Point-to-point reply to reviewers

Paper wes-2025-120

Title:

Large Eddy Simulation of the IEA 15-MW Wind Turbine Using a Two-Way Coupled Fluid-Structure Interaction Model

September 26, 2025

The authors wish to thank the reviewers for their comments and suggestions on the paper to enhance the manuscript. The paper has been revised accordingly. The revised parts have been highlighted in red in the updated manuscript. If mentioned, the page number refers to the revised paper. In the following we report point-to-point replies concerning the questions and comments raised by the reviewers.

Reviewer #1

Main Critique Points

Documenting the software and its validation status.

- *As a reader, I found it a bit hard to trace how the different pieces of your in-house UTD-WF framework were developed across prior work, and what has been validated under which conditions. This is important to know that the in-house code you use - i.e., the foundation for all your work and insights - is credible and correct in the first place. Adding a short provenance/validation paragraph would strengthen the manuscript.*

  A paragraph about the provenance of the in-house code and the validation of its different version has been now added at the beginning of Section 2.1.1 (red paragraph on page 4).

- *Clarifying which subsets constitute formal validation would help readers map prior evidence to your present setup. In that light, two clarifications would make your contribution easier to interpret:*

  1. *Scope of prior validation vs. present cases: Prior validations include uniform and ABL inflows for a 5 MW turbine; your study addresses the IEA-15 MW case for a sheared laminar inflow configuration. Since larger rotors can amplify aeroelastic effects, it would be helpful to note explicitly that the present application extends the validated setting and to discuss any implications or arising uncertainties.*

     We have now included a small paragraph discussing this important point in Section 2.1.1 (last lines of the red paragraph on page 4). In particular, the added lines are as follows: "Notice that prior validations by (Della Posta et al. 2022) of the CFD-CSD solver were made on a laminar uniform and a turbulent sheared inflows for a 5 MW NREL turbine, whereas our study extends the validated setting to the IEA-15 MW case for a sheared laminar inflow configuration. However, as discussed in the framework of the IEA Wind TCP Task 47 (Cacciola et al., 2025), turbulent fluctuations appear to have a much stronger impact than shear on load response of aero-elastic numerical codes. Moreover, high-fidelity codes appear rather consistent in predicting loads, while engineering models tend to overpredict fatigue loads, particularly for large rotors".

2. *Citations for solver provenance: Where you cite the solver's origin, consider also referencing Santoni et al. (2015) as the first UTD-WF publication, prior to Santoni et al. (2020). Further mentioning the following papers and validations that were added to Santoni et al. (2015) would help readers that are unfamiliar with the code's history. Just so they know that the inhouse code you use is validated within a certain range.*

   We have now used Santoni et al. (2015) as first reference to the in-house code.

Comparing BEM with CFD for aeroelastic simulations

- *The conclusions drawn about the relative suitability of CFD-based versus BEM-based aeroelastic simulations are not entirely straightforward, because the setups being compared differ in more than one respect. In particular:*
  - *Case A (CFD-CSD) couples a finite-volume LES solver with ALM to a linear structural model (A.1: OV; A.2: TN).*
  - *Case B (OpenFAST with BeamDyn) uses BEM aerodynamics with a nonlinear structural solver.*
  - *Case C (OpenFAST with AeroDyn/ElastoDyn) combines BEM aerodynamics with a linear structural solver.*

  Because both the aerodynamic and structural solvers vary simultaneously, it is difficult to isolate whether observed differences stem from the flow solver or from the aeroelastic solver (or from their specific implementations). Any causal conclusions should therefore be presented with appropriate caution.

  At the beginning of section 4.2, we added as a preliminary comment the following sentence: "It is important to note that the four solvers employed differ in both their aerodynamic and structural modeling approaches. As a result, it is not always possible to unambiguously determine whether the observed discrepancies in the results originate from the fluid-dynamic models or from the structural formulations."

  Moreover, We have rephrased all along the paper and in the Conclusions section, several sentences providing causal conclusions drawn by the comparison between these codes.

- *A related point concerns the comparison with Bernardi et al. (2023). That study focused on a different turbine (NREL 5 MW), under uniform inflow, and reports blade load distributions that do not match those of the present paper. Drawing direct inferences from those results to the IEA 15 MW case with sheared inflow is thus challenging, as multiple differences may drive the discrepancies. In addition, the current manuscript discusses the 5 MW results extensively without showing them, requiring the reader to switch between papers to follow the discussion. To strengthen this part, I would suggest either limiting the cross-paper comparison, or alternatively presenting a direct comparison in the present manuscript — for instance, by reproducing a nondimensionalized plot from Santoni et al. (2017) in a comparable format that allows readers to interpret the differences more clearly.*

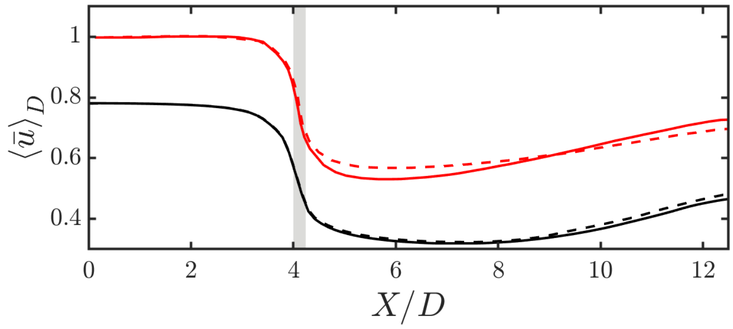

Figure 1: Rotor-averaged velocity along the streamwise direction normalized by the undisturbed velocity at the rotor height, namely, $U_\infty = 10\ m/s$, for the present data (black curves) and the work of [1] (red curves). The grey region represents the area covered by the rotor. (RO - - - -, TN ——).

As suggested by the reviewer, we have included in figure 4 of the revised paper, reported here for completeness, a reproduction of the nondimensionalized plot from Santoni et al. (2017) in a comparable format that allows the reader to interpret the differences more clearly. Moreover, we have eliminated some comparison with the 5MW turbine results in the conclusions.

Convergence study

- *The grid convergence section currently relies on only two simulations. While this gives a first impression, two points are generally not sufficient to establish a clear convergence trend. I would therefore suggest either adding at least one additional grid resolution to demonstrate a systematic trend, or explicitly referring to convergence studies from previous publications with a closely similar setup.*

  Following the suggestion of this referee, we have added a computation with a coarser grid, consisting of 1.3×10^8 grid points. The outcome of the simulation in terms of angle of attack, forces and momentum is provided in Figure A.2 (page 25). All quantities are found to compare rather well, as discussed in the manuscript.

