodynamics, and operating-point dependence. These effects appear mostly outside the scope of the present model. Therefore, the authors should narrow the title, abstract, and conclusions accordingly.

Thank you for this valid comment. We agree that the presented work is subject to several limitations and should be understood within them. We attempted to state these limitations clearly in the initial manuscript, but rereading it, we agree that we left too much room for misinterpretation.

We have made the following concrete modifications:

- **For Part 1, all claims of ‘validation’ or ‘verification’ of the modeling based on potential flow theory based have been replaced with ‘*comparison between CFD and potential flow theory results*’.**

- As before, both titles contain the key terms “blade section” and “attached flow”. The new title suffix of Part 1, “Comparison of CFD and potential flow theory results”, states the comparison without implying any broader claims.
- Both introductions and conclusions now explicitly list all limitations mentioned above: “The study is based on a two-dimensional, three-degree-of-freedom typical section, one representative blade section, one airfoil, two angles of attack, and attached-flow conditions only.”
- Both conclusions additionally state that transferring these results to a full blade or full turbine requires further analysis, due to the additional effects involved, such as radial structural variations, rotation, centrifugal stiffening, gravity, controller effects, blade twist, prebend, sweep, three-dimensional aerodynamics, and operating-point dependence.
- Both abstracts state the most important limitations, namely that the study is based on a two-dimensional blade section model in attached flow conditions only. They conclude by stating that the findings are limited to this scope and that transferring them to full turbines requires further research.
- A major concern with this paper is the derivation of the unsteady aerodynamic model. The issues affect the generalized aerodynamic matrix used in the flutter calculation. In particular, the force projections in Eqs. (8)–(16), the Theodorsen and Greenberg terms in Eqs. (21)–(24), and the quarter-chord angle expression in Eq. (25) requires a careful rederivation. The manuscript does not sufficiently distinguish among circulatory lift, non-circulatory lift, Greenberg edgewise-velocity terms, and the rotation of the steady load vector. The angle used to rotate the steady lift vector should be a geometric or kinematic quantity, whereas the Theodorsen function belongs in the circulatory part of the lift response. The present formulation appears to mix these concepts, especially through the use of the Theodorsen function in the expression for the unsteady quarter-chord angle of attack. This raises the possibility of double-counting or misplacing the unsteady aerodynamic lag.

Thank you for this comment. Following your concern, we have carried out a full re-derivation; it is given in Part 1, Sect. 2.2.2.

The core idea of the derivation remains the same. We start from the general expressions for the forces in x , y , and θ direction as functions of the aerodynamic forces, linearize them, and then derive the correct expressions for each of the linearized components.

The updated derivation is considerably easier to follow and interpret, as each component is now treated in a dedicated subsection, which also clearly separates the different aerodynamic effects.

Regarding the angle used for the rotation of the circulatory lift, we follow the work of Li et al. (2022, 2026), who derived this angle based on the work of Theodorsen and Garrick.

Your encouragement to carry out a re-derivation proved valuable, as we did indeed find one shortcoming in our derivation: the Greenberg terms, i.e. the influence of edgewise motion, were not correctly taken into account in the angle used for the rotation of the circulatory lift. This has now been corrected, which improved the agreement of the F_{xx} transfer function, the edgewise force induced by edgewise motion.

- The manuscript repeatedly refers to the Theodorsen/Greenberg-based sectional model as a “low-fidelity” aerodynamic model. That terminology is perhaps acceptable in comparison with CFD, but it does not reduce the burden of mathematical correctness. A low-fidelity

model can still be rigorous, internally consistent, and useful; conversely, if the force projections, angle definitions, non-circulatory terms, Greenberg terms, or sign conventions are incorrect, the model is not merely "low-fidelity" but incorrectly formulated. The authors should avoid treating agreement with selected CFD damping results as sufficient validation unless the underlying aerodynamic equations are first rederived and checked, and compared with standard check cases.

Thank you for this comment. We agree with your assessment.

