Firenze, 18/02/2026

Dear Editor,

We would like first to thank the Reviewers for the accurate and qualified observations that have made major improvements in the paper possible.

Based on the Reviewers' comments, a throughout revision of the work has been carried out and parts of the paper have been expanded.

Our comments and responses to Reviewers' comments have been highlighted in blue-colored text in this communication and are visible as revisions in the track-changes version of the paper. We really hope that this version can be now worth of publication in *Wind Energy Science*.

Best regards,

Francesco Papi, Pier Francesco Melani, Alessandro Bianchini

Reviewer 1

In this paper, the authors conduct high-fidelity URANS simulations on different airfoil families to determine the aerodynamic coefficients under a wide variety of operating conditions. Overall, the current research contributes to improving the understanding of blade design and efficiency using new types of airfoils and providing a complete database to be used by the community for this purpose. Before conducting the final simulations, the authors carefully conduct domain, grid and timestep sensitivity analyses. The data set provides a solid foundation for future research to build upon and offers novelty by incorporating compressibility effects. However, the main weakness of the paper is the lack of clarity in several key justifications or model assumptions, which makes the computational setup and results difficult to interpret.

After reviewing the manuscript, I have grouped my comments into two categories, major concerns and minor corrections/suggestions.

Major comments:

1. Overall, the text is poorly written. Below are the main issues that should be addressed:
    1. First of all, the authors do not cite properly the references. Throughout the entire text citations in brackets are used, without differentiation between \cite and \citep commands (if written in latex). Since this happens in the entirety of the text, it really decreases its quality.
       We are aware of this inconsistency. It is caused by the reference management tool we use internally, and we usually correct it at a final stage if the manuscript is accepted. However, we have now corrected the inconsistency the reviewer pointed out.
    2. Secondly, hyphens and en dashes are not used appropriately, or authors are not consistent with their use throughout the whole text. For example, in some parts free-stream is used and in others freestream. Please check the regulations on hyphens, en dashes and em dashes and the specific comments in the minor suggestions section below.
       We have revised the entire text based on this comment.
    3. Punctuation after equations is not used at all. After each equation a comma or a full stop should be added. Except if they are followed by ‘and’ or unless punctuation is not required at all.
       Thank you for pointing this out. We have checked the manuscript guidelines and added commas after the equations.
    4. Moreover, equations are not referenced correctly. Equations should be referred to by the abbreviation "Eq." and the respective number in parentheses. For example: in Eq. (4). Referencing the equation before stating the formula is also redundant, see Lines 258 and 260.
       Thank you for pointing this out. Referencing the equation before the formula is generally encouraged by editors as it helps the readability of the manuscript in case the equation is separated from the text after formatting. We have also corrected one instance where “Eq” is not capitalized.
    5. SI units are not in the correct format. Regarding the **notation of SI units**, if units of physical quantities are in the denominator, contain numbers, and are abbreviated, they must be formatted with negative exponents (e.g. 10 km h^{-1}instead of 10 km/h) (see Table 2 in the manuscript).
       Corrected.
    6. There are major capitalization errors. Titles and headings follow sentence-style capitalization (i.e. first word and proper nouns only). This applies to table and figure headings as well. For example, in the text there are mentions of Lift-to-Drag ratio.
       We did not find any section heading capitalization errors. We have corrected all the figure captions. Thank you for pointing this out.

    7. There are many sentences that are too long to read and follow. For more details see minor comments below. Also please check for articles missing, such as 'the'.
       Thank you, we have reviewed all of the highlighted sentences.
    8. The paper is missing a clear structure. First of all, in the introduction itself a paragraph (usually at the end) outlining the overall structure of the paper and the story is not present. Moreover, the authors in no Section used introductory paragraphs to explain the purpose of each section, making the paper and the flow hard to follow.
       The paper follows a fairly classical Introduction – methods (including dataset description) – results structure. However, we agree with the Reviewer that adding an outline of the paper at the end of the introduction could improve the clarity. We have added it accordingly.
    9. The most concerning issue is the captions under figures. Almost every caption is wrong. The description does not align with the variables presented in the figures, references are cited wrong, sometimes the legends/labels do not much the plotted lines, variables used in legends are not fully explained and plot limits are sometimes wrong. Moreover subcaptions, such as (a),(b) are used incorrectly and inconsistently. In the minor comments section, the specific instances are further explained.
       The Reviewer is right; many figures in the second half of the paper featured incorrect captions. This was a result of many slight revisions that were performed to convey the data more effectively. We have reviewed all the figure captions and checked for inconsistent capitalization, lettering or errors.