- *In addition, the statement "the results obtained using the coarse and fine grids are extremely close ... the curves of the angle of attack are almost indistinguishable" comes across as rather qualitative. It would strengthen the section to include a more quantitative measure of convergence (e.g. RMS difference, maximum deviation along the span, or an error norm), so readers can assess the actual magnitude of the differences. Further, the figure of the angle-of-attack comparison in the grid comparison study would be much more clear if it would be scaled to include a maximum angle of 15 degrees. The angles of attack on the cylindrical root section are not of interest to the reader.*

  We have included in the discussion a quantitative measure of convergence, in particular the absolute and relative errors on the angle of attack at 80% of the span. Further, following the suggestion of the referee, the figure of the angle-of-attack comparison in the grid comparison study has been scaled to include a maximum angle of 15 degrees.

- *Tip-loss models: I also noticed that different formulations of the tip-loss model are used across the compared solvers. Specifically: OpenFAST applies the classical Prandtl tip and root loss correction (line 222). The CFD-CSD solver employs the [2] correction (line 144). Since the Shen formulation is not equivalent to the standard Prandtl approach, the choice of model may* *itself contribute to differences in the results. For the sake of a fair comparison, you could report clearly which correction is applied in detail in each case and provide the coefficients used in the Shen model. This would make the comparison more transparent to the reader.*

Indeed OpenFast and the CFD-CSD code use two different tip-loss models, but we have used a cross-code calibration to set the coefficents of our model to match the forces close to the tip reported by OpenFAST, which uses the Prandtl model. Concerning the tip-loss model used for the CFD-CSD code, we have used the Shen model, whose coefficients c_1 and c_2 have been set in the following way. In particular, c_1 has been set to the value reported in the Shen 2005 paper ($c_1 = 0.125$), which is based on a comparison with experimental distributions of normal forces near the tip for the NREL rotor at a wind speed of $10\,m/s$, and a tip speed ratio of 3.79. Whereas, the second coefficient has been increased with respect to the value reported by Shen (2005), after a calibration of the coefficient to match the forces close to the tip reported by OpenFAST for the same turbine and flow case, leading to the choice of $c_2 = 32$. A comment has been added to the manuscript reporting these details.

Detailed Critique Points

2 Methodologies

- *2.1 CFD-CSD Solver*

  - *L. 121: The subsection caption currently reads "2.1 CFD-CSD solver". From what I know, subsection titles should follow a consistent style. Please either use title case throughout (e.g., "2.1 CFD-CSD Solver") or sentence case consistently. At present, there is a mix (for example, "4 Results and Discussion" in title case versus "3 Flow and structural setup" in sentence case). Perhaps also check the WES journal guidelines, whether to use title or sentence case.*

    Looking at the journal guidelines, it is stated that "Titles and headings follow sentence-style capitalization i.e. first word and proper nouns only". Thus, we now used the sentence style, not capitalizing the first letter of nouns. This has been now done throughout the paper, in a consistent way.

  - *L. 123–125: The text states: "The simulations of the flow around the wind turbine are carried out through Large-Eddy Simulations (LESs) of the incompressible, filtered, 3D Navier-Stokes equations, employing our in-house UTD-WF solver (Santoni et al., 2020)." To my understanding, the first introduction of the UTD-WF code was actually in Santoni et al. (2015), "Development of a high fidelity CFD code for wind farm control". The 2020 paper (Santoni et al., 2020) applies the solver at wind farm scale, but the code originates from the earlier work. Since UTD-WF is not as widely established as solvers like OpenFOAM, it would strengthen the methodology section if you also reference publications where the solver has been validated against either an established CFD solver of similar fidelity or experimental data.*

    We have now included more extensive comments concerning the validation of the code, both at the beginning of Section 2.1.1 and in the two validation Appendices (A.1 and A.2).

  - *L. 138: The manuscript currently cites Troldborg (2009) for the Actuator Line Method (ALM). A more appropriate primary reference would be Sørensen and Shen (2002), who first introduced the ALM.*

Thank you for the suggestion, now we have included Sørensen and Shen (2002) as primary reference.

– *L. 144: You mention comparing the Shen tip-loss correction with the Prandtl tip-loss model. Could you please specify the chosen free parameters in the Shen model? It would also help to clarify how you ensured that both tip-loss approaches are consistent, so that observed differences in results are not simply due to discrepancies between the models themselves.*

  As reported above, the coefficient c_1 of the Shen model has been set to the value reported in the Shen 2005 paper ($c_1 = 0.125$), which is based on a comparison with experimental distributions of normal forces near the tip for the NREL rotor at a wind speed of $10m/s$, and a tip speed ratio of 3.79. Whereas, the second coefficient has been increased with respect to the value reported by Shen (2005), after a calibration of the coefficient to match the forces close to the tip reported by OpenFAST for the same turbine and flow case, leading to the choice of $c_2 = 32$.

– *L. 159–160: The structural model is attributed to Della Posta et al. (2022, 2023). To provide readers with a clearer overview of how the full solver framework was assembled, it would be useful to briefly note the sequence of developments: Santoni et al. (2015): introduction of the ALM–LES flow solver (UTD-WF), Santoni et al. (2017): tower–nacelle representation using the immersed boundary method, Della Posta et al. (2022): coupling of the aeroelastic solver, Della Posta et al. (2023): implementation of the unsteady aerodynamics model (Leishman–Beddoes dynamic stall). You might phrase it along the lines of: "The structural model used in the present study builds upon the UTD-WF framework progressively developed in Santoni et al. (2015, 2017) and further extended by Della Posta et al. (2022, 2023), where the aeroelastic solver and the Leishman–Beddoes dynamic stall model were implemented."*

  This suggestion has been implemented at the beginning of section 2.1.1 of the new version of the paper.

3 Flow and Structural Setup

- *L. 235: The subsection caption currently reads "3 Flow and structural setup". For consistency with the other section titles, the first letter of nouns should be capitalized (e.g., "3 Flow and Structural Setup"). Please check the WES journal guidelines and apply the chosen style consistently throughout.*

  This has been now done throughout the paper, in a consistent way.

- *L. 248–249: The text states: "The turbine location is 4 diameter units from the inlet and centered in the spanwise direction." Could you please elaborate on whether this positioning is sufficient to ensure that there is no unintended influence between the turbine and neighboring boundaries? It would be helpful to explain how these dimensions were chosen—either by referencing a small domain-geometry convergence study (perhaps in the appendix) or by citing relevant literature that uses a similar domain setup.*

  In the revised paper, we have discussed the choice of the layout with reference to the data avalilable in the recent literature.

- *L. 250: The manuscript notes: "Furthermore, we impose (...) a radiative outlet boundary condition." This boundary condition type is relatively uncommon in CFD literature. A short explanation of its formulation would therefore be valuable for readers. For instance, Santoni et al. (2017) also use this condition and explicitly provide the form of the equation:* $\frac{\partial \tilde{U}_i}{\partial t} + C \frac{\partial \tilde{U}_i}{\partial n} = 0$. *To my knowledge, this BC is commonly referred to as "convective boundary condition" If you wish to use the term "radiative boundary condition," it would be helpful to clarify this naming choice and cite Santoni et al. (2017) for context.*

  We have now explicitely reported the form of this boundary condition, and changed the name with "convective boundary condition".