To address your concerns, we have carried out the thorough re-derivation described in the previous point. In addition, we now present our verification results. First, the Theodorsen function implementation was verified by comparing the flat-plate heave and pitch response with the experimental data of Halfman (1951) and Rainey (1957), and with the MATLAB implementation of the Theodorsen function provided by Leishman (2022). Second, the classical flutter speeds of a flat-plate flap-twist model were compared with the literature reference data of Zeiler (2000). Both verifications are included in this response as optional appendices, so that the reviewers can see the verification results in full. In the final paper, however, we suggest only stating that these verifications were carried out successfully, in order to limit the length of the paper and focus on the results that provide direct insight. Both optional appendices can be found after Part 1 in Opt. Appendix B and Opt. Appendix C.

To avoid misinterpretation, we no longer use the terms "low-fidelity" and "high-fidelity". Instead, the results are now presented as a comparison between CFD-based and potential flow theory based models.

- The non-circulatory terms also require a more rigorous derivation. The manuscript switches between inflow-fixed and chord-fixed coordinates, while the apparent-mass forces depend directly on the imposed acceleration direction and the direction in which the pressure force acts. The transformation between the inflow-fixed coordinates and the chord-fixed edgewise/flapwise coordinates should be written explicitly and carried through the derivation. The added chordwise apparent-mass term, proportional to the square of the airfoil thickness, is also not a classic unsteady aerodynamic or Theodorsen result. It may be a plausible engineering correction for a thick airfoil, but it should not be introduced as though it follows directly from the classical formulation. The authors need to provide a derivation or citation, the theoretical basis for the term, its sign convention, and an assessment of the sensitivity of the stability results to its inclusion. If the term is empirical or heuristic, that should be stated clearly.

Thank you for these comments.

In our new derivation (Sect. 2.2.2 of Part 1) we did include the transformation between the inflow-fixed and the chord-fixed coordinates more explicitly. We still give the expressions for the aerodynamic components (e.g. circulatory lift) as a function of the inflow-fixed coordinates, but then immediately include the transformation. The final expressions are therefore easy to understand and as a function of the chord-fixed coordinates, instead of the transformation all the way at the end in the initial version.

We agree that the chordwise apparent-mass term is not part of the classical Theodorsen formulation and should not have been introduced as such. We have rewritten the paragraph to state explicitly that it is a heuristic correction. Its theoretical basis is the classical potential flow result for the added mass of an elliptical cylinder, which for a flat

plate reduces exactly to Theodorsen's normal apparent mass term. The airfoil is approximated by an ellipse of equal chord and thickness. The revised text also includes the corresponding references, the derivation of the normalization and the sign convention.

Regarding the sensitivity, we performed an additional stability analysis in which only this term was omitted. For the six detailed cases considered in the paper, the maximum influence on the edgewise flutter speed is 0.3%. This is not surprising. The term is purely real, i.e. in phase with the edgewise displacement, so it performs no work over an oscillation cycle and does not affect the damping directly. It acts as a small additional mass in edgewise direction, which slightly lowers the edgewise frequency. Its influence on the damping is therefore only indirect, through small changes in the modal frequency, and with it the reduced frequency, and in the mode shape. This is now also mentioned in the manuscript.

- There also appears to be a possible sign inconsistency in the perturbation terms associated with the relative velocity magnitude. The camber-moment perturbation and the drag perturbation should follow from the same expansion of the relative velocity squared. If the normal-velocity perturbation enters the camber-moment term with one sign but the corresponding drag perturbation with the opposite sign, the manuscript needs to explain why. Otherwise, this suggests a possible sign error. Because the stability result depends on small aerodynamic work terms and phase relationships, such sign issues cannot be treated as minor algebraic or notation details.

Thank you for catching this mistake. The error was confined to the equation as written in the paper; the implementation used to compute the results was correct. The equation has been corrected in the revised manuscript (Eq. 55 in Part 1).

- There is also a possible inconsistency in the coordinates of the Greenberg (unsteady free-stream) terms. The derivation states that the Theodorsen and Greenberg expressions are formulated in inflow-fixed coordinates, using motion components parallel and perpendicular to the relative wind. However, at least one expression appears to use an edgewise displacement amplitude without clearly indicating whether it is the inflow-fixed or chord-fixed component. If this is a typographical issue, it should be corrected. If not, then the derivation may be mixing coordinate systems.

Thank you for this correction. This was a typographical error in the paper only, and it has been corrected in the revised manuscript (Eq. 24 in Part 1).