2. The initialization of the URANS simulations in Section 3.1 requires further clarification. Although the authors state that the simulation starts from steady RANS fields, it is not specified whether these RANS computations were laminar, turbulent, or included transition modeling. Moreover, the inflow conditions should be specified.
   Thank you for the comment. The RANS initializations are run in the same exact conditions of the following URANS runs. Because this approach is used in all runs within the dataset, and not only during validation, we have added this clarification to Section 3: *"The simulations are initialized from a steady Reynolds Averaged Navier-Stokes (RANS) simulation with the same inflow conditions and model set-up as the following URANS runs. RANS simulations are run for 2000 iterations or until all residuals drop below 1e-6. After initialization, the simulations are run for 50 chord-based throughflow times or until numerical convergence, where numerical convergence is achieved if the percentage variation over the first and last values used to compute the mean aerodynamic coefficients is less than 0.5%."*
   We have also better specified the boundary conditions that were prescribed, as requested: *"An open-field, bullet-shaped domain is used as shown in Fig. 4. This geometry allows for the use of a single inlet boundary, where inflow velocity, turbulence intensity and length scale are specified and a single outlet boundary, where the free-stream pressure is imposed."*.
   Finally, we have better specified the inflow conditions used for the domain sensitivity, as shown in the response to the Reviewers following remarks. To avoid possible confusion, we prefer not to specify the turbulence intensity as it does not influence fully-turbulent results, which were chosen for this reason.
3. In Section 3.1, where the domain sensitivity analysis is conducted several critical details are missing.
    1. In the current domain setup, how long is the domain in the streamwise direction? Is it enough for the wake to develop and for possible disturbances to not contaminate the flow around the airfoil?
       All boundaries are placed 600 chords away from the airfoil, which ensures they do not interact with the airfoil itself. This includes the outlet. The text has been amended as follows: *"An open-field, bullet-shaped domain is used as shown in Fig. 4. The domain boundaries are placed 600-chords away to avoid interference with the pressure distribution on the airfoil, as discussed in more detail in Section 3.1, while allowing the airfoil's wake to fully develop."*

    2. Moreover, the Mach number should be specified as well, and if incompressible operation is taken into account it should be justified why this was selected over compressible scenarios.

       The flow is considered incompressible in this step due to the low Mach number of 0.08. We have included this in the text: *“The influence of domain size on the predicted performance in fully turbulent conditions at a Reynolds number of 15*106 is shown for the DU00-W-212 airfoil as a representative example in Fig. 5. Owing to the low Mach number of 0.08, the flow is treated as incompressible.”*

    3. Finally, the grid refinement between the domains should be explicitly mentioned and justified, as changing mesh resolution could influence numerical dissipation and turbulence resolution.

       The Reviewer is correct in pointing this out. The same grid resolution is maintained across all the domains. The total number of elements varies accordingly as a function of the domain size. We have added this clarification: *“The same grid resolution is maintained across all domain sizes by adjusting the total number of elements accordingly, thereby isolating the effect of domain size.”*

4. Regarding the timestep sensitivity analysis, stability should be also ensured apart from convergence. The timestep is reported in terms of chord flow-through times, but this does not directly indicate the local stability of the scheme. Therefore, the corresponding CFL numbers should be also reported.

   Thank you for the comment. The Navier Stokes equations are solved using an implicit coupled resolution scheme, which is able to maintain numerical stability even in the case of high CFL numbers. The CFL can reach locally-high values due to the very small cells that are used around the airfoil. For the final calculations the Courant number is approximately 50, which is within the range of allowed values for the solver. Please see the Ansys Fluent User Guide for more information (https://ansyshelp.ansys.com/public/account/secured?returnurl=/Views/Secured/corp/v242/en/flu_ug/flu_ug_sec_solve_calc_time.html?q=Performing%20Time-Dependent%20Calculations). We have added a clarification regarding the flow solver: *“A sensitivity to the timestep length is shown in Fig. 8. Although an implicit coupled solver is used to numerically solve the Navier-Stokes equations, which ensures numerical stability even for very high timesteps in problems such as the one evaluated herein, the timestep must be small enough to accurately resolve the relevant flow features.”*

5. The title in Section 3.2 is quite misleading. This section serves more like a validation study against reference data, than a quantification of compressibility effects. The considered Mach number is very low for compressibility to have a significant effect and justify a 2.2% reduction . It would be helpful to confirm this behavior by performing a similar comparison at a more representative Mach number. The results and the goal of this paper could become more substantiated if a comparison with low-order methods could be added, so the differences and the advantages of the current methodology and subsequent dataset would be highlighted.