4 Results and Discussion

- *All captions of the subsections, similar to the caption of chapter three, should have the first letter of the nouns in capital letters. So instead of "4.1 Flow analysis" − > "4.1 Flow Analysis" and the same for 4.2, 4.3, and 4.4*

  Done (see previous questions).

- *4.1 Flow Analysis*

  - *L. 280–283: The manuscript states: "Contrary to what Santoni et al. (2017) observed in their work on the 5MW reference turbine invested by a uniform inflow, the rotor-averaged velocity for the TN configuration in the wake remains slightly lower than for the OR case, indicating that wake recovery is slightly hindered by the presence of the tower." It is useful to compare with Santoni et al. (2017), but in order for readers to interpret the comparison correctly, it would be important to also highlight the notable differences between the setups (e.g., Santoni et al. (2017) used no-slip boundary conditions at all walls — top, bottom, and lateral — and neglected aeroelastic effects). Furthermore, I recommend caution in how the observed results are phrased. Since this finding is based on a single CFD-based aeroelastic solver, it would be safer to state that wake recovery appears to be hindered by tower presence, but that further validation is required as the result does not fully align with expected aerodynamics. In the following sentence, the text currently shifts from observation to causality: "The reason for this behavior can be found in the different aspect ratio of the tower for the present turbine." Here, I would suggest a more cautious phrasing such as: "One possible explanation for this unexpected trend could be differences in the tower-to-rotor aspect ratio."*

    We agree with the referee. We have now restated the comparison following the suggestions of the referee.

  - *L. 291: The sentence "Figure 5 represents the time-averaged TKE for both configurations on different planes." refers to a figure that is placed two pages later. This is not a critical issue, but if the layout allows, placing the figure closer to its first mention would improve readability.*

    Done.

  - *L. 296–297: The manuscript states: "This suggests that the tower does not increase the kinetic energy entrainment but it rather has a slight shielding effect on wake recovery." Similar to my earlier comment, I would recommend avoiding the phrasing "this suggests"* *here, as it conveys a level of certainty not yet supported by validation against other solvers or experimental data. A more neutral formulation (e.g., "This result may indicate ..." or "In this simulation, the tower effect appears to ...") would be more appropriate.*

Done.

- *4.2 Aerodynamic loads on the blade*

  - *Figure 7: It may improve clarity if the angle of attack (AoA) is shown in a zoomed-in range (e.g., [0°, 15°]), as otherwise it is difficult to interpret the details. The legend also appears to be missing for this plot, which makes comparison harder. In addition, the BeamDyn AoA and forces in the most outboard two blade sections seem to approach zero, which looks unexpected. It would be helpful to add a short explanation if there is a known reason for this, or otherwise acknowledge it as an open point.*

    Concerning the AoA, this is due to the fact that Prantl tip-loss correction used by OpenFAST is applied to the velocity, and not to the forces, leading to a zero angle of attack at the tip. Concerning the forces, both tip-loss correction models are constructed in order to lead to zero forces at the tip.

  - *L. 339–341: The sentence currently reads: "Indeed, the blade-tower interaction leads to an oscillations of the aerodynamic forces and of the incidence angle around $\theta = 180$, i.e., when the blade is pointing down." There is a grammar error here: it should read "leads to oscillations"*

    Done.

  - *Figure 8b: There appears to be a notable disagreement in F2 between CFD-CSD/OV and ElastoDyn, even though the two models agreed reasonably well on AoA. Moreover, given that CFD-CSD/OV predicts a lower AoA compared to ElastoDyn, one would expect a lower F2 compared to ElastoDyn, yet the opposite is observed. Could you please clarify this?*

    Here, F_2 is the flapwise force, the one perpendicular to the plane of rotation of the blades. Since F_2 depends on lift times the cosine of the flow angle, it should decrease when the AoA increases with a constant pitch and twist angle. Thus, we think that the trend observed in figure 8b is consistent with that of figure 8a. Concerning the error, since the relation between the AoA and the force is not linear, but it depends on the cosine of the flow angle, we think that the errors on these two variables are not supposed to scale linearly. In particular, on the AoA we observe an error of $\approx 4.5\%$, while on F_2 the error is $\approx 12\%$. Since the AoA is rather low in the considered plots, its cosine will be rather high, justifying the increased weight of the error on the flapwise force.

  - *L. 353–355: The manuscript states: "On the other hand, when torsional feedback is included, CFD-CSD/T and BeamDyn solvers agree rather well for all the quantities considered, regardless of the linearity or non-linearity of the models." I would suggest reconsidering this phrasing. In Figure 8a there are visible discrepancies across all azimuthal angles, and in Figure 8c there are large differences at both low and high azimuthal angles. Instead of stating "agree rather well for all quantities," it might be more precise to describe where the agreement is reasonable and where significant differences remain.*

We have modified the paragraph, discussing in detail these differences (see lines 393-395).

- *Figure 9: To improve readability, it would be helpful to use a consistent color scheme across the colormaps, so that interpretation of the results is more straightforward.*

  The colormaps in figure 9 have been unified.

- *L. 374–377: The manuscript notes: "Although some mild differences can be observed in their amplitudes and phases, the frequency of these oscillations appears consistent between the two solvers and comparable with the natural frequency of the first torsional mode." Here I would recommend two adjustments:*

  1. *The differences in amplitude, especially for α and F3, are not "mild" but rather notable. In some cases (e.g., BeamDyn-TN), the qualitative shape also differs, showing more oscillations than CFD-CSD/T.*
  2. *Instead of subjective wording like "mild" or "strong," it would be clearer to describe the differences in quantitative or qualitative terms (e.g., "larger amplitude," "different oscillation patterns").*

     We have now described the differences in a quantitative way, reporting the approximate errors for all the curves (lines 416-418).

- *4.3 Power and Thrust coefficients*

  - *L. 388–390: The manuscript states: "The results reflect the dependency of the power and thrust coefficients on the tangential aerodynamic force F2 and the normal aerodynamic force F3 at the 80 % of the blade, respectively." For clarity and consistency, please use a single naming convention throughout. At present, F2/F3 are sometimes referred to as tangential/normal, and in other places as edgewise/flapwise. Consistency will make it easier for readers to follow.*

    We have adopted the naming flapwise/edgewise throughout the revised paper.

  - *L. 391–392: The text notes: "Notice that, also here, we can observe that the drop in the Cp curve appears to be rather consistently predicted by BEM and CFD." I would recommend revising this statement. The qualitative shape of the drop differs between the two methods: the BEM prediction exhibits notable oscillations before and after the drop, whereas these are not present in the CFD results. Thus, describing the predictions as "consistently" aligned may be misleading.*

    We have now modified this sentence, following the comments of the referee (see lines 433-434).

  - *L. 395–399: The manuscript explains: "Indeed, the flow induced by a thinner tower (in diameter units), as in the case of the 15-MW wind turbine, might be better described by a potential flow solution compared to the one induced by a thicker tower, as in the case of the 5-MW wind turbine, and may thus lead to the observed improved agreement between BEM and CFD results." Since you make several direct comparisons with the results of 5MW turbine simulations, these comparisons become a central part of the discussion. To improve readability, I suggest including the relevant 5MW results directly in the figures,* *rather than expecting readers to switch between different papers.*

To follow this comment of the referee, we have now included a figure concerning the wake recovery data extracted by the paper of Santoni et al 2017 [1] (see reponses above).

