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Comment on wes-2021-130

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Referee comment on "Comparison of Free Vortex Wake and BEM Results Against Large Eddy Simulations Results for Highly Flexible Turbines Under Challenging Inflow Conditions" by Kelsey Shaler et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2021-130-RC1, 2022

Dear authors,

the topic of the study is definitely interesting and relevant, as there are still quite some uncertainties about how well aerodynamic codes perform in unsteady, and especially turbulent, conditions.
Unfortunately I think this article needs to be improved in some important aspects before the implications of the study become clear.

The main areas of improvement are (see the specific comments below for more details on these points):

- The 'average QOI' measure that is used quite often in the code is very difficult to interpret for me. A lot of different measures, some of them reduntant, such as rotor torque and power at constant rpm, are blended together in this value.

- There is much more information needed on the other codes than OLAF. When evaluating shear, yaw and turbulent inflow the relevant models in the BEM code need to be mentioned. Some information on the discretization and modeling of the SOWFA results also needs to be in the paper to judge if these results can be used as a reference. In the discussion, some emphasis is on the tower wake. The tower modeling in the three codes needs to be explained to understand this discussion better.

- The time step for OLAF is determined by looking at steady results. It is likely that the unsteady cases might need a finer time step than roughly 0.1 seconds.

- It is somewhat unclear which parameters (turbulence intensity, yaw, shear) where chosen for the different cases. Table 6 could be modified to describe the cases used in the different sections in the paper in detail.

- A lot of plots need at least axis ticks, maybe a grid would be even more clear. As it is, it is difficult to see for example where 180 degrees are in the plots that use azimuth on the x-axis.

- I can't see any physical explanation for the results in Figure 16, where the mean rotor torque changes by 10% due to a TI change from 0% to 1%. I think there must be an error either on the post processing or in the application of the turbulence. For reference, for actuator line results in Meyer Forsting et al (see below) the difference in mean torque between 0 and 15% TI was found to be less than 10%.

- The results are obtained for a flexible turbine. Please add some plots indicating how the deflected blades look at 8 m/s to give an idea of the shape of the rotor.

- A lot of topics are touched in the introduction, and quite many shortcomings of the BEM method are named. However there are not many references to the relevant literature, so it is often unclear if these are issues that can only be adressed by higher fidelity codes. It is also unclear what other work has been done previously on comparing aerodynamics codes of different fidelities for similar cases. Below is a list of relevant publications from the top of my head. Forgive me that there is a bias towards DTU publications. I don't say that all of these should be added, and it would be great to add some references to publications from other institutions as well. The only publication that I think should definitely be added as a reference because it is so closely related to the present work is the publication comparing actuator line and FVW results by Meyer Forsting et al.

Please find some relevant references below, followed by specific comments to the text.

Best regards,
Georg

Large rotor motion (floating platform):

- Ramosâ□□García, N., Kontos, S., Pegalajarâ□□Jurado, A., González Horcas, S., & Bredmose, H. (Accepted/In press). Investigation of the floating IEA Wind 15 MW RWT using vortex methods Part I: Flow regimes and wake recovery. Wind Energy. https://doi.org/10.1002/we.2682

Comparison between FVW and BEM:

- Perez-Becker, S., Papi, F., Saverin, J., Marten, D. , Bianchini, A. Paschereit, C.O.:Is the Blade Element Momentum theory overestimating wind turbine loads? – An aeroelastic comparison between OpenFAST's AeroDyn and QBlade's Lifting-Line Free Vortex Wake method. Wind Energ. Sci., 5, 721–743, https://doi.org/10.5194/wes-5-721-2020, 2020
- Hauptmann, S., Bülk, M., Schön, L., Erbslöh, S., Boorsma, K., Grasso, F., Kühn, M. and Cheng, P.W.: Comparison of the lifting-line free vortex wake method and the blade-element-momentum theory regarding the simulated loads of multi-MW wind turbines. J. Phys.: Conf. Ser. 555 012050, 2014
- Boorsma, K., Wenz, F., Lindenburg, K., Aman, M., and Kloosterman, M.: Validation and accommodation of vortex wake codes for wind turbine design load calculations.Wind Energ. Sci., 5, 699–719, https://doi.org/10.5194/wes-5-699-2020, 2020