   Based on the Reviewer’s suggestion we changed the title to “Verification of the numerical model”. Regarding the effects of compressibility, an ample discussion is provided in the results section, where the effects of Mach number on aerodynamic efficiency, design lift coefficient and other relevant parameters are shown. The goal of Section 3.2 is first and foremost to show the good agreement of the simulation set-up used in this study with respect to other numerical tools and secondly to highlight the small but tangible impact of compressibility even at low Reynolds numbers, better motivating the inclusion of these effects in this dataset. It must be noted that compressibility effects can be included also in low-order methods such as XFoil if the user desires. Based on these aspects we believe that a full comparison between the methods would distract from the main message of the paper and have chosen not to include it. We have improved the comment of Figure 9: *“In addition to highlighting very good agreement between the current set-up and the reference data, Figure 9 (a) also shows a 2.2% decrease in the lift-to-drag ratio at the angle of attack of peak efficiency of 8°. This measurable reduction in efficiency is* *present despite the relatively low inflow Mach number of 0.075, motivating the inclusion of compressibility in the dataset."*

6. For the SBES simulations presented in Section 3.4, post-stall regime is also modeled. However, the authors do not explain how timestep was selected. Capturing unsteady stall phenomena typically requires a significantly finer timestep than pre-stall conditions. The authors should therefore provide and clarify the criteria based on which the timestep was selected.
   The Reviewer is correct in pointing out that this detail was missing. Indeed, a very fine timestep was used. It was chosen in order to guarantee a CFL of approximately 2 around the airfoil and below 5 in the entire domain. This ensured that the SIMPLE solver which was used to couple pressure and velocity converged robustly. We have included this in the text: *"The SIMPLE pressure-velocity coupling solution algorithm is used. The timestep is chosen in order to guarantee a Courant number (Courant et al., 1967) that does not exceed 2 in most cells around the airfoil and is below 5 in the entire domain. This guarantees the stability and robust convergence of the SIMPLE algorithm."*

Minor comments:

1. Attention should be paid to compound adjectives and their use throughout the entire text. For example lines 9,17: free-transition boundary layers, wind-turbine applications. Modifications should be done accordingly throughout the whole manuscript for similar occasions.
   Thank you. We have thoroughly checked the manuscript to the best of our ability.
2. Line 24: suffering is redundant here.
   Corrected.
3. Line 30: "which made scaling…in an unsteady environment, within which wind-turbine blades typically operate, challenging". This sentence needs rephrasing.
   Rephrased. The sentence now reads: "*These airfoils mostly featured relatively low thickness-to-chord ratios and narrow optimal operating windows. Such characteristics proved ill-suited to the highly unsteady conditions experienced by wind turbines and posed significant limitations on blade scaling."*
4. Line 32: Check reference as author/s is/are not mentioned.
   Thank you for pointing this out. This reference points to a public web page and as such the "Copernicus" style citation does not include the authors. This will be anyhow checked for full compliance with journal standards during copy editing if the paper is allowed to move forward.
5. Line 39: Bending, to the cube of.
   Thank you. Added: *"assumed to be proportional to the cube of the airfoil relative thickness"*
6. Line 41: Repetition of word development. Sentence should be rephrased.
   Thank you. Added: *"Development efforts were significantly accelerated by the introduction of panel methods, which have been studied since the early 1960s (Erickson, 1990)."*
7. Line 49: the word privileged is suggested to be replaced. (e.g., prioritized).
   Replaced.
8. Line 64: not available in open access.
   Replaced with publicly available.
9. Lines 66-67: DTU 10~MW, IEA 10~MW etc. A space should exist between the number and the SI units. Also between numbers and % a space should be added.
   Corrected.
10. Line 71: open-access should not have a hyphen (except if used as an adjective). Be more consistent on the use of hyphens throughout the text.
    Replaced throughout the text.
11. Line 74: the Reynolds numbers.
    Added.
12. Line 74: data is countable, therefore plural should be used. Please correct throughout the text.
    While technically the plural of "datum", the word is often used in a countable/uncountable manner depending on the context (https://en.wikipedia.org/wiki/Data_(word)). After reviewing the paper we left it as is. This is probably best addressed during the typesetting phase.