- *L. 407–408: The sentence "Overall, it can be said that the performance drop due to the passage in front of the tower is somewhat more limited for the 15MW NREL turbine than for the 5MW counterpart (...)" could be made more precise. The repeated references to the 5MW turbine again suggest that the corresponding results should be explicitly shown and discussed here. Also, the phrases "it can be said" and "somewhat" are too subjective. A more precise wording might be: "Results indicate that the performance drop relative to the corresponding average performance is smaller for the 15MW turbine than for the 5MW counterpart." Ideally, a quantified comparison (e.g., percentage reduction or absolute values) would make this statement more rigorous. Further, the 5MW simulations were performed with uniform inflow, while the 15 MW simulations are performed using a sheared inflow. The lower wind speed in the lower part of the rotor plane means that the bottom half of the rotor plane, where the tower is located, will produce less than half of the total power. This will inevitably cause the performance drop due to the tower to be smaller relative to the total produced power. Therefore the observed difference is not only due to the change in turbine size, but also due to the change in inflow conditions.*

  The sentence has been rephrased in the following way: "It can be concluded that the performance loss induced by the passage in front of the tower is less pronounced for the 15 MW NREL turbine ($\approx 5\%$) compared to the 5 MW turbine ($\approx 15\%$), with both BEM theory and CFD yielding similar predictions in the case of the 15 MW turbine." Of course, also the sheared inflow can have a non negligible effect, and this point is now clearly aknowledged at several points in the paper (see, for instance, lines 453-459).

- *L. 410–411: The manuscript states: "Moreover, results seem to suggest that for very large rotors the presence of the tower may constitute a less critical issue for the blade deformations than for smaller rotors (...)". This phrasing is somewhat misleading. It is not possible to draw conclusions about "issues" such as fatigue without dedicated simulations or experiments. What is actually shown are differences in blade deformation predicted by aeroelastic simulations, which have not yet been validated for this case. I would therefore recommend rephrasing along the lines of: "Moreover, the present simulation results predict that for very large rotors the tower effect on blade deformations is less pronounced than for smaller rotors." At the same time, it would be important to acknowledge that this conclusion is based only on one 15MW sheared inflow simulation, where the tower is located in the part of the rotor plane where deflections are generally smaller, and one 5MW uniform inflow simulation, so broader generalizations are not yet possible.*

  The sentence has been changed according to the suggestion of the reviewer and refernce to the different inlet condition has been added at lines 453-459.

- *4.4 Structural response*

  - *L. 458–459: The manuscript states: "The amplitude of the deformation is however consistent with that obtained by Trigaux et al. (2024) using LES." The wording "consistent" is somewhat ambiguous here. It would strengthen the statement if you could clarify in which sense the amplitudes agree — e.g., whether they are similar in absolute magnitude,* *in relative deviation from a reference case, or primarily in qualitative trend.*

We have now reported quantitatively the values of the oscillations shown by Trigaux et al. (2024) in their figure 7b (see lines 511-512 of the present paper).

- *L. 474–476: The text reads: "However, the gap between the BEM and the CFD-CSD/T curves is quite large. This can be attributed to the different aerodynamic and structural model used in BEM and LES."*

  1. *The expression "quite large" is subjective; it would be clearer to either quantify the gap (e.g., percentage difference, RMS error) or replace it with a more neutral term such as "substantial" or "noticeable."*
  2. *The phrase "this can be attributed to" presents speculation as a fact. Since both the aerodynamic solvers (BEM vs LES) and structural solvers (modal vs torsional models) differ, the discrepancies cannot be uniquely traced to one factor. A more balanced formulation would be: "These differences likely arise from the combined effects of both aerodynamic and structural modeling approaches."*

     We have now rephrased these sentences as suggested by the referee, also providing a quantitative estimate of the error (see lines 517-518).

- *L. 491–493: "the second and third flapwise natural frequencies are indeed recovered by all the numerical models". These peaks are visible, because they are at the 13th and 26th multiple of the rotational frequency (13p and 26p). They are not visible because they are at the second and third flapwise mode, because flapwise modes have very large aerodynamic damping, which is also why the first flapwise mode is not visible.*

  We agree with the referee, although we haven't noticed that before. We have now changed the text according to this referee's comment (lines 543-546).

5 Conclusions

- *L. 522–527: The manuscript states: "The entrainment of kinetic energy driven by the tower leads to higher turbulence levels in the near wake, but then result into a slightly decreased mixing behind the turbine, differently to what has been found for the NREL 5MW wind turbine, whose wake recovery was found to be promoted by the presence of the tower. This finding can have important implications for the aerodynamic loads on downstream turbines in wind farms and overall farm efficiency." This is a counter-intuitive result, as prior studies (e.g., the NREL 5MW turbine case) suggested the opposite effect. Since the observation is based on a single solver without confirmation from experiments or other CFD-based aeroelastic simulations, I recommend formulating this more cautiously. For example, instead of "this finding can have important implications", you could write that "this result requires further examination, as it appears counter-intuitive and has not yet been confirmed by other studies." This way, the uncertainty is acknowledged while still highlighting the potential significance.*

  Thank you for the suggestion, we have restated this sentence in the manuscript (lines 579-580).

- *L. 551–553: The manuscript concludes that "CFD-CSD can capture complex aerodynamic loading and turbulent effects better than BEM." While this is likely true in general, the specific evidence presented here — larger amplitudes at lower frequencies — does not in itself* *constitute proof of that statement. It would strengthen the conclusion to either rephrase more cautiously (e.g., "In this case, the CFD-CSD solver captured larger low-frequency fluctuations than BEM") or to provide additional evidence that directly supports the broader claim.*

We have rephrased the sentence in a more cautiously way (see line 604-605).

- *L. 562–563: The differences between CFD-CSD/T and BeamDyn are described in a way that attributes them to solver fidelity. However, since the two approaches differ in both aerodynamic fidelity (LES vs. BEM) and structural fidelity (torsional vs. modal models), it is not possible to unambiguously assign the discrepancies to one component. I recommend rephrasing along the lines of: "The observed differences likely stem from the combined effects of differences in aerodynamic and structural fidelity, and cannot be uniquely attributed to one component alone."*

  We have rephrased the sentence as suggested by the referee (see lines 617-619).

Appendix A Grid Convergence Study for the LES Simulation

- *L. 573: To demonstrate a clear trend in grid convergence, more than two grid resolutions are typically required. With only coarse and fine grids, it is difficult to establish whether the solution is truly converging. One possible approach would be to use the finest grid as a reference and compute a root-mean-square (RMS) error relative to it. Plotting the RMS across multiple grid resolutions would then allow you to illustrate the convergence trend more quantitatively.*

  Done (see answers above).