Non-planar rotors:

- Li, A., Gaunaa, M., Pirrung, G. R. and Horcas, S. G.: A computationally efficient engineering aerodynamic model for non-planar wind turbine rotors. Wind Energy Science, 7, 75-104. https://doi.org/10.5194/wes-7-75-2022, 2022
- Li, A., Gaunaa, M., Pirrung, G. R., Meyer Forsting, A., and Horcas, S. G.: How should the lift and drag forces be calculated from 2-D airfoil data for dihedral or coned wind turbine blades?, Wind Energ. Sci. Discuss. [preprint], https://doi.org/10.5194/wes-2021-163, in review, 2022.

Swept blades:

- Li, A., Pirrung, G. R., Gaunaa, M., Madsen, H. A., & Horcas, S. G.: A computationally efficient engineering aerodynamic model for swept wind turbine blades. Wind Energy Science, 7, 129-160. https://doi.org/10.5194/wes-7-129-2022, 2022

BEM modifications to deal with shear and yaw error:

- Madsen , H.Aa., Larsen, T. J., Pirrung, G., Li, A., & Zahle, F. (2020). Implementation of the Blade Element Momentum Model on a Polar Grid and its Aeroelastic Load Impact. Wind Energy Science, 5, 1-27. https://doi.org/10.5194/wes-2019-53

Comparison between FVW and actuator line:

- Meyer Forsting, A., Pirrung, G., & Ramos García, N. (2019). A vortex-based tip/smearing correction for the actuator line. Wind Energy Science, 4, 369-383. https://doi.org/10.5194/wes-4-369-2019

Comparison between aerodynamic codes in general:

- Schepers, J.G. et al: IEA Wind TCP Task 29, Phase IV: Detailed Aerodynamics of Wind Turbines. Zenodo. https://doi.org/10.5281/zenodo.5809137, 2021

Vortex code MIRAS:

- Ramos-García, N., Sørensen, J., and Shen, W.: Three-dimensional viscous-inviscid coupling method for wind turbine computations, Wind Energy, 19, 67–93, 2016

Specific comments:

- Title: 'Structural' could be removed, it is a bit confusing to me in the title because all the models are aerodynamic models.

- Abstract page 1 line 5: 'Free vortex wake (FVW) methods model such complex physics while remaining computationally tractable to perform the many simulations necessary for the turbine design process.' I am not sure if this is true. A typical design process would need many simulations with slightly altered design variables. Maybe you know a publication where FVW methods were used for turbine design?

- Abstract p 1 l 9 : 'low-fidelity blade-element momentum (BEM) structural results'. Maybe 'structural' could be removed here.

- Abstract p1 l 10:'high-fidelity simulation results': Maybe this could be replaced by 'actuator line results' to be more specific.

- p 1 l 21: 'As rotor size increases, substantially more energy is captured through greater swept area, thus reducing specific power while increasing turbine capacity factor.' This is not so much a feature of the rotor size but rather of the turbine design. A large rotor or a small rotor can both be designed for high or low specific power.

- p 2 l 25 'increased blade flexibility and the use of multi-element airfoils, such as the use of flaps' I don't quite understand what the flaps have to do with the large blade deflections.

- p 2 l 30: 'large blade deflections may cause a swept rotor area that deviates significantly from the rotor plane and the turbine near wake to diverge from a uniform helical shape.' I agree that these are important issues. We have tried to solve these to some degree also in the engineering model context, see the articles by Li et al I mentioned above

- p2 l 31: 'Such deviations violate the planar swept area assumption, causing three-dimensionality of the aerodynamic effects and increasing the importance of accurate and robust dynamic stall models.' I don't follow the argumentation here. How do we get from violations of BEM assumptions to the need for more accurate dynamic stall models?

- p3 Figure 1: It would be nice to include the root vortex as well in the figure. If I understand correctly, the wake will roll up into a root and tip vortex after the near wake region and this is not clear from the figure.