- The manuscript itself indicates that not all aerodynamic transfer functions agree well between the low-fidelity and CFD models. In particular, some edgewise-motion-induced components have poor agreement or even opposite signs in parts of the reduced-frequency range. The authors argue that these discrepancies have a limited effect on the final damping result for the selected case, but that does not establish that the low-fidelity aerodynamic formulation is generally reliable. These discrepancies are especially important because the paper's stated purpose is to build confidence in low-fidelity aerodynamic models. The authors should identify which transfer-function discrepancies matter, which do not, and why. They should also avoid broad validation language unless the unexplained discrepancies are resolved.

Thank you for this comment. As mentioned above, the re-derivation revealed one further improvement in our derivation of the potential flow model. In addition, a sign error was

found in the code used by Stäblein et al. (2017a). Both points are explained in more detail in points 4. and 5. of our general response to the reviewers. Together, these improvements led to a much better agreement with the CFD result in the imaginary part of the F_{xx} term for our derivation, and in the F_{xx} and F_{yx} terms as a whole for the literature reference model.

Nevertheless, some unexplained differences remain. A considerable effort went into understanding these 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. Note also that very little reference data, and very few reference implementations, are available for forces acting in edgewise direction or induced by edgewise motion. To the best of our knowledge, the potential flow models used in this work have been derived and implemented correctly.

To address your concern, we have revised the broad validation language throughout the paper. Instead of “validation” or “verification”, we now describe the work as a comparison between results from CFD and potential flow theory.

- The CFD comparison is also not sufficient to justify the validation language. The manuscript compares classic unsteady aerodynamics with a two-dimensional CFD calculation, but it does not provide enough verification of the CFD reference. More evidence is needed on grid convergence, time-step sensitivity, domain-size effects, boundary-condition effects, numerical dissipation, and extraction of the aerodynamic transfer functions. Without this, the CFD result cannot be treated as a definitive reference.

Thank you for this comment. We have added the results of our CFD verification studies as Opt. Appendix A to Part 1 of the revised manuscript, in the same way as the optional appendices described in the previous point. They show the influence of the most important parameters we identified, namely mesh discretization, time step, domain size, number of pseudo-time iterations and pulse amplitude, on the final aerodynamic quantities of interest, i.e. the transfer functions. As these plots take up many pages and do not provide direct insight into the results, we suggest not including them in the final paper. Instead, the paper now states which verification studies were carried out.

In addition, we have added a much more detailed description of the CFD setup to the main text, as well as a dedicated Appendix B listing all CFD settings.

- The blade-section kinematics and force definitions need to be corrected and redrawn. In particular, the angles in the blade-element/typical-section diagram appear to be defined incorrectly. For this problem, the angle of attack, inflow angle, chord line, lift direction, drag direction, and pitch/torsion convention must be geometrically consistent. Because the governing equations depend directly on these sign conventions and angle definitions, an incorrect angle construction relative to the chord undermines confidence in the subsequent force projections and physical interpretation. The authors should provide a corrected diagram showing the chord line, relative velocity vector, inflow angle, angle of attack, lift and drag directions, elastic axis, aerodynamic center, center of gravity, and positive directions of edgewise, flapwise, and torsional motion.

Thank you for this comment. The figure of the blade section has been completely redrawn and now includes all the suggested kinematics, reference points, angles and forces (Fig. 1 in Part 1).

- The implementation of the Theodorsen and Greenberg functions also needs to be documented. Given the noted ambiguity in the analytical formulation, it is difficult to know whether the numerical implementation uses the same sign conventions, coordinate definitions, reduced-frequency definition, and complex-valued transfer-function convention as the derivation. The authors should state explicitly how the Theodorsen function ($C(k)$) is evaluated, which branch/sign convention is used for the Hankel (or equivalent approximation if one is used), how the Greenberg terms are implemented for edgewise motion, and how the complex aerodynamic transfer functions are assembled into the matrix ($A(p, \alpha_0)$). They should also provide verification against a simple canonical case, such as a harmonic plunge and the pitch of a flat plate, before applying the formulation to the cambered thick airfoil section. Without such implementation checks, apparent agreement in selected modal damping results does not demonstrate that the Theodorsen/Greenberg model has been implemented correctly.