- *L. 579: The manuscript states: "The comparison in figure A1 shows that the results obtained using the coarse and fine grids are extremely close each other along the entire blade span." This phrasing is somewhat vague and subjective. To make the convergence assessment clearer, it would be better to quantify the agreement, for example by reporting RMS errors or percentage deviations. Also, there is a small grammar correction: it should read "close to each other" rather than "close each other."*

  The updated figure has now been discussed in much more detail. Also, the typo has been corrected.

Appendix B. Validation of the Structural model

- *Caption: Please ensure consistent use of title case or sentence case across the entire manuscript. For example, the current caption "Appendix B. Validation of the Structural model" mixes styles. The WES journal guidelines specify which convention should be followed — it would be good to align with that.*

  Looking at the journal guidelines (Titles and headings follow sentence-style capitalization i.e. first word and proper nouns only), we now used the sentence style, not capitalizing the first letter of nouns. This has been now done throughout the paper, in a consistent way.

- *L. 599–600: The manuscript states: "The computed values of the modal frequencies appear to be consistent with the other results, although some discrepancies in the higher-order modes* *are observed." Since the 6th mode (corresponding to the first torsional mode) is likely the most relevant for the present study, it would strengthen the paper to explicitly address these discrepancies. Please clarify in which sense the results are "consistent" and provide more detail on how the 6th mode compares across solvers.*

As now clearly reported in the paper, the structural model for the IEA 15MW wind turbine has been cross-validated with many other aeroelastic numerical codes within the framework of the International Energy Agency (IEA) Wind TCP Task 47 TURBINIA [3]. In this IEA Task, a consortium of research institutions and industrial partners benchmarked their own aeroelastic codes on the IEA 15 MW wind turbine [4]. Since we cannot report in this paper data from all these partners, we have provided here a comparison of literature data. Concerning the latter, we have now clarified the discrepancies and added more details about this comparison.

- *Figure B2:*
  - *From my understanding, the 6th mode (first torsional mode) should be central to your analysis. I would expect clustering of results for solvers without torsion degrees of freedom (e.g., purple, green) as distinct from those including torsion (e.g., red, blue, grey, black). However, this clustering is not clearly visible. Is it possible that there has been a mix-up between the 6th and 7th modes in Table B1? For instance, in the 7th mode the aeroelastic solvers without torsion do not appear, whereas they do for the 6th mode. Even if this is not the case, a short discussion of why this expected separation is not observed would improve clarity.*

    We thank the reviewer for the comment as it highlighted a mistake in the order of the modes from the literature.

    Indeed, we confirm that our modal analysis provides a 6th mode with torsional nature. However, as noticed by the reviewer, some of the mentioned solvers do not include torsional degrees of freedom in their structural models. Hence, as the natural frequencies of the torsional modes are missing, there cannot be a perfect correspondence between the order of the modes of the various models. In particular, the frequencies erroneously indicated as 6th mode for ElastoDyn and H2-PTNT can only correspond to flexural modes.

    According to these observations, we have corrected the figure and we have moved the natural frequencies of the former 6th mode of ElastoDyn and H2-PTNT to our 7th mode, which is instead an edgewise mode that can be captured by these models and has a similar frequency as well.
  - *There is also a large spread in the higher-order modes. It could be informative to explicitly separate the results into torsion and no-torsion degrees of freedom. For example, ElastoDyn does not include torsional degrees of freedom, so one would expect systematic differences relative to models that do. The same applies to H2-PTNT.*

    We thank the reviewer for the suggestions, and we trust that the previous answer clarified the separation between torsional and non-torsional modes.

    Spread between the higher-order natural frequencies in the literature, instead, may reasonably stem from the differences in the models used. As higher frequencies are considered, we observe that coupling between the torsional, flapwise, and edgewise degrees of freedom arise, which may be potentially affected by the different modeling assumptions adopted in the definition of the mass and stiffness matrices, or even by differences in the material properties used. However, we believe that our results are within the tolerance observed in the literature and thus guarantee a proper validation of the structural model used in this work.

Sources

- *The reference "Hansen, M. (2015). Aerodynamics of wind turbines. Routledge." should be adapted to match the citation style required by WES. At the moment, the publisher and format do not appear consistent with standard referencing. Please check the journal guidelines to ensure proper formatting. For example, depending on the required style, the same reference would look as follows:*

  We have used the bst file provided by copernicus for the bibliography section. We carefully checked the biblio file and the reference above is correctly inserted. We think that the publisher may fix some formatting problems on the bibliography, in case they would be an issue.

  - *APA Style: Hansen, M. O. L. (2015). Aerodynamics of wind turbines (3rd ed.). Earthscan.*
  - *Harvard Style: Hansen, Martin Otto Laver, 2015. Aerodynamics of wind turbines. 3rd edn, Earthscan, London, UK.*
  - *Chicago Style: Hansen, Martin O. L. 2015. Aerodynamics of wind turbines. 3rd ed. London, UK: Earthscan.*
  - *Vancouver Style: Hansen MOL. Aerodynamics of wind turbines. 3rd ed. London, UK: Earthscan; 2015.*

References

[1] Christian Santoni, Kenneth Carrasquillo, Isnardo Arenas-Navarro, and Stefano Leonardi. Effect of tower and nacelle on the flow past a wind turbine. *Wind Energy*, 20(12):1927–1939, 2017.

[2] Wen Zhong Shen, Robert Mikkelsen, Jens Nørkær Sørensen, and Christian Bak. Tip loss corrections for wind turbine computations. *Wind Energy: An International Journal for Progress and Applications in Wind Power Conversion Technology*, 8(4):457–475, 2005.

[3] J. G. Schepers, K. Boorsma, R. Bois, G. Bangga, J. Jonkman, C. Kelley, E. Branlard, W. Gonçalves Pinto, M. Imiela, O. Hach, L. Greco, C. Testa, N Aryan, H. Madsen, A. Croce, S. Cacciola, G. Pirrung, N. Sørensen, C. Grinderslev, C. Bernardi, S. Cherubini, A. Bianchini, F. Papi, L. Pagamonci, C. Braud, L. Höning, J. Theron, and K. Mohan. Turbinia, turbulent inflow innovative aerodynamics. Technical report, IEA Wind TCP–Task47, 2025.

[4] S Cacciola, A Croce, G. Bangga, G. Pirrung, Madsen H., N Sørensen, G. Grinderslev, N. Bonfils, E. Persent, I Gilbert, A Joulin, L. Greco, N Aryan, A Castorrini, A Morici, M Chetan, J Jonkman, E. Branlard, S. Cherubini, C Bernardi, K. Boorsma, J. D. Schepers, A Bianchini, L Pagamoci, F. Papi, O. Hach, M. Imiela, and D. Witt. A comparative study of different modeling tools and analysis techniques for aeroelastic stability assessment. In *submitted to The Science of Making Torque from Wind*, 2025.

The revised manuscript with the highlighted changes mentioned in this point-to-point document is attached in the following pages.