- p3 l 89: 'As part of OpenFAST, induced velocities at the lifting line/blade are transferred from OLAF to AeroDyn15 and used to compute the effective blade angle of attack at each blade station, which is then used to compute the aerodynamic forces on the blades.' Some more detail is needed here in my opinion. When the effective angle of attack is computed in aerodyn, is the theodorsen effect (typically part of the dynamic stall model) then switched off? Otherwise there may be a double accounting for the vorticity shed behind the blade. I would think that the AOA from OLAF is already the effective AOA.

- Discretization study: The discretization study (related to the time step) is performed for uniform wind speed. The dynamic behaviour (which is compared later on in the article) will likely have a much bigger dependency on the time step. The time step that is chosen corresponding to 5 degree is very large (above 0.1 seconds even for rated rotor speed). For comparison, a discretization of 0.025 seconds, corresponding to between 0.8 -1.1 degree azimuthal discretization was chosen in Ramosâ□□García et al.: 'Investigation of the floating IEA Wind 15 MW RWT...'. Are the other codes using the same time step?

- p6 l 153: 'This was done because it is not possible to know a priori which regularization parameter can be used as a reference for the convergence study. The questions remains as to whether the actuator-line simulations can be used as a reference.' I think a lot more information on the actuator line simulations is needed here: Spatial discretization, time step, any filtering or smearing correction applied, how was the unsteadiness handled regarding both theodorsen effect and dynamic stall, how were the forces smeared out ... Without this information, it is really impossible to know whether the actuator line simulations can actually be used as a reference. Regarding the regularization parameter: One conclusion in Meyer Forsting et al. ' A vortex-based tip/smearing correction for the actuator line' was that the force smearing in actuator line computations behaves like a vortex core, and that activating the smearing correction leads to similar results as using a very small vortex core radius. So I think it should be possible to have an idea from the actuator line simulations which regularization parameter should be applied in the OLAF simulations, but I don't have much information on the actuator line simulations and I might be wrong.

- p7 'Based on the results given in Table Table 5'. One 'Table' should be removed in this sentence

- Table 5: Why not use the zero crossings from Figure 5? At least for 10 m/s and 12 m/s the curves look pretty smooth. For 12 m/s the zero crossing is at roughly 1.2, which is somewhat far away from 0.87.

- p8 l161: 'In this work, the OpenFAST-FVW code was compared to traditional OpenFAST-BEM and large-eddy simulation': There needs to be a lot more information on these other codes. For example what yaw correction, dynamic inflow, unsteady airfoil aerodynamics model does the BEM employ, where is the AOA defined in all codes (important for cone and prebend, see Li et al 'How should the lift and drag forces be calculated...') Also some information on discretization is missing. Especially because there are detailed studies on the discretization of the vortex code in the previous section, some info is needed on the AL simulations, as I wrote in an above comment.

- p9 l 166: 'For all simulations, structural modeling was done using the structural module ElastoDyn from OpenFAST.': Is it correct that this means that elastic torsion is not modeled? I would think that, especially when using the more computationally heavy aerodynamic models, it would make sense to use higher fidelity on the structural side. Because the simulations are elastic, it would be nice to include some deflection plots (at least flapwise deflection, if, as I think, torsion is not included) so that the reader can get an impression of how the rotor actually looks like at the different wind speeds.

- p9 table 6: It would benefit the paper greatly if this table was more detailed. It would help if it was clear exactly what the parameters are in the different cases studied in the following sections. Then it would be much easier to see if, for example, there is shear and turbulence in the yaw misalignment cases studied in Section 4.1.1.

- p9 l 165: 'The wind turbines blades'. Change to 'The wind turbine blades'

- p9 l 172: 'Turbulence is simulated using the Kaimal spectrum with exponential coherence model and the standard IEC turbulence model was used. The time-dependent 2D wind field is propagated along the wind direction at the mean wind speed of the midpoint of the field.' Some more information is needed here: How is the turbulence applied in the vortex code and in the AL code? It is not straight forward to match the BEM code turbulence there I would think.

- p9 l 174: 'Each case from the measurements is simulated in TurbSim and OpenFAST six times'. I don't think there are any measurements in this study?

- p 10 Table 7: It needs to be specified if the blade forces are normal and tangential to the local cord or to the rotor plane. Also I think the methodology later of averaging all the deviations of the different QOIs needs to be rethought somewhat. Some QOIs are redundant (such as shaft torque, rotor power and in-plane bending moment who behave identically for fixed rotor speed, see Figure 6 a), b) and d)).