13. Lines 78-81: The sentence is too long. Please rephrase and split.
    Rephrased: *"As such, all modern aero-elastic tools are based on polar data and therefore accurate airfoil aerodynamic coefficients are essential to obtain reliable results. Because airfoil-based models are used in some capacity along the entire design chain of a wind turbine, accuracy impacts all phases of the process, from initial blade design to load verification and certification."*
14. Lines 92-95: Sentence should be split as it is currently too long.
    Changed.
15. Line 97: is approximately.
    Corrected.
16. Line 97: in the ones of -> for.
    Corrected.
17. Line 99: "(Timme,2020)" Please be careful with the citation commands. Use \cite or \citep where necessary and correct throughout the whole text. Which-> who.
    Corrected, thank you.
18. Line 107: being used is redundant and repetitive.
    Removed.
19. Line 108: near-stall region.
    Added hyphen.
20. Line 114: multiple airfoil families...between airfoil families. Repetition.
    Rephrased: "*Multiple airfoil families are included in the dataset, allowing for fair comparisons between them."*
21. Line 120: thickness-to-chord.
    Corrected.
22. Line 132: numbers can exceed. The Reynolds and ... numbers.
    The first phrase was moved slightly below.
23. Line 133-134: use punctuation in Equations and formulas, please correct through the text.
    Added a comma after all of the equations
24. Line 136: T is the temperature. Should be T, so the font matches with the font of the equation.
    Corrected.
25. Line 139: Section. Capital s.
    Corrected.
26. Line 140: "if" is redundant.
    Corrected.
27. Line 143: "is increasingly limiting", maybe consider replacing with "is becoming increasingly limiting".
    Corrected.
28. Lines142-145: Long sentence.
    Broken up into two sentences.
29. Line 145: URANS -> Explain abbreviation. Make sure you mention and explain abbreviations at the first instance of use. (Correct also line 169).
    Thank you, corrected.
30. Line 150: Parameters used.
    Thank you, corrected.
31. Line 164: consider replacing the word "insist" as it is not fully appropriate in this context.
32. Lines 170-173: Long sentence, please rephrase.
    Divided into two phrases.
33. Line 194: "extremely distant" needs to be rephrased.
    Replaced.
34. Line 198: The word "perusal" is not commonly used, maybe consider replacing it.
    We have reformulated this phrase based on one of the Reviewer's previous comments.
35. Line 205: Be careful with the use of capital letters in the captions. Capitals should be used only at the start of each sentence. Moreover the caption seems wrong as it depicts the maximum lift

to drag ratio error, the lift coefficient and lift to drag ratio. Finally, make sure to explain all variables mentioned in legends and labels. (such as variables D,C).

The reviewer is right. Corrected the Caption of Figure 5, 7 and 8.

36. Line 210: the word “used” is quite repetitive.

    Rephrased.

37. Line 209-210: Again the panels in the caption do not seem to align with the panels in the figures. Also, please remain consistent on how you use subcaptions (a),(b) etc. as they should come after the variable/panel they describe in the caption. For example: Lift coefficient (a), drag coefficient (b) etc.

    Caption was changed.

38. Table 4 title should be rephrased. Maybe “Number of total and chordwise elements” sound better. Articles are also missing. ‘The tested’ should be added.

    Rephrased.

39. Line 224: Caption in Figure 7 is incorrect. Beware this is lift and drag coefficients. Moreover, explain what is Ec. Citations is not correctly referenced in most captions. Finally, the markers and linestyles are not consistent between the legend and the plotted data. In panel 7(c), between 10-15 deg , there seem to be scattered points for the 1000 Ec and 1300 Ec cases that do not fall on the plotted lines. Please explain why this is the case.

    The Reviewer is right, the caption was corrected. The small differences that the Reviewer noted are due to correspondingly small differences in the airfoil coefficients near stall, which are amplified in the airfoil efficiency. As shown in comparison with experimental data, the observed change in lift and drag coefficients is quite small, and the predicted coefficients are very close for all mesh sizes above 1000 Ec (chordwise elements).

40. Figure 8: Correct caption based on previous comments. Explain the axes and variables correctly, as well as the legend and variables in them. Where is tf defined?

    The caption was changed and an equation was added to define tf (equation 4).

41. Line 230: While the lift..., the drag...5 to 10 timesteps per chord...

    Thank you. Corrected.

42. Line 232: Further decrease does not...

    Further decreasing the timestep from 10 to 20 and then 40 t/tf does not yield improvements, hence the use of plural. We have anyhow rephrased: “A further decrease in timestep does not ..”

43. Line 238: “shown in Figure (c,d)”. There is no panel d. Please use and refer to the correct Figures and subfigures.

    Thank you for pointing this out, the caption has been corrected.

44. Figure 9: Caption is wrong. Please include in the labels also the reference data.

    Thanks again for highlighting this. The reference data comes from an aggregated plot in the reference which is reported in the figure caption. The reference does not show the individual datasets with separate colors, they are rather all aggregated with the same color to highlight the spread between the various CFD predictions. Given that the reference is reported in the figure caption we have left the legend unchanged.

45. Line 253: in mind and were...Be consistent with the tenses.

    Corrected.

46. Line 253&256: “freestream” and “free-stream”. Be consistent on the terminology you use throughout the text.

    Replaced freestream with free-stream throughout the text.

47. Line 254: “However” is redundant.

    Removed.

48. Line 268: experimental and numerical results (ones is not needed).

    Accepted.

49. Line 272: Wrong equation referenced.

    Corrected with new equation numbering.