Point-to-point reply to reviewers

Paper wes-2025-120

Title:

Large Eddy Simulation of the IEA 15-MW Wind Turbine Using a Two-Way Coupled Fluid-Structure Interaction Model

December 12, 2025

The authors wish to thank the reviewers for their comments and suggestions on the paper to enhance the manuscript. The paper has been revised accordingly. The revised parts have been highlighted in blue (for this referee) and red (for referee 1) in the updated manuscript. If mentioned, the page number refers to the revised paper. In the following we report point-to-point replies concerning the questions and comments raised by the reviewers.

Reviewer #2

Main Critique Points

1. *The term "aeroelasticity" is well known, I suggest removing this definition.*

   We have eliminated this sentence.

2. *Strictly speaking, fatigue itself is not an aeroelastic phenomenon, but aeroelastic effects can cause or accelerate fatigue in wind turbines.*

   We have specified that in the text. The referee can find the revised version of the manuscript attached and follow the text in blue.

3. *I suggest to slightly rephrase this sentence, softening it.I agree with the authors that BEM aerodynamics has significant limitations. Anyhow, it is currently the state-of-the-art solvers for practical industrial applications and certification. Moreover, beside highlighting its limitations, several works have demonstrated that, if properly tuned, it can be a valuable engineering-type solver, complementary to higher fidelity ones which have also higher computational costs. See, for instance, the results of IEA WIND Task 47..*

   We have restated the text in this paragraph, and cited the references reported by the referee.

4. *I don't agree with this point of view. It is true that panel methods (and in general potential flow solvers and/or free-vortex wake methods) and ALM solvers are computationally more expensive than BEM. Nevertheless, they are less expensive that blade-resolved CFD. Furthermore, many papers show that panel codes and free-wake vortex methods are able to capture unsteady blade/rotor aerodynamics with good accuracy in different operating conditions (including off-design) whenever massive flow separation phenomena do not occur. See, for instance:..*

   We have restated the text in this paragraph, and cited the references reported by the referee.

5. *From this sentence it appears that CFD is described as the best compromise between accuracy and computational costs. Differently, within the research topic addressed in this work, CFD should be seen as the reference high-fidelity approach that can be used for a (limited) number of computations in order to refine and/or assess the lower fidelity models.*

   We agree with the referee and we have restated the text in this part.

6. *This sentence is not clear. The pargraph describes tools such as FAST which is not based on a lifting-line aerodynamic formulation.*

   Sorry it was meant to be "BEM" instead of lifting line. We have corrected that in the text.

7. *At this point is not clear if, within the aeroelastic computation, the ALM elements follow blade deformation or not.*

   We have now specified that the ALM elements do not follow the blade deformation.

8. *Please provide more details about the procedure used to compute the flow velocity contribution to urel. Which approach was used (Line Averaging Technique, for instance, or others?).*

   The definition of u_{rel} is given in eq. (7). The interpolation from the structural FEM nodes for u_{def} to the Cartesian CFD mesh on which u_{abs} is defined, is made with a simple linear interpolation. Then, the resulting u_{rel} is interpolated on the nodes discretizing the blade.

9. *Although the authors refer to previous works for the structural model, it is preferrable to briefly outline it here. Which type of beam model is used (Euler-Bernoulli, Tiomoshenko, others? Is it a linear model or a nonlinear one? Are the Coriolis forces and the spin-softening effect taken into account?*

   We have now specified that it is a linear model taking into account Coriolis, centrifugal and Euler effects. Concerning the type of beam model (Euler-Bernouilli), it is specified in the text under eq. 6.

10. *Please indicate if the modal analysis is performed on the rotating or nonrotating blade. In the latter case, is it performed past the deformed or undeformed configuration?*

    We have now specified this at several points in the paper.

11. *The authors state that they are using a modal approach to solve the aeroelastic equations. How is the Lumped Mass representation here mentioned linked to the modal approach? As a general comment, in my opinion the description of the structural model is not clear and lacks suitable clarifications*

    Following the approach of Reschke (2005) and Saltari et al. (2017), the inertial coupling terms arising from the noninertial frame of reference are described using a reduced set of coefficients, which are estimated via finite element method discretization. Details can be found for the specific case of wind turbine's blades in Della Posta et al. (2022). The lumped mass representation has thus been used to define the mass matrix of the blade, to evaluate the inertial coupling terms according to the mentioned method, and also as an input of the eigenproblem prividing the natural frequencies and modes used in the work.

    Also, we have considerably changed and extended the description of the structural model.

12. *Usually in modal approaches, the quantity d includes, for each blade section, flap (w), lag (v) and torsion (theta) DOFs. Consistently, also w' and v' will appear in the aeroelastic equations, so they will be computed as part of the aeroelastic solution. It is not clear to me*

why the torsion angle is here accounted for in addition to d.

Flap and lag are indeed considered in the structural dynamics solver, and their time derivatives are considered in the definition of the relative velocity in equation 7 as $\boldsymbol{u_{def}}$, which is defined on the basis of the translational contributions of $\dot{\boldsymbol{d}}$ only, as clarified now in the text. As explained after equation 8, $v_2 = u_{def} \cdot E_2$ is the flapwise deformation velocity component, and $v_3 = u_{def} \cdot E_3$ is the edgewise deformation velocity component, indicating the translational structural dynamics. The torsion θ is also considered as part of the elastic state relevant for the aeroelastic coupling, in order to take into account angular deformations that change the actual orientation of the local airfoil on the deformed blade with respect to the impinging flow.

13. *What do you mean by "bendings"?*

    Bendings here indicate rotations around X_2 and X_3, which describe the edgewise and flapwise (in-/out-of-plane) angular deformations respectively.

14. *It is important to provide an indication of the computational costs of a typical solution.*

    The increase in computational cost induced by the structural solver is negligible, as it accounts for less that 2%.

15. *Please describe how this is computed, as it is one of the critical aspects of ALM methods.*

    Please, see answer to comment number 8 of the present referee.

16. *I am confused by the appearance of θ_{tors} here. If I understood correctly, the modal dofs already include both bending (flap and lag) and torsion. Please clarify.*

    As now mentioned in the revised methodology, once the modal dynamics is advanced in time, the displacement is projected back onto the physical base to recover its expression in the rotating frame of reference. We have also explicitely explained in the revised manuscript that $\boldsymbol{u_{def}}$ only considers the time derivative of the translational degrees of freedom, so that the presence of θ_{tors} should be clearer in eq.8.

17. *OpenFAST is a widely used tool, so, in my opinion, there is no need to devote a specific section to its description. I suggest that simply specifying the main characteristics of the aerodynamic and structural formulation (sepcifically indicating if blade torsion has been included or not - i.e. if BeamDyn or Elastodyn has been used) is sufficient.*

    We have now eliminated this subsection and included only a small paragraph in the next section.

18. *I presume the shear inflow is imposed at the inlet, is it correct? If so, using a no-slip condition at the bottom will modify the actual inflow at the rotor disk. Please clarify this aspect.*

    We have now explained in the text that indeed the flow changes from the inflow to the turbine, but the provided profile comply with the no-slip condition.

