Reviewer Responses

Reviewer #1

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.

  AUTHOR RESPONSE: Since so many QoI are considered in this work, this "average QoI" measure is meant to be a big-picture view of how the models perform across the QoI. This is often used in conjunction with the box-and-whisker plots, and the interpretation of these plots and the average QoI metric have been clarified in the text.

- 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.

  AUTHOR RESPONSE: Thank you for bringing this to our attention. We have added the following sections describing the BEM/OLAF and SOWFA models, respectively.

  "The tower shadow model for the blade induction and blade loads are the same for BEM and OLAF, the details of which can be found in Aero (2022). An additional tower potential flow model was used in OLAF simulations to model the effect of the tower on the wake."

  "The Simulator fOr Wind Farm Applications (SOWFA) is a collection of libraries to perform large-eddy simulations of windplant flows (Churchfield et al., 2012; Churchfield and Lee). SOWFA is built on top of the OpenFOAM framework Weller et al.(1998). In SOWFA, the filtered Navier Stokes equations are solved using a finite-volume formulation. The spatial derivatives are computed using second order finite-difference and the time advancing is done using second order backward differentiation. The wind turbine blades and tower are modeled using body forces from actuator line model (Martínez-Tossas et al., 2015;Churchfield et al.; Sørensen and Shen, 2002) "

- 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.

  AUTHOR RESPONSE: The authors agree that the recommended time step size can vary based on many factors, including the turbulence intensity level. This is mentioned in the text and the link to turbulence intensity is now specifically called out as well. The discretization studies in this work are meant to provide a starting point for those who might be using FVW models, and OLAF in particular, and are not meant to cover all conditions and turbines.

- 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.

  AUTHOR RESPONSE: Table 6 has been modified to explicitly state all tested conditions, and the text has been altered to clarify how the parameters are varied and what the nominal conditions are.

- 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.

  AUTHOR RESPONSE: Grid lines have been added to all plots.

- 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%.

  AUTHOR RESPONSE: Thank you for bringing this to our attention, it seems there was a discrepancy in the post-processing of the results. There is now a minimal difference in rotor torque between 0% and 1% TI, with a larger difference when the TI is increased to 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.

  AUTHOR RESPONSE: This is a good point, thank you from bringing this up. In Figure 5, we have removed rotor power and added OoP blade-tip deflection. This QoI was not added to other figures because the trends with Shear and TI mirror the OoP bending moment trends. Instead, the matching trend is mentioned in the text.

- 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.

  AUTHOR RESPONSE: Thank you for pointing us to these references, we agree that more literature could have been provided on similar studies investigating aerodynamics with different levels of fidelity. We have now modified the introduction to include some of the work you provided.

Specific comments:

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

  AUTHOR RESPONSE: This has been removed.

- 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?

  AUTHOR RESPONSE: We agree that FVW are still not being used for full wind turbine load case calculations. They are more and more involved in the design process though. We have reformulated the sentence replacing “many” with “key”. We have also added the following reference which investigate the application of FVW to design load calculations: https://wes.copernicus.org/articles/5/699/2020/

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

  AUTHOR RESPONSE: This has been removed

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

  AUTHOR RESPONSE: This change has been made

- 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.

  AUTHOR RESPONSE: This has been clarified in the text.

- 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.

  AUTHOR RESPONSE: The authors agree this statement does not belong in this sentence, and it has been removed.

- 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

  AUTHOR RESPONSE: This reference has been added to the paper.

- 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?

  AUTHOR RESPONSE: You are correct, there is no direct link between the two statements. What was meant here was that the large flexibility of the blades leads to larger motions of the blade, which can lead to larger angle of attack fluctuations, and therefore increasing the importance of accurate and robust dynamics stall models. The violation of the planar swept area increases 3d interactions between the blade elements and therefore further violate the assumptions of independence of the blade annuli. The text was modified accordingly.

- 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.

AUTHOR RESPONSE: Yes, the root vortex is included after the near wake region. Unfortunately the image can not be altered at this time, but the point about the root vortex has been added to the caption of the figure. If we are able to modify the image before publication we will do so.

- 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.

  AUTHOR RESPONSE: You are right, the shed vorticity effect of the dynamic stall is automatically turned off when OLAF is used. We have added a sentence in the text to precise this.

- 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?

  AUTHOR RESPONSE: The authors agree that a follow-on study in which the response to turbulent inflow is explored in more detail, including for the discretization and convergence studies. However, currently it is beyond the available resources. Given the small differences seen for all tested discretization levels (Figure 2), the authors are comfortable moving forward with a 5-degree discretization level, especially in the attempts to balance model accuracy with computational cost. As part of the IEA Wind tasks, we have also observed that such discretization appears to be common for vortex filament practitioners. Vortex particle practitioners (such as Ramos-Garcia et al.) typically require finer spatial and temporal discretization since they use lower-order vortex elements (i.e. particles instead of filaments). We further note that OpenFAST can use a smaller time step for aeroelastic simulations, in which OLAF is called at each time step (with a new circulation solved for based on the new blade position and velocities), but the wake is only updated at the time step of OLAF. This allows for a compromise between accuracy and computational time.

- 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.

AUTHOR RESPONSE: We agree with the reviewer, the vortex core has a big effect on aerodynamic quantities along he blade. We have expanded this section and have now added the following text to describe the LES simulation step:

"We use a core spreading value of e/D=0.05 in SOWFA. This value is close to the typical values used in other studies but is not expected to produce optimal results Martínez-Tossas et al. (2015); Martinez-Tossas et al. (2017, 2018). Recent work suggests that optimal values require much finer grid resolutions or corrections Martinez-Tossas and Meneveau (2019); Meyer Forsting et al. (2019). Using an optimal regularization is an active research topic and will be considered as part of future work. "

Regarding the regularization, despite the recent research carried on (as pointed out by the reviewer), we believe that more work is still needed to come up with a regularization method that is consistent across computational methods and independent of spatial and temporal discretization. We have therefore chosen to keep this parameter as frozen in the discretization study.

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

  AUTHOR RESPONSE: This change has been made.

- 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.

  AUTHOR RESPONSE: Thank you for noticing this, it appears to have been a typo. We have now corrected it to 1.2.

- 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.

AUTHOR RESPONSE: The velocity sampling (used to compute AoA and forces) in the ALM is done at the actuator points. We have added this text to the manuscript to clarify this:

"Velocity sampling is done at the actuator points, to be consistent with the original formulation of the actuator line model and other studies (Sørensen and Shen, 2002; Martínez-Tossas et al., 2015;Martinez-Tossas et al., 2018)."

Details of the BEM and OLAF have also been added to the text.

- 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.

  AUTHOR RESPONSE: The authors agree on the importance of torsion is aeroelastic simulations. However, at this time the computational cost of BeamDyn, the higher-fidelity elastic model available in OpenFAST, makes it intractable to run with OLAF. There are plans to address this issue and hopefully a study including torsional effects can be conducted in the future.

  OoP deflection plots have also been included. Torsion is not included in this OpenFAST model because ElastoDyn, the structural model, does not handle this DOF.

- 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.

  AUTHOR RESPONSE: This table and the text have been clarified.

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

  AUTHOR RESPONSE: This change has been made.

- 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.

AUTHOR RESPONSE: These details have been added to the text.

- 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?

  AUTHOR RESPONSE: This language has been changed.

- 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 (b) and d)).

  AUTHOR RESPONSE: The blade forces are normal and tangential to the local cord. This has been clarified in the table. However, these QoI have since been removed from the paper. Since so many QoI are considered in this work, this "average QoI" measure is meant to be a