Dear reviewers

Thank you so much for your time and effort in reviewing our paper. Please find our answers to your comments below (in blue). In the coming days we will submit the revised manuscript together with a pdf highlighting the differences made to the text.

Emmanuel and co-authors

Reviewer 1

Generally, a good and well-structured paper. A few typographical errors which can be quickly corrected. A few statements which are somewhat questionable, which however can probably be addressed with a suitable reference. The results shown do not demonstrate any new physical findings, but rather validate the solver. The addition of a general lifting line solver to OpenFAST is welcome and will help contribute to the uptake of vortex methods.

Specific comments:

- It would be constructive to describe how this regularisation is being applied to remove the singularity. There are a number of ways of applying this...

> We have added the following comment to clarify how the regularization is applied: "We used a Lamb-Oseen regularization kernel as a multiplicative factor to remove the singularity, the regularization parameter is the same for the wing and the wake, and constant throughout the wake."

- The mid-span loading behaviour for the shear case appears to have almost "reversed" behaviour. Is this what is meant by "challenging towards the root"?

> The following was added: "The shear-only case appears to be challenging, especially at 33% and 48% span, where the behavior captured by the codes are opposite to what is observed in the measurements."

- Is this possibly an erroneous reference? The indicated paper does not describe the DMST model used in any way and in fact states that the model produces inconsistent results.

> This is the correct reference, but our sentence was most likely misleading. We have corrected the text to mention that we have extracted the DMST results from the article, but the DMST is not described in the paper. As you mention, the paper highlights some discrepancies between the vortex code results and the DMST, which our study confirms. We will mention this in the revised version.

One sees that the tower effect is being taken into account. Is this being done with the standard cylinder model of AeroDyn? Worth describing.

> Thank you for your comment, we have added some precision about this in the revised version.

Is this an open question? For capturing accurately unsteady induction effects, shedding at the trailing edge (or in fact slightly displaced from the trailing edge as described in Katz & Plotkin) appears to produce the best results. Perhaps include a reference which describes where this question is discussed.

> Thank you for pointing this out. I also thought that shedding at the trailing edge (or 1/4 chord after) was the preferred approach, but in this study, by comparing with CACTUS results, the performance of CACTUS was slightly better at times despite only shedding the vorticity at the lifting line. This was offsetting to me, which is why I mentioned this as an open question. I've yet incorporated your comment into the revised version.

Is it described somewhere how this is calculated/accounted for (eg. a report of reference somewhere). It appears that the losses due to excrescences play quite a role in accurate power prediction.

> I've added some details on the experimental calculation of the excrescence torque in the revised version.

Reviewer 2

Interesting paper showing a very useful update to the OpenFAST software suite.

General comments:

-Although this is a great addition to the public available OpenFAST software, it would be great to make the novelty of the research in the paper stand out better. The methods presented are mostly already used in wind energy and there could be MORE EMPHASIS ON NEW SCIENTIFIC FINDINGS obtained using this tool. It is recommended to FOCUS MORE ON THE NOVELTY aspect to make a better contribution to wind energy sciences.

-In the specific cases many open ends are described which should be topic of future research. Although very difficult, it would be helpful if the paper can be MORE CONCLUSIVE ON SOME SUB-TOPICS TO PROGRESS beyond the current state of the art.

> Thank you for your comment, we fully agree that simply presenting the features and results of a code does not constitute significant scientific findings. We have tried to provide qualitative and quantitative analyses for various applications, using additional computational tools and measurement data, to verify and validate the tool. The verification and validation is relevant since our vortex tool has received very little verification and validation since its recent application. As you mentioned, the methods are already well established. We have tried to touch upon areas of improvements, but we agree that more research could be done on the topics we highlighted. Following your comment, we have tried to better emphasize

our scientific observations throughout the text and in the conclusion, highlight the areas needing research, and extend on the path forward to tackle these challenges in hope of address them in the future. Each of the application we present are succinct and could be extended in a more thorough study, which we hope to do in the future if we are awarded follow-up projects. Presenting several brief applications seemed relevant for this tool which is intended to be multi-purpose. We will submit a version addressing your comment. Below are some examples of sentences that were added to the text.