- p 11 Figure 6: Redundant plots, see above. In general the deviations from the BEM at zero yaw seem to be too large. Are the BEM computations set up correctly? The comparisons in Ramosâ□□García et al. 'Investigation of the floating IEA Wind 15 MW RWT using vortex methods...' for example showed differences of about 1.4% at zero yaw. Also in the Task 29 Phase IV report the differences between BEM codes and FVW codes for axial inflow are much smaller. If everything is indeed set up correctly then some explanation for this larger than expected difference at zero yaw should be given.

- p 12 Figure 7: It is very different to interpret this Figure that includes the averaged deviation for all QOIs. I think you need to look at values for some individual QOIs to make any interpretation possible. What do the dots represent? In section 4.1 it was stated that the yaw misalignment cases were simulated in constant inflow (I guess this means uniform?). But if I misunderstood that and there is indeed turbulence, then I would expect six seeds according to section 3, but there are more than six points per yaw angle.

- p 13 Figure 8: Maybe it would be nicer to show in-plane and out-of plane loads instead. Then there is a more direct relation to the observed power differences. There are also some issues with overlapping axis labels.

- p 13 Figure 8 c): How can there be such large differences in axial induction, but much smaller differences in normal force?

- p 14, top 'Equation 4.1.1.'.: Should this be Equation 5?

- p 14 Equation 5: I would rather compute the difference at each spanwise location divided by the value at that same location. As they are presented here I think the differences are not intuitive. For example the BEM-SOWFA difference according to Equation 5 is apparently up to more than 50% at 85% span, while the local differens in the plot is about 20% (500N vs 600N).

- p 14 l 222: 'BEM results compare well for no yaw misalignment results, averaging 3.29% but reaching up to 22.1%'. For the simplest case in the comparison, I don't think deviations of up to 22.1% qualify as comparing well.

- p 15 l 230: 'moments show three large dips corresponding to the blade passing behind the turbine.' I guess you mean behind the tower?

- p 15 l 237: 'During the blade motion where the blade is upstream of the tower' For a downwind turbine, the blade is probably almost always downstream of the tower. Do you mean 'where the blade is not in the wake of the tower' instead?

- p 16 Figure 11: 'Plots on the right show mean quantities and plots on the left show percent difference of the mean results.' Right and left is switched.

- p 16 l 253: 'For all results but tower-base yaw moment, the relative trends of OLAF and BEM are the same with changing shear exponent, though the percent difference between the results does increase slightly with increasing shear exponent' No, for example increasing the shear exponent from 0.1 to 0.2 increases torque and OOP moment in OLAF but decreases it in BEM.

- p 17 l 269: 'This is expected, since the flow behind the tower is dominated by the tower wake and likely unaffected by the inflow shear exponent.' First, I am not sure how the tower is modeled in the different codes. Second the flow behind the tower is affected by both tower wake and shear exponent: The tower wake will change depending on the incoming wind speed, which will be given by the shear exponent.

- p 18 l 283: 'For all OLAF results, the yaw moment spikes when a blade is behind the tower,' It is a bit difficult to see without a grid in the plots, but I don't think that the spike is when a blade is behind the tower. Then there should be a spike at 180 degrees, but it is after 200 degrees I think.

- p 18 l 288: 'but the secondary bump that OLAF predicts is not present for the BEM results.' I'm curious where this bump is coming from.

- p 19 l 299: 'This is further supported in Figure 12, which shows box-and-whisker plots for each shear exponent' Should this refer to Figure 15 and turbulence intensity instead?

- p 19 l 308: 'However, as soon as turbulence is introduced into the inflow, this relationship flips and BEM predicts significantly lower rotor torque and OoP blade-root bending moment for all azimuthal locations.' I think something must be wrong here. I can't believe that adding one percent TI changes the mean torque by +-10% in OLAF and BEM.

- p 21 l 315: 'it was found that for all considered QoIs, SOWFA, OLAF, and BEM results compare well for steady inflow conditions with no yaw misalignment.' I disagree. The rotor power was off by 10% between BEM and the other codes for uniform inflow, see Figure 6.