50. Line 273: lift-to-drag ratio is also mentioned as Lift to Drag ratio in other instances. Please double check and use one terminology in the manuscript. Moreover, check for missing articles (the lift-to-drag ratio at low angles).
    Thank you, corrected.
51. Line 273: for the computation of the entire...
    Thank you, corrected.
52. Figure 10: Correct caption. (a), (b) and (c) should go after the variables/panels they describe.
    Corrected, thank you.
53. Line 286: Simulations simulations. (Repetition)
    Corrected.
54. Line 288: blends an LES.
    Corrected.
55. Line 306: Ranges should be indicated with an en dash.
    Corrected throughout the text.
56. Line 307: “full domain totals”, please rephrase.
    Rephrased: *“The total number of elements is roughly $24*10^6$.”*
57. Line 313: The strong...
    We want to convey that the flow is strongly three dimensional and prefer strongly over strong.
58. Line 321: The moment coefficient...
    Corrected.
59. Figure 12: Correct references.
    Corrected.
60. Line 356: “The separation point is derived (by?)”...
    Rephrased: “In the BL model, flow separation is described by the static separation point (f) and center of pressure (x_CP). The separation point is derived from the static characteristics via the Kirchhoff formula in Eq. 8”.
61. Line 370: influences.
    Corrected.
62. Line 405: Wrong Figures are referenced.
    Thank you, corrected.
63. Figure 15: First, please indicate on the x-label the magnitude of the Re numbers. Moreover, once again rephrase the caption. Capital letters or lower case letters in variables are used randomly and subcaptions are not in the correct positions.
64. Line 428: Wrong figure reference.
    Thank you, corrected.
65. Line 436: There is no Figure 17(c).
    Corrected, thank you.
66. Figure 16: In the legend there are the dot markers for the experimental data but these are not plotted. Moreover, panels (d) and (h) fall out of bounds. Based on previous corrections, the caption needs to be again adjusted.
    We decided to remove the experimental data from this comparison at the last minute, which lead to the mistake. The caption has been corrected and the “EXP” nomenclature removed in the Nomenclature.
67. Line 445: Figure X? Correct reference.
    Thank you, corrected.
68. Figure 19: Please correct the caption, the variables explained are not correct. The x-axes for the Reynolds number should indicate the order of magnitude.
    Corrected. The Reynolds number in the x-axis of Figures 15, 16, 18 and 19 was corrected.
69. Line 477,478: bend—twist, shear—twist (use en dash as it shows equal relationship between words).
    Changed to en dash.
70. Line 478: increase broaden (please rephrase and use one of these verbs).
    Corrected.

71. Line 478: optimal.
    Corrected.
72. Figure 20: Please adjust based on previous comments.
    Corrected.
73. Line 541: Correct reference.
    Corrected.
74. Figure 11: This figure does not offer something in the discussion. Should it be used, please discuss in more details.
    Thank you for the suggestion. We prefer to leave this figure in the paper since it helps the reader visually grasp the SBES approach and understand the underlying physics. More experienced readers can also use it to visually and qualitatively judge the quality of the simulations. We have nevertheless expanded the references to this figure in the text.
75. Figure 18: This figure is a repetition of Figure 16. The experimental data could be plotted with the respective simulations of Fig. 16. Moreover, around 6 degrees and 9 degrees two y-values are reported. Please double check.
    Thank you for the comment. Since the experiments are performed in different conditions than those in the dataset we prefer to have a separate figure. The intent of Figure 18 is to show that the experiments show a similar trend in maximum CL/CD if broadly compared to the dataset. The trend of an approximately flat CL/CD as a function of the Reynolds number is consistent between simulations and experiments, with decreases at higher Reynolds number. We have revised the comment of Figure 18 as follows:
    *"The experimental results are compared in Fig. 18 to the same set of airfoils shown in Fig. 16. In the experiments performed on the DU-00-W-212 airfoils efficiency peaks at Re=9*10^6 (Fig. 18 (b)), while the angle of attack at which the maximum lift-to-drag ratio is located (Fig. 18 (a)) decreases as the Reynolds number increases before starting to increase at Re=1.5*10^7. The simulations are consistent with this trend, with some differences from airfoil to airfoil and depending on the inflow conditions. The Mach number is also much higher in the simulations, as it ranges between 0.01 and 0.46, while it does not exceed 0.1 in the experimental reference."*
76. Figure 10: It is not completely clear to which model and parametrization each linestyle is referring to in the legend.
    The Reviewer is correct; not enough information was given to interpret the figure. We have opted to simplify it and only show the adopted transition model compared to experimental data and to numerical data computed with the default correlation by Langtry and Menter.

Overall, the manuscript needs major changes and rewriting before considering for publication. If all comments as described above are addressed, I recommend that the manuscript should be accepted.
We wish to thank the Reviewer again for the very detailed and useful review of the manuscript. We are confident that we have successfully addressed the remarks and believe the manuscript has improved as a result.