19. *Rotating or nonrotating? Past the deformed or undeformed configuration (if rotating)?*

    We have now specified that it is the rotating undeformed configuration.

20. *Is the pitching moment computed past the c/4 point of each blade section?*

    We confirm that.

21. *I suggest changing the scale of the contour plot because it is difficult to appreciate the different TKE levels. If this is difficult considering the predicted values of TKE, it may help showing directly the TKE difference between the TN and OR case. Moreover, the figures are really small.*

    We have enlarged and reorganized the figures. The slices are now presented in a single column to enhance their readability.

22. *I don't understand the values y/D indicated in the plot. If I see correctly from Fig.5 the tower base is at y/D=0, so why would these horizontal planes be at y/D¡0? Moreover, top panels of Fig 6 show a deviation of the hub wake from the turbine rotation axis. Something similar happens when the rotor is in yaw, but here it is not the case. It would be interesting to provide a justification for this phenomenon.*

    In this figure y=0 was erroneously set at the hub of the turbine, we have changed that for consistency, introducing the value h_{hub}. The deviation of the hub wake from the turbine rotation axis is not due to the yaw but to the mutual effect of the asymmetry induced by the rotation of the blades and the wake meandering. Concerning the second effect, the wake is subject to low frequency oscillations, and in this snapshots it happens to be oscillating towards higher values of z, which combines positively to the slight effect towards higher values of z of the direction of rotation of the blades.

23. *Considering the number of lines in the figures it is quite difficult to recall everytime which result correspond to which line/marker. I suggest adding a graphical legend to the plots.*

    Done.

24. *As a general comment to Figs 8 and 10, I suggest showing only the fluctuation of the quantities of interest, while reporting the mean values in suitable tables. As the paper is dealing with aeroelasticity, unsteady fluctuations are more interesting than the mean values. Differently, using the current plots, they are not easily appreciable.*

    We have tried to plot only the fluctuations in figures 8, 10 and 12, removing the mean values, but the plot becomes very confusing since many of the linea are superposed. For completeness, we report the plot in figure 1 of the present document. This considered, we have decided not to remove the mean from the figures 8, 10, 12.

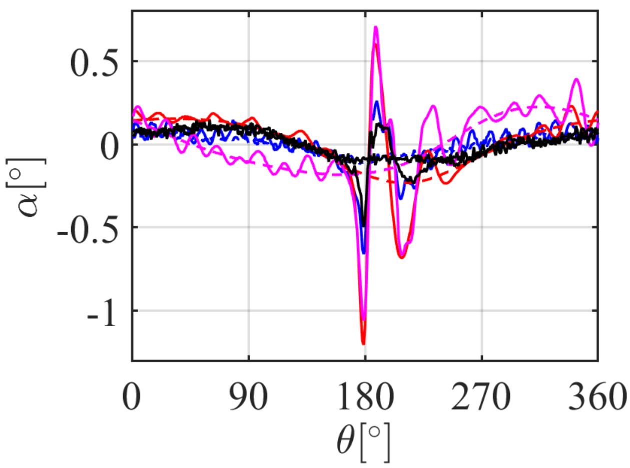

Figure 1

25. *Here flapwise and tangential force are mentioned. Before they were F1, F2. Please define a unique nomenclature for these quantities.*

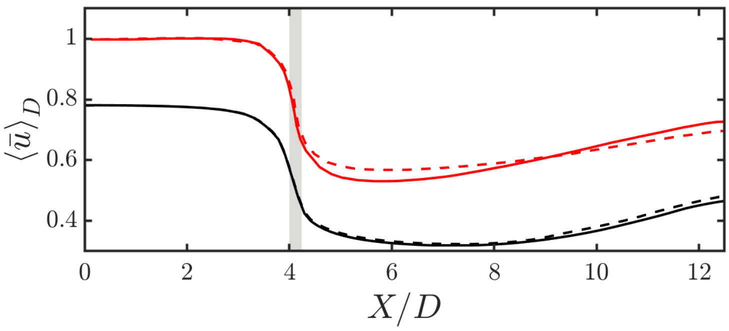

Figure 2: Rotor-averaged velocity along the streamwise direction normalized by the undisturbed velocity at the rotor height, namely, $U_\infty = 10\ m/s$, for the present data (black curves) and the work of Santoni (2017) (red curves). The grey region represents the area covered by the rotor. (RO - - - -, TN ——).

We have now clearly defined the flapwise and edgewise forces at the end of page 12 : "flapwise and edgewise components (normal and tangential to the rotor disk),respectively, of the aerodynamic force per unit length F_2 and F_3". In the reminder of the paper, we now use F_2 and F_3 within the figures, and "flapwise and edgewise " in the text.

26. *This metrics is intuitively clear, but needs a formal definition.*
    Done.

27. *Why are the left and right contour maps different? In addition, it would help the figure clarity if a sketch of the rotor/tower were included (maybe using transparency).*

    The colormaps have been unified. Also, a sketch of the tower is now included in each subfigure.

28. *The parallel between the 5 MW and the 15 MW behaviour is very interesting and involve several aspects. Anyhow, for the sake of readibility, the results related to the 5 MW which are needed to support the comments here addressed must be here reproduced. Otherwise it is quite difficult to follow the comments on the basis of the textual description of plots not shown here.*

    Concerning the comparison with the 5MW turbine, as suggested by the reviewer, we have included in figure 4 of the revised paper, reported here for completeness as figure 2 of the present response, a reproduction of the nondimensionalized plot from Santoni et al. (2017) in a comparable format that allows the reader to interpret the differences more clearly. Also, we have included the results of Bernardi et al. (2023) in figure 10 of the paper. Moreover, to comply with another referee's comment, we have eliminated some further comparison with the 5MW turbine results.

29. *Which comparative 5 MW-15 MW result support this conclusion? An explicit comparison of the mentioned results is needed here.*

    We have now included the curve of the figure 3 of Bernardi et al. 2023 in which it can be clearly seen that the presence of the tower induces larger oscillations in the power coefficient. However, since the inflow was different in this literature case, we have restated the sentence as: "Moreover, the present results predict that, for very large rotors and a sheared inflow, the tower effect on blade deformations is less pronounced than for smaller rotors".

30. *Indicating the value of frot would help the readibility.*

    We have now reported this value.

31. *Although well known, a formal definition of 1P, 2P and so on is needed.*

    Done.

32. *Considering the combination of frequencies experienced by the rotor, the expected result in terms of thrust and power (both axial loads) is that only the multiple of blade passage frequency are retained in the PSD (so 3P, 6P and so on). This is actually confirmed by all proposed solvers here, except Beamdyn, which shows a significant peak at 1P. The authors should comments and justify this unespected finding.*

    We appreciate the reviewer's observation regarding the presence of a peak at 1P in the spectra of the power and thrust coefficients for the BeamDyn-only configuration.

    The presence of this peak may be likely explained by the fact that the baseline configuration of the wind turbine used in our simulations considers an additional degree of freedom that takes into account the mechanical connection between the blades.

