RESPONSES TO REVIEWER #2

General comments

Not being familiar with the ALM subject I thought I a very difficult review. I am happy to report that despite the novelty of the subject (for me), the paper allowed me to understand a great deal of the overall subject and the authors interest.

Overall, the paper is well written with useful plots illustrating the points the authors wanted to make. Despite some minor syntax errors mostly attributed to the paper's length the text can be read easily. Being unexperienced in the topic, having sections dedicated to the state of the art and explanations about the theory was very useful, although it may be redundant for more experienced readers. Having such sections in appendices could have been another option.
The Authors would like to thank the Reviewer for his appreciation of our study and tried their best to further improve it based on the comments received. Regarding the theoretical bases, the authors would be inclined to leave the corresponding sections within the text since they are thought to be instrumental for the understanding of the numerical approached proposed herein.

The comments and questions below are for my clarification. I believe that the paper should be **accepted** after the **minor comments** in the questions below are answered.

Technical comments

1. I understand the need of a well-known and simple airfoil such as NACA 0018, despite no longer being used in wind turbine designs. However, the Reynolds number is too low to be representative of modern wind turbine, some viscous effect may still be seen thereby possibly questioning the outcome. Such study would have been preferential above Re = 2-3 10^6. Did you choose 500k in order to save some computing time?
2. The turbulent intensity seems quite high compared to wind tunnel experiment or even some wind farm sites following the IEC classification. Why did you choose 1 % and not a lower value (which could have helped your CFD simulation)?

Thank you for the comments. An intermediate Reynolds number of 500e3 has been selected as an optimal compromise between computational cost and achievable accuracy. In our experience, although viscous effects are still relevant, RANS solvers perform reasonably well in such operating conditions. The main drawback is the need of a turbulence model accounting for laminar-to-turbulent transition – in this case, the k-ω SST with intermittency transport – that also requires higher levels of inlet turbulence intensity to work in a stable way. This is the reason for the selected value of 1% for the TI. We have clarified these choices in the revised manuscript.

3. The CFD setup followed a previous work performed by the authors, the RANS approach tends to smooth out any flow unsteadiness and presents only averaged results. When looking at the unsteady phenomena of tip vortices, another approach would have been more pertinent such as LES or (i)DDES even URANS. Did you perform sensitivity studies on the CFD baseline as it could impact the rest of the study (such as the AoA recognition)?
4. Similarly, the vortex detection may not be fully accurate using a RANS approach rather than a time varying one.
5. Figure 7 and Figure 8 are very clear highlighting the three-dimensionality effect introduced by the finite wing. Because of the RANS solver 3D cases may not be truly those plotted, or at least the error bar due to the unsteady nature cannot be seen.

Reviewer's comments are pertinent. The set-up of the numerical simulations has been dictated by the main scope of the paper, which is not an exact reproduction of experimental measurements, but rather the investigation of the main mechanisms behind tip losses effects using a high-fidelity method (BR-CFD) and of the ALM capability of reproducing such effects at parity of other modeling choices. In this perspective, flow features that would not be captured anyway by the ALM formulation, mainly unsteadiness in the 3D flow field across the wing and in the tip vortex shedding, have been removed on purpose.
The reliability of the selected post-processing techniques on the hand has been verified in previous works by the authors, at least for the quantities of interest for the present investigation:

- *Tip vortex tracking:* the vortex identification methods used in this work has been validated against unsteady measurement and numerical simulations of a wind turbine in a previous work of one of the authors (https://doi.org/10.5194/wes-8-1659-2023). This study has also shown that a RANS methodology can correctly predict the average metrics of the shed tip vortices, despite their inherently non-stationary nature, with limited differences from an LES approach. For this reason, the inaccuracies introduced by performing a RANS simulation rather than LES can be considered limited in terms of tip vortex convection velocity, core radius and strength;
- *LineAverage:* this methodology has been applied by its creators (https://doi.org/10.1002/we.2196) and by the authors (https://doi.org/10.1016/j.enconman.2020.113284) in previous works on high-fidelity simulations of both horizontal- and vertical-axis wind turbines, showing accurate results in both steady and unsteady conditions.

6. Very impressive 2D CFD results matching the experimental CL and CD!
7. Did you average the whole wing to produce the CL and CD curve for the 3D BR-CFD (fig 4)? It could have been interesting to plot some of the different positions along the span as used further down the paper to illustrate the spread in results.

The 3D BR-CFD data was computed upon integration of the local pressure over the wing surface. The text has been updated to clarify this aspect. Furthermore, following your suggestion, the corresponding figure has been updated with the lift and drag curves computed at different spanwise positions along the wing.

8. To complete those interesting figures (Figure 7 and Figure 8), it may be useful to add onto the wing shape the 3D AoA contour (if feasible)? It could show interesting pattern regarding its spanwise evolution.
9. The evolution of the radial flow component could also be plotted onto the wing surface since its behaviour is monitored in the next sections.

Thank you for the interesting suggestion. We believe that the information in terms of flow deformation induced by the tip vortex can be presented by 3D streamlines as effectively as the suggested plotting variables (3D AoA, radial flow component). Figures 7 and 8 have been updated accordingly.

10. Did the 3D AoA calculations account for the radial flow in the Cp calculation? The equation 9 mention the local velocity, is the possible "flow deviation" due to the tip accounted for?

Thank you for noting. The *LineAverage* method only accounts for the velocity components in the same plane of the sampling line, complying with the conventional definition of the angle of attack. This aspect has been clarified in the text. Based on the method's formulation, it is then supposed to account for any flow deviations locally induced by the tip vortex.

11. The sentence in Line 389-394 (However, the magnitude…) is very important and should be split into several sentences to avoid losing the reader.

Agreed. The corresponding sentence has been rewritten.

12. The conclusion is clear and well written summarizing the outcome and future work. Will the bullet point 2 solve/address the issue in bullet point 3?

This is an interesting question. It is not clear from the study whether the gap in terms of tip vortex structure between ALM and BR-CFD can be filled by improving the ALM formulation or it is related to the intrinsic differences between the two methods. Our opinion is that, even it was possible, the corresponding mesh requirements would be so high to make the ALM not convenient anymore. The conclusions of the paper have been updated to highlight this aspect.