Reviewer 2

This paper presents a very interesting and sound methodology for constructing a comprehensive database of aerodynamic characteristics for a variety of wind turbine airfoils covering the full angle-of-attack range from –180° to +180°. The authors generate high-fidelity results using CFD for the conventional operating angle of attack range—approximately between the negative and positive stall angles—and subsequently extend these results to the complete ±180° domain through an empirically based extrapolation procedure. The overall approach is robust, clearly explained, and results in an open database that is highly valuable for lifting line calculations of wind turbines which require such airfoil data. In addition to the remarks provided by my co-reviewer, I would like to offer a few further comments and questions.

1. Line 10: Beyond the stall region… This statement becomes clearer if you explain more explicit that the before mentioned CFD airfoil coefficients are generated for the conventional aoa range only, i.e. from negative to positive stall.
   Correct. We have now clarified this better in the abstract.
2. Line 26: Reference Bussel and W?
   Corrected, thank you.
3. Figure 1 This is certainly an intriguing figure, but it raised so many questions for me that it became more distracting than informative. It made me wonder whether including it is worth the effort—especially since its overall purpose and message are not entirely clear to me. Apart from the non-linear time axis, which looks unusual—and where, for example, the DU airfoils appear to be placed around 1991 even though some of them, such as the DU-00-212 used in the follow-up DNW-HDG test in your paper, were developed later—my main question is how the numerical values were determined. Several DU airfoils exist with 21% and 18% thickness, so which specific variants were used to generate these numbers?
   Thank you for this comment. Indeed, the intended scope of the figure could be improved. We intended this figure as a general overview of the trends designers have been following over the years. We acknowledge that this figure is not exhaustive, as efficiency and bending stiffness, although very important are not the only possible design objectives. Moreover, we chose to use publicly available data to generate the figure, derived with different methods in different conditions, in-line with the intended scope of evaluating general design trends and not compare families directly. Based on the reviewer's feedback we have improved the figure in the following ways:
   - Bending stiffness is proportional to sectional moment of inertia to differentiate airfoils with the same relative thickness
   - The relative thickness that was considered for the root and tip airfoils is reported in the figure
   - The time axis was generalized, emphasizing that some airfoil families in particular were developed over a range of years
   - The caption includes the specific airfoils we considered and the sources of the data used to generate the plots
4. Figure 1: Bending instead of bending
   Thank you, typo corrected.
5. Line 69: CENER instead of CNER
   Thank you, typo corrected.
6. Line 76: Between the lines, you refer to 3D rotational effects, which cause the aerodynamic behavior of a rotating blade to differ from the 2D airfoil characteristics as generated by you. Many readers may think: "It's useful to have these 2D airfoil data, but what is their relevance for actual wind turbine operation?" It would therefore be helpful to elaborate more explicitly on the value of 2D airfoil data. o I would start by identifying the sources of deviation between 2D airfoil performance and the performance on a rotating blade. Beyond 3D rotational effects, there are

also unsteady aerodynamic phenomena (your data are time-averaged) and 3D geometric influences, which may be particularly relevant for quantities such as $cd(90^\circ)$, as discussed below. o Nonetheless, as far as I know all methods which correct for 3D rotational effects, geometric effects, or unsteady airfoil behavior still rely on steady 2D airfoil data as their fundamental input. This means your dataset has substantial value (especially in the context of design approaches based on lifting-line models. It may be worthwhile to mention lifting-line models explicitly at an appropriate point in the manuscript).

Thank you for the comment. Based on this, we have added an additional paragraph describing how the coefficients can be applied to wind turbine simulations: *“As discussed in Section 2, this dataset is intended for use in wind turbine applications. Therefore, it must be noted that the coefficients in this dataset are relative to a non-rotating infinite-span uniform wing with. In other words, they are relative to the two-dimensional sections described in Section 2. Before being applied to a three-dimensional wind turbine blade they should be corrected three-dimensional rotational augmentation (Snel et al., 1992), and for finite aspect ratio, which affects the post-stall extrapolation and maximum drag coefficient (Viterna and Corrigan, 1982).”*

7. A question on Line 95 and further, on the maximum value of cd: Are we comparing apples with apples? I wonder whether perhaps some cd values are 2D (e.g. the values around 1.8) where others may be on a blade with finite aspect ratio which may lead to cd values around 1.3?

   The Reviewer is correct. The text was not structured in a clear way. We have indicated the reasons for which lower maximum drag coefficients are often used in wind turbine rotors, and the sources of uncertainty in the estimation process.

8. Line 104: you may be a bit sharper in motivating value of post stall data. It is not only for ultimate blade load estimation in parked conditions but these data may be of relevance for understanding stall and vortex induced vibrations as well. Even though these are unsteady effects I think that steady 2D data form the basis for modelling these effects.

   Thank you for the comment. We have added a remark as suggested in Section 1: “*Although horizontal-axis wind turbines do not typically operate in the post-stall region, these conditions can be encountered when parked, and can give rise to unsteady phenomena such as vortex induced vibration (VIV) which can drive ultimate loading. Even in the absence of unsteady loading on the blade, accurate sectional coefficients at high angles of attack can help reduce modelling errors in engineering tools.”*

9. Line 120: You mention that airfoils with t/c > 21% are included only, but the last one in table 1 has a thickness of 18%

   The Reviewer has a point. We initially included only geometries with t/c>21%. However, we later decided to also include the NACA-64(3)418 as it has been commonly used as a tip airfoil in many relatively recent utility-scale wind turbines (see https://doi.org/10.5194/wes-10-1929-2025). We have added this clarification to the manuscript: *The NACA63(3)418 airfoil is included despite featuring a relative thickness of 18% as it has been used in recent actual wind turbine blades (Braud et al., 2025).*

10. Line 147: You mention the effect of compressibility on the slope of the lift coefficient. Isn’t the drag rise equally important (e.g. use the Prandtl Glauert relation for this drag rise)

    The Reviewer is correct, we have added this clarification.

11. Table 3: The abstract says that simulations are done for conditions which are typical for utility scale wind turbines. Still I wonder whether tip speeds of 153.3 m/s are typical for modern wind turbines. This would lead to a significant drag increase from the Prandtl Glauert relation but also to significant erosion issues. It is more than fine to do simulations for a very wide range of conditions but could you explain the reasons why you have chosen these conditions?

    The Reviewer is right in noting that these conditions are highly unlikely. We have included the following clarification: *“As a consequence of the smaller chord of the tip airfoils, because the same range of Reynolds number was simulated for all relative thicknesses, a maximum velocity of 153.3 m/s is considered for airfoils with relative thickness of 21% and below. This velocity should be regarded as a conservative upper bound rather and is unlikely to be reached during steady-state wind turbine operation. However, its inclusion ensures that the dataset remains* *valid under transient scenarios, such as rotor overspeed events, extreme gusts, or rapid changes in operating conditions, which can temporarily lead to significantly increased local inflow velocities.”*

12. Section 3.3: I remember some issues in the AVATAR project wrt the y-Re theta model at the high Reynolds numbers test, which was the reason why many participants switched to the eN transition model or they modified the model. It seems you apply some modifications as well but it may be good to explain how your transition modelling relates to the transition modelling in these references: Ceyhan, O., Pires, O., Munduate, X., Sørensen, N., Schaffarczyk, A. P., Reichstein, T., Diakakis, K., Papadakis, G., Daniele, E., Schwarz, M., Lutz, T., & Prieto, R. (2017). Summary of the blind test campaign to predict the high Reynolds number performance of DU00-W-210 airfoil. In 35th AIAA Wind Energy Symposium (AIAA 2017-0915), Grapevine, TX, USA. Colonia, S., Leble, V., Steijl, R., & Barakos, G. (2017). Assessment and calibration of the γ-equation transition model for a wide range of Reynolds numbers at low Mach. AIAA Journal, 55(4), 1126–1139. https://doi.org/10.2514/1.J055403

    Indeed, we are aware of the shortcomings of the gamma-Re-theta model when it comes to natural transition at very low turbulence intensities and high Reynolds numbers. We have chosen to use this model anyway due to the fact that it can be customized within Ansys Fluent. We agree that this aspect could be better highlighted. We have therefore included the following statement in section 3.3: *When simulating cases with low inflow turbulence some authors have noted some deficiencies in the γ-Re_θ model (Diakakis et al., 2019). However, other studies have also shown that correlation with experiments can be significantly improved in correlation-based transition model though calibration (Colonia et al., 2016, 2017).*

13. Line 254: I agree that modern wind-turbine airfoils—as a matter of fact earlier generations of wind-turbine airfoils as well—experience effectively lower turbulence levels because the rotational component contributes to the resultant velocity in the denominator of the turbulence intensity. However, in my view, the turbulence intensity scales approximately as ~ Iambient/λr For an ambient turbulence level of say 10% and a local tip speed ratio of say 5 the resulting turbulence intensity would be 2%, much lower than 10% indeed but still twice the threshold value of 1% mentioned by you.

    The Reviewer’s remarks are very relevant. Indeed, given the fact that inflow turbulence scales with relative velocity, the level encountered by the airfoils depends on many factors, such as their radial position, the operating rotational speed and the inflow turbulence. The value that we indicated is a threshold above which bypass transition may start to play a role, depending on the fluid-dynamics problem at hand. Boundary layer transition mechanisms are discussed in detail in Lobo et al. (2023), where natural transition is identified as the predominant mechanisms causing boundary layer transition in the absence of external flow disturbances such as the wake of another turbine.

    Furthermore, it can be argued that inflow variations with a large enough length scale, which occur slow enough, could be treated as variations in mean inflow from the airfoil perspective, rather than inflow turbulence.