    We acknowledge that this aspect warrants further investigation, and we plan to examine it in more detail in future work. Nevertheless, we would like to emphasize that the amplitude of the 1P peak is several orders of magnitude smaller than the dominant spectral components at higher frequencies. As a result, its contribution to the overall system dynamics—and thus to the conclusions drawn in this study—is negligible. Therefore, while the mechanism behind this peak is of scientific interest, it does not materially affect the results presented here.

33. *This aspect is quite critical for the comparison between BEM-based and LES-based solvers. How can the authors be sure that the imposed inflow at the rotor disk between the two approaches is the same (at least to a certain extent)? An analysis of the inflow velocities on the reference blade along the azimuth angle is needed to quantify the differences in the wind incoming to the rotor. Moreover, in Task 47 many analyses have been performed (aslo by some of the authors) on how to align the inflow to BEM and to CFD (altough in that case it referred to the turbulent inflow). Anyhow, I suggest to investigate if any of those methodologies can be used here to align the inflow to BEM and to CFD.*

    We have indeed compared the aerodynamics of the present code with other ones in the IEA Task 47 "TURBINIA". In figure 4.25 of the report of Task 47 (Shepers et al. 2025), the differences in rotor aerodynamic modeling between different codes are highlighted by the time traces of hub height wind speed and axial wind velocity at a co-rotating probe on the turbine blade. Before comparing the rotor simulation results it was ensured that identical inflows were prescribed between the different codes. Figure 4.25 displays that the lifting line codes do not agree well with CFD ones on the hub height wind speed and the axial wind speed of the most outboard co-rotating probe. The differences originate from the fact that for the CFD codes the rotor induction cannot be separated from the undisturbed wind field. Hence, also when identical inflows are imposed, CFD codes have a lower level of the axial wind speed compared to lifting line ones, due to the rotor blockage. This is an intrinsic different between these two types of modeling, which do not allow to make a one-to-one comparison of the aerodynamics. Although this discussion is very relevant to this paper, we cannot include it in the present paper since those data are not public. However, we have included a sentence about this point in the paper.

34. *Also for this analysis I suggest removing the mean values from the plot (mentioning them in a table) and focusing only on the fluctuations (this intended for the phase-average results).*

    We have tried to do that but the plot became confusing, since the lines will be mostly superposed. Thus, we have chosen not to follow this comment.

35. *Again, if a literature results needs to be used as a comparison, it is better to show it here in order to avoid the reader going back and forth from this paper to those referenced.*

    We have now included the results of Trigaux et al (2024) in figure 12.

36. *This result is espected as the lag deformation is mainly driven by gravity.*

    We have now specified that.

37. *In my opionion this conclusion is quite weak if not supported by a sensitivity analysis on the number of needed modes in Elastodyn. Moreover, to me it is still unclear if the OV model really does not take into account blade torsion (see my comment on this aspect above), so, considering the abscence of torsion in Elastodyn, the discrepancy could be related also to this (although this is in contrast with the lower value of lag deformation at the tip). This aspect requires more in depth investigation.*

    Concerning our CFD code, as explained at page 8, the CFD-CSD/OV approach considers the torsional degree of freedom, but does not include it in the two-way coupling. Whereas, Elastodyn does not consider the torsional degree of freedom at all. Thus, we agree with the referee that this discrepancy can be related also to this point. However, we cannot extend the ElastoDyn module to consider more degrees of freedom, since it is embedded in the OpenFAST code only in this form. We have thus discussed this point in the text and stated at page 12-13 that "the four solvers employed differ in both their aerodynamic and structural modeling approaches. Moreover, the flow that impacts the turbine is not exactly the same for the CFD and OpenFAST solvers, since in the former case it is imposed at several diameters upstream the rotor plane. As a result, it is not always possible to unambiguously determine whether the observed discrepancies in the results originate from the fluid-dynamic models or from the structural formulations".

38. *Please reproduce the mentioned results in your plots!*

    Done

39. *General comments to Fig 13: - the labels indicating blade eigenfrequency are hardly readable; - I suggest adding the grid lines to better appreciate the different values of the peaks. Moreover, it seems clear that blade deformation dynamics is governed by the lower components of the spectrum (the analysis of the energy content of the different harmonics should confirm this). So, although frequencies higher than the 1st flap appear, they have a very small magnitude. I suggest to reduce the x range to show only the most significant harmonics. Also the y range should be changed because as it is now it is very difficult to appreciate the peaks values. I understand that the author's aim was to show the consistency of flap/lag/torsion prediction by evidencing the role of some of the blade natural frequencies. I suggest to do this analysis only for one DOF and change the other plots using a reduced frequency range.*

    We have increased the labels and added the grid lines. Also, we have increased the y axis for making the peaks more visible. The figure has an improved readbility, we thank the referee for this comment.

40. *This aspect is not clear (see my previous comments on this).*

    Please, see the answer to the comment number 37.

41. *This sentence is quite general and not supported by specific evidences. Aeroelasticiy is a multidisciplinary topic so it must be tackled by a step-by-step approach. I agree with the choice*

presented in this work where the structural model is assessed against available literature data. What is somehow missing is its aerodynamic counterpart. Indeed, I suggest adding a purely aerodynamic comparison between the proposed CFD and other data (FAST can be an option, but ideally solvers with a similar fidelity as the one here proposed would be more appropriate). In this way the origin of the observed discrepancies can be related to the structural model, to the aeordynamic one or to their coupling.

Please, see the answer to question 33 of this referee.

42. *As the response of the system should be governed mainly by the frequencies arising from the combination of contribution stemming from aerodynamics, I suggest to comment the origin of the peaks shown in Fig 13. For instance, the rotor rotation frequency appears clearly (and it is dominant). What about the others?*

    This has been done at page 23.

43. *This is not a limitation of OpenFAST but is related to the different inflow provided to the two solvers (see my comment on this above).*

    We have now stated that in the text, although we specify that the CFD inflow is not turbulent, but has indeed variations in the transverse direction.

44. *Please reproduce here the cited result.*

    In this particular case we have not included the cited results in the plot since the curve has many peaks and extracting it point-by-point would have induced large errors. However, we have included all the referenced results about the power coefficient and deformations, whose curves are rather simpler and more suitable to the extracted point-by-point.

45. *This is not a limitation of OpenFAST but is related to the different inflow provided to the two solvers (see my comment on this above).*

    We have recalled here about the different inflow, although we do not think it is only due to the inflow conditions, which differ only slightly, but to the simplified treatment of aerodynamics.

46. *This very difficult to be seen from the plots in their current layout. Moreover, the ability of Blade Element Momentum Theory (which is inherently steady) to capture high-frequency aerodynamics is questionable. Considering the very small values of the harmonics shown around blade modes, I am not convinced that the BEM-based simulations can be so reliable.*

    Here we do not refer to the high-frequency aerodynamics effects (i.e., turbulence), but to the aeroelastic (structural) high frequency vibrations. We have now better specified that.