Thank you for this comment. We have restructured the derivation in Sect. 2.2 of Part 1 so that it is now directly traceable how the aerodynamic transfer functions enter the aerodynamic matrix $A(k, \alpha_0)$.

The suggested verification against a canonical case of a flat plate in harmonic plunge and pitch is now given in Opt. Appendix B, as mentioned above.

- The paper also needs a clearer explanation of the physical mechanisms. The role of the steady load vector is important, but the manuscript does not clearly separate the effects of mean lift, zero-lift angle, camber moment, force-vector rotation, non-circulatory force, and structural edge-twist coupling. The discussion shows that these terms matter, but it does not explain the causal pathway with sufficient clarity. The authors should show explicitly how the aerodynamic work contribution changes when each term is retained or removed.

Thank you for this comment. We agree that a clear understanding of the causal pathway is essential, and we would like to explain how this is addressed across both papers.

This work addresses two distinct research questions, which, following the requests of other reviewers, are now treated in two separate companion papers. The first research question, addressed in Part 1, is how the choice of aerodynamic model influences the stability prediction. To answer it, the stability results obtained with aerodynamics based on CFD and on potential flow theory are compared for a wide range of cases, covering varying structural edge-twist and flap-twist coupling as well as different steady angles of attack. In addition, a dedicated sensitivity study analyzes the effect of specific aerodynamic components on the stability prediction. We believe that, within the stated limitations, these results answer the first research question and form a self-contained and valuable contribution.

The second research question, addressed in Part 2, is to better understand the underlying mechanism of the edgewise instability. Our approach there is to decompose the approximate aerodynamic damping into the contributions of the individual degrees of freedom and force components. We complement this with a systematic reduction of the model to the minimal set of aerodynamic terms and degrees of freedom that still reproduces the instability, resulting in a quasi-steady model with only three

aerodynamic terms and a two-degree-of-freedom model. We believe this is the most effective way to uncover the causal pathway of the mechanism, since it isolates the essential interactions instead of analyzing the full model with all its terms at once.

The reviewer's suggestion goes one step further: investigating how each individual aerodynamic component affects not only the final stability result, but also the causal pathway itself. This would require a dedicated analysis, which we consider outside the scope of the present work. We agree, however, that it is a valuable idea for future research.

- The correction of the earlier Stäblein et al. methodology is potentially important, but it should be presented more systematically. The manuscript should state clearly which previous results remain valid, which results have changed, and which conclusions require reinterpretation. As written, the corrigendum aspect is mixed into the main analysis and is difficult to assess.

Thank you for this comment. We agree that a systematic assessment of the effect of the improvements on all results of Stäblein et al. (2017a) would be valuable. Unfortunately, this is not possible within the scope of this work, as it would require re-analyzing each result presented in the original paper. We therefore limit ourselves to explaining in which aspects the methodology can be improved, showing how these aspects are reflected in the code implementation, and including the improved methodology in our comparisons.

Consequently, this work should not be seen as a direct corrigendum, and we have reformulated the manuscript accordingly. Instead, we state that we use the methodology of Stäblein et al. (2017a), propose improvements and corrections, and use the improved model as an additional reference in our comparison.

- Overall, the paper is far too narrowly based for the strength of its conclusions, and the unsteady aerodynamic formulation contains issues that need a full and careful rederivation. The authors should substantially revise the manuscript by limiting the claims, avoiding broad validation language, strengthening the CFD verification, correcting the blade-element/typical-section diagrams and angle definitions, rederiving the circulatory, non-circulatory, Greenberg, and steady-load-vector terms, and clarifying the physical mechanisms. The paper is not ready for publication in its present form.

For these reasons, I would not characterize the required revisions as minor. The manuscript needs a careful rederivation of the unsteady aerodynamic terms, clearer correction of the blade-section geometry and angle definitions, better documentation of the Theodorsen/Greenberg implementation, stronger CFD verification, and more limited claims about validation.

We are grateful for your careful reading of our manuscript and for the thorough and critical review. The comments led directly to concrete improvements in the aerodynamic modeling, including the re-derivation of the potential flow model and the identification of a further error in the literature reference implementation, and to an overall higher quality of the paper.

We have made substantial revisions addressing all of the points raised, as detailed in our responses above, and hope that the revised manuscript now meets the expectations.