    For all the reasons above, the current dataset includes a set of airfoil coefficients computed with very low inflow turbulence of 0.35% and coefficients computed in fully turbulent conditions. The users can decide to interpolate between the two sets based on the specific needs at hand. We have added this discussion to section 2: “*In addition, this value is in line with the state-of-the-art. For instance, it is not uncommon to see aerodynamic coefficients used in blade design exercise being computed with N_{crit} values in the range of 7 to 9 (Zahle et al., 2024), which correspond to low levels of turbulence intensity, as shown in Eq. 6 discussed later on in this study.*

    It must be noted that the inflow turbulence wind turbine airfoils experience during operation varies widely due to many factors. Depending on the turbulence level in the incoming wind field, the actual turbulence on the blade section scales based on the relative velocity, which depends on the local tip speed ratio of each section. Especially in the case of outboard blade sections, this can lower the turbulence intensity level significantly. The many factors at play make *choosing a single value challenging. However, providing users of the dataset with coefficients computed with low inflow turbulence and a fully turbulent boundary layer, allows for interpolation between the two, in order to fine-tune the coefficients to the application and the level of desired conservativeness, if used for blade design applications."*

14. Figure 12: Good to compare with SBES simulations. A question: Have you considered a comparison with measurements? I am not sure if there are measurements for -180+180 at 10M but could a comparison at lower Reynolds have value too? E.g DNW-HDG measurements from -90 to +90 degrees at RE = 6 M or others?

    Thank you for pointing this out. The motivation for using SBES as a reference is that we were unable to find high angle of attack tests on the FFA-W3 airfoil family. In addition to the maximum drag coefficient we are also interested in the shape of the lift and drag curves to choose / calibrate a post-stall extrapolation model. We have included the following statement in Section 3.4: *"While a direct comparison to high angle of attack experimental data was not directly performed in this study, the maximum drag predicted by the SBES simulation is in line with the existing literature, towards which the reader is redirected for more in-depth discussions on the topic (Timmer, 2010, 2020)."*
15. Line 358: derived from THE static characteristics?

    Corrected, thank you.
16. I suggest adding a brief description of the AVATAR experiment at an appropriate point in the text to provide readers a bit of context. For example, you could mention that the campaign involved detailed pressure-distribution measurements, supplemented by drag data obtained from a wake rake in the pressurized DNW-HDG wind tunnel. Despite the model having a chord length of only 15 cm, the pressurization enabled testing at a variety of Reynolds numbers up to 15 million where it is noted that the tunnel turbulence was different (or something like that)

    Thank you for the suggestion. We have decided to insert such a paragraph at the end of Sect. 3: *"Experimental and numerical data generated during the AVATAR project (Schepers et al., 2018) serve as the benchmark for model validation and calibration in this study. During the project, experiments were conducted on the DU-00-W-212 airfoil at Reynolds numbers ranging from 3*106 to 15*106. Such high-Reynolds number data is exceptionally rare and was made possible by the DNW High Pressure Wind Tunnel (HDG) in Göttingen. By pressurizing the tunnel to increase air density, flight-scale Reynolds numbers were achieved on a model with a chord of only 15 cm. The resulting dataset includes aerodynamic coefficients from positive to negative stall, alongside detailed surface pressure distributions. Furthermore, these benchmarks were used by project members to cross-verify various computational tools (Sorensen et al., 2016). The following sections detail the numerical framework: Section 3.1 discusses grid and domain sensitivity; Section 3.2 provides a verification of the fully-turbulent setup against established CFD results; Section 3.3 explores boundary layer transition modeling sensitivity; and Section 3.4 addresses the methodologies for post-stall extrapolation to high angles of attack."*
17. In the abstract, you mention that differences with other open-access datasets are discussed. To me, that discussion feels somewhat limited. You might consider referring to the Aerodynamic Table Generator developed at TNO (formerly ECN): Bot, E. T. G. Aerodynamic Table Generator. A Manual. ECN-C-01-130, December 2001 (in Dutch unfortunately). Aerodynamische Tabel Generator This tool produces aerodynamic characteristics for a wide range of airfoils based on wind-tunnel measurements for the conventional angle-of-attack range and then extrapolates to −180° to +180° using an empirical method. In that sense, its approach is quite similar to yours. The key difference, of course, is that it relies directly on measurements—an advantage in terms of realism, but also a limitation because such measurements exist for only a restricted set of conditions, resulting in far less flexibility than your method.

    The Reviewer is correct in mentioning that comparisons to other existing datasets were limited and not explicit enough. While in the introduction we had provided references for experimental datasets and for the coefficients used in reference wind turbines, we had not explicitly compared to this dataset. We believe the main strength of this dataset, in addition to the inclusion of compressibility effects via high-fidelity simulations, is the fact that multiple families can be compared/chosen as they are computed in identical conditions. **We have, however, more explicitly discussed the differences between the current and other datasets in the introduction, and in the conclusions.**

Good luck withing finalizing this excellent work!