Comments to the paper no. **wes-2023-88**

An insight into the capability of the Actuator Line Method to resolve tip vortices

The authors present a very interesting, detailed and well designed numerical analysis to investigate the origin of ALM limitations (if any) in describing blade tip angle of attack and loads behavior. To this aim a very simple straight 3D wing geometry has been selected and many different analyses dealing with AOA, loads, velocity field, tip vortex structure are suitably addressed.

Moreover, a very important effort is done by the authors to derive some general guidelines on the use of ALM-based solvers on the basis of their numerical evidences.

The paper is well written, although there are some minor technical corrections that I have proposed in the following. I also appreciate the writing style which, except for a fee parts (see comments below), suitably follows the investigations conducted keeping the reader always focused.

Generally, in the paper both present and past tenses are used when referring to literature results and also presented results: please make a coherent choice. I strongly suggest to use always the present tense.

As a general comment I found the paper a bit long. I understand that the investigation is wide and it requires suitable comments and descriptions. Nevertheless, I encourage the authors to try to make the paper more concise also trying to avoid some repeated or too much “educational” sentences. Some examples are reported in the following.

My indication is to **ACCEPT** the paper only after **MINOR REVISIONS** following the comments listed below.

SPECIFIC COMMENTS

<u>Abstract</u>

l. 10: the paper is focused on the ability of the ALM to describe blade tip vortex flow features. Which other vortex-like structures do the authors refer to in the abstract? The same sentence is repeated in the Introduction (l. 36). Please rephrase these sentences to clarify.

<u>Introduction</u>

1. The introduction is well written and correctly reports the investigations on the limitations and capabilities of ALM solvers to describe wind turbine (WT) blades aerodynamic behaviour. Lines 24-35 outline an overview of the existing methods for WT rotor aerodynamics modelling. Within this general summary, which span from engineering-type BEMT formulations up to CFD, mid-fidelity vortex methods should be mentioned. Indeed, this family of 3D solvers is well-known to be able to capture (at a very reduced computational cost) rotor blade aeroloads over a wide range of operating conditions and it has been demonstrated that they provide an accurate prediction of blade wake shape close to the rotor disk. The latter aspect is particularly related to the topic discussed in the paper and, for instance, in a future work it would be very interesting to compare the ALM strategy proposed by the authors to the outcomes of such models. Some examples of effectiveness of the mentioned methods are reported in the following papers and I suggest to mention them for completeness as alternative solvers able to

describe tip vortices features. The second work, in particular, includes specific computations of wind turbine blade wake shape and velocity field downstream.
Boorsma, K., et al, “Progress in the validation of rotor aerodynamic codes using field data”, Wind Energy Science, 2023, 8(2), pp. 211–230.
Greco, L., Testa, C., “Wind turbine unsteady aerodynamics and performance by a free-wake panel method”, Renewable Energy, 2021, 164, pp. 444-459.

2. l. 25: replace “framework” with “domain”.
3. l. 27: “required for their aeroelastic…”
4. l. 46: “.. of the ALM element size h_ALM < 0.25beta…”: the symbol h_ALM is not defined in the text and it is not clear, at this point, what does “ALM element” refer to and which size the authors are considering. Please rephrase the sentence to better clarify.
5. Section 1.2: the standard ALM is well-known to be an iterative method. Presumably also the Frozen-ALM requires an iteration, but this is not explicitly mentioned. Moreover, the “frozen” definition does not exactly suggest that there is an iteration. I suggest the authors to better clarify this aspect.

Section 2

The description of the test case must be placed within the numerical results section and not before the description of the numerical methods.
On lines 94-95 the authors state that the Reynolds number was selected to obtain a linear behaviour of the airfoil. It is not clear which type of linearity the authors refer to. Is it referred to the airfoil lift? Please clarify.

Section 3

1. In the description of the ALM the AOA calculation step is not mentioned.
2. Line 104: replace “rotor” with “wing”.
3. Line 105: replace “...then, based on the sampled flow field…” with “...then, based on this…”
4. Line 106: replace “projected” with “exerted as sources of momentum”
5. Line 112: it has been already mentioned that the ALM relies on tabulated airfoil polar data.
6. Line 123: replace “at which” with “where”.
7. Eq. 2: symbols rc and rt are not defined in the text.
8. Line 158: “A uniform cartesian grid…”: please clarify if this refers to the spanwise direction or to a different one.
9. Line 159: the symbol h_ALM is not defined in the text.
10. Line 161: replace “The anisotropic and Gauss….” with “Differently, the anisotropic….”
11. Line 163: replace “As this process took place…” with “As this process does not consider….”.
12. Table 3: the columns of ALM-iso and ALM-GG show only one value, it is not clear why.

Section 5

1. Line 239: the vortex Aspect Ratio is not defined in the text. Please revise.
2. Section 5.2: Equation 9 is not clear to me. Does the vector sj indicate the tangent-to-the-line unit vector? If so, then symbol |sj| denotes its magnitude, hence the dot product with the velocity vector is not needed. Moreover, on line 258 only the trailing vorticity is mentioned whilst the proposed method can, within a potential flow assumption, take into account both trailed and shed vorticity. Please revise this part.

Section 6

1. Line 273: the Frozen ALM has been previously defined, no need to repeat here.

2. Lines 287 – 290: the authors state that for the BR-CFD computation it is not possible to use the LineAverage method to sample the velocity field due to possible intersection of the sampling line with the airfoil. Indeed, if the sampling circle is centered at c/4 it would be possible to sample the velocity on a circle with r greater than ¾ c. This type of velocity sampling would be more consistent with respect to some of the proposed ALM computations and would eliminate the need of an additional 2D CFD computation.
3. Line 305: "...section lift computed at alpha …"
4. Lines 306-308: "This effect is also….SS suction peak". This sentence is not clear. The reduction of the SS peak is evident from Fig. 7 but it is not clear to which shift of the stagnation point the authors refer to. Please clarify.
5. Line 310: "… 2D BR-CFD towards the trailing edge"
6. Line 313: "… and 8d-e (bottom)"
7. Line 315: "… the corresponding blade section takes the name …"
8. Line 320: "Differently, in the region between 97% of the span and the tip ~~instead~~ the flow…"
9. Line 322: "… in Fig.9 (right)".
10. Line 337: "… Section 6.1 demonstrates …"
11. Line 338-339: Please remove "along the blade". It is already clear that the progressive reduction of AOA_eq refer to the spanwise direction.
12. Line 341-342: Please remove "if the answer is positive".
13. Section 6.2.1: From my understanding, the AOA shown in Fig. 10 and computed from the BR-CFD simulation is the outcome of the 2D-equivalence (so it is alpha_2D_eq). Is it correct? In this case, it is not surprising that this AOA (coming from an equivalence which is not providing accurate results at the tip) is not consistent with the loads from BR-CFD shown in Fig. 11. From this point of view (and also considering the conclusions of the paper) I think it is worthy computing the AOA from BR-CFD using the LineAve methodology to better clarify these aspects. Moreover, in Fig. 11 the results of the F-ALM are not shown (or are they coincident with the standard ALM?).
14. Section 6.2.2: **Lines 408-410** can be shortened for the sake of brevity. In lines 411-413 the authors comment that the BR-CFD and F-ALM predicted velocity fields are similar. In my understanding the mentioned symmetric behaviour refers to the chordwise direction but it is never indicated in the text. Moreover, the predicted fields do not show the rotation of the velocity field about the z axis (induced by the presence of the airfoil), which should be a well known limitation of ALM approaches. Finally, the BR-CFD downwash velocity shows an asymmetric chordwise behaviour already at the mid-span section. The authors should comment more in deep these different field features and link them to the actual limitations of the ALM approach. **Line 417:** please remove "...commented in the previous paragraph" and add the reference to the actual section. **Lines 418-420**: these lines refer to evidences of Fig.17 that, at this point of the paper, has not been mentioned yet. I suggest to include these comments where Fig 17 is commented or, alternatively, move Fig 17 in this section.
15. Line 441: "...airfoil aerodynamics…"
16. Line 442: "… conditions. As shown in Section 6.2.1, in the rest of the span ($0.5<z/2H<1$) ~~instead~~ …". Please remove "This aspect has already been discussed in Section 6.2.1".
17. Line 446: It is not clear what the author refer to by "height".
18. Section 6.2.4 is directly linked to Section 6.2.2. Thus I suggest to postpone Section 6.2.3 and place those two sections one after each other or even merge them.
19. Line 474: the authors mention the "shed vorticity" which is zero in a steady flow. Please clarify.
20. Figure 17: It is quite hard to distinguish the different results shown in the figure.
21. Line 500: I don't understand the sentence "lack of intersection between the BR-CFD…..". Please rephrase it for better clarity.

22. Line 512: “Only isotropic kernel is considered”. It has been already pointed out, please remove.
23. Line 533: Please remove “superficial glance”.
24. Lines 535 – 539: these part is not clear. Please rephrase it to better clarify the interpretation of the presented results.
25. After their in-depth analysis the authors come to the conclusion that the standard ALM, if properly tuned (e.g. appropriately selecting the value of rs) can provide accurate predictions of the AOA of the tested wing. It would be very interesting to see the spanwise loads estimate by using the rs value providing the best match of the AOA with respect to the BR-CFD outcomes. I suggest to add to Fig.19 a similar one regarding blade loads.