Manuscript ID: WES-2021-155

Optimisation of a multi-element airfoil for application to airborne wind energy

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This work presents an aerodynamic analysis of a multi-element airfoil.

The topic is of definite interest to the scientific community, is well written, and all the points highlighted by the associate editor in his first review have been addressed and implemented by the authors in the current version of the paper.

However in my opinion some changes are needed before the final publication.

General Comments

1. The first point I would like to highlight is related to the optimization process. Actually what is presented in Chapter 3 is a (interesting) parametric analysis, where the authors show the effect of different parameters on the aerodynamic coefficient E^2C_L. For each of these (or pair of these) parameters, a series of simulations is then made from which the effect on the aerodynamic coefficients is extracted. In these analyses the point of "optimum" is then identified and compared with the baseline (which, I guess is the original industrial configuration). Section 3.3 then combines these individual best conditions to define an optimal geometry on which the wake analysis is then done. This process then does not actually identify the true absolute optimum nor can it be called an optimization process (which instead involves a complete/complex mathematical optimization procedure, in this case constrained, leading to the identification of the optimal parameters and eventually the active constraints). Furthermore, the indicated procedure starts by doing the analyses at an AoA of 17deg, equal to the baseline optimum, which, in general, may not be the final optimum angle of the optimal configuration. However, I think the work is scientifically relevant, so my proposal is to change the title of the paper ("Parameter analysis" instead of "Optimization"). The name optimization in fact induces in the reader an expectation about a mathematical model that is not present in the paper. Moreover, this point and the limitations of this procedure should be emphasized in section 3.3. Line 259/260 are then to be revised since the increase on the aerodynamic coefficient E^2C_L of 50% should be better discussed.
2. Another point is the application of this parametric analysis. The title again refers to AWE system, but, except in section 1 where an extensive analysis of the state of the art of these systems is made, the rest of the paper shows several figures with a 2D model with 4 airfoils. The application to AWE systems should therefore also be strengthened a bit more in the other sections. It is clear that the choice of considering E^2C_L as an aerodynamic parameter (and not E) is already, by itself, something that looks at the world of AWE systems, but probably a figure showing the real system, even a rendering, would help the reader to get immersed in this framework. From this point of view, the often-mentioned construction constraints perhaps should be better defined. In summary, it would be helpful to present in the paper a picture of the baseline system and how this multi-profile is actually realized.

Comments

- Lines 74-75. For a wind turbine the aerodynamic parameters to be analyzed is the lift-to-drag ratio C_L/C_D. So C_L/C_{Dmax} refers to the best ratio (i.e. best aerodynamic efficiency). So please check the sentence: "[…] where C_{Dmax} is given by the maximum value of drag coefficient[…]".
- Lines 110-120. The final mesh should be better described: 1) the chord length of 1m should be written before (not at line 122), 2) the size of the total domain (figure 2a) should be defined, 3) the cell size of 20mm is the size in which refinement zone? 4) is there some quantitative analysis on the quality of the final mesh?
- Figure 4. The comparison with the literature does not allow a realistic validation of the tool used: the difference in the lift coefficients could be explained by an offset in the AoA, as written, but that of C_D, if due to pressure drag, clearly has little meaning. Is there no possibility of adding a further comparison with other data?
- Figs. 6-10. The figures with the pressure coefficients could be improved in their readability if different symbols are used for suction side and pressure side. In so doing, the discussion in the text when referring to the separation in the pressure-side (i.e. line 158) or suction-side (line 169) is easier to be seen in the pictures.
- Fig 6. I'm not sure about the representation of the circular path behind the main/Strut airfoils: I, more realistically, expect a separation zone. I think that here the authors should try to discuss a little bit more this also by commenting on the limits of the 2D steady-state model and/or the effect of the Reynolds.
- Section 3. Already commented in the "general comments"
- Fig 12c. The effect of the Front Flap Scaling seems symmetrical: the optimum value at 130% is very similar to the one at 70%. Why then the choice of this point? What would it change in terms of manufacturing constraints to move in the other direction?
- Figure 18. These are a very interesting analyses. But needs more info. 1) the "y-position on line" (i.e. y-axis) is measured from where? Is the y-origin at the same height of the Strut TE? Maybe a picture with the airfoils may help. 2) Ux in the x-axies has no unit. 3) it's not clear the far field flow velocity value U_inf. Maybe you can plot directly the non-dimensional value Ux/U_inf

Minor Comments

- Line 74. "[…]for this studies. the optimization[…]". Check the sentence.
- Line 114. "in a y-plus value of 30 with […]". This is not very clear.
- Lines 121-124. This sentence is too long. Please think of revising it by inserting a full stop for example after "1m".
- Fig 13 and 16. Quadratic interpolation in these figures in some areas has strange trends. Maybe using a "shape-preserving interpolation" method is better [see, for instance fig 16c between -100 and zero or between 100 and 200mm]