> "We observed a strong dependence of the flow-quantities on the lifting-line with respect to the regularization parameter. We expect that the regularization parameter should be characteristic of the physical size of the bound vorticity to obtain a realistic simulation of a wing or a turbine blade. This physical size is mostly likely be related to the size of the boundary layer, which is likely proportional to the chord. As we have observed, results will also be a function of the spanwise discretization. Vortex methods require the size of the regularization parameter to be proportional to the grid size for the method to converge to the Euler or Navier-Stokes equation. Therefore, physical and numerical regularizations operate differently, and we expect that a reformulation of the lifting-line algorithm itself is necessary to ensure convergence of the method. The topic of regularization is being actively researched for actuator line CFD vortex-based method. Future work should focus on the convergence of the lifting-line method with blade discretization, through comparisons with measurements and blade resolved simulations."
>
> "The discrepancies between BEM and OLAF observed in the yaw case seem to indicate that the implementation of the yaw model in AeroDyn may need further improvements."
>
> " In this section, we have presented examples of simulations of 2D and 3D VAWT, verified them using other simulation tools, and validated them against measurements.
>
> By diving into the implementation details of CACTUS, we have found some differences of formulation, which can explain the differences observed between the two simulation codes. Some of the differences between OLAF and CACTUS include: the presence (or absence) of a ``trailing-edge'' vortex, the location of the control points (on the nodes or in-between them) and the location of the points used for the determination of the angle of attack (CACTUS uses points at the 1/4, 1/2 and 3/4 chord for the BV model). Additional features were implemented in OLAF and it is now possible to switch between these formulations. Additional work is needed to determine which formulation is the most accurate.
>
> The current approach for VAWT modelers consists in tuning the dynamic stall parameters to obtain performances that matches the measured ones. We have applied this approach in this work to illustrate that the method can indeed be used successfully. Nevertheless, the approach cannot be considered satisfactory and the large spread of results that we obtained in Figure X for different dynamic stall models indicates that more research is needed on the topic. In particular, future work should focus on deep stall and large fluctuations of angle of attack, which are relevant for VAWT.
>
> We found that, when the turbine passes its own wake, the simulated loads were in noticeable discrepancies with the field measurements. The reasons for such differences are currently not

well understood. They may be related to regularization issues and potentially the lack of vorticity shedding when the blade is stalling. It is also possible that the blade-vortex interaction is not well captured by the lifting-line vortex method. Flow field measurements focusing on the wake and its interaction with the blade may help answer this question.

Specific comments

-The 'we'-form is widely used throughout the paper which to the reviewer's opinion should be prevented.

> I personally agree, but our communication department is asking us to use the "we" form instead of the passive voice.

-Section 3.2

Would it be interesting to showcase the effect of regularization parameters for a wind turbine application rather than for an elliptical wing?

> Thank you for your recommendation, we have added a case for a horizontal axis illustrating a more realistic case.

-Section 3.3

Can the authors comment on the discrepancy between OLAF and BEM observed in Figure 4? Should these models give identical results for a constant and uniform wind case or not?

> Thank you for your suggestion, the errors might be slightly higher than usually expected, which is likely due to the fact that this simulation is at a higher loading. We have added the following to the text:

"Despite the simple flow situation, differences are observed between the methods, which can be attributed to fundamental differences in the formulations: BEM assumes the blade annuli to be independent, does not inherently account for out-of-plane effects such as prebend, and relies on empirical corrections. In particular, this simulation has an average induction factor of 0.4, corresponding to a high loading case where a high-thrust correction is needed in BEM. Vortex methods are of higher level of fidelity, but they suffer from the issue of regularization touched upon earlier, and the introduction of more scales as the discretization is refined. The mean relative errors in axial inductions and angle of attack are of 4\% between the two methods. The mean relative error of the tangential induction is around 20\% and the error in normal and tangential forces is 3% and 6% respectively. The differences between BEM and vortex methods are in line with results from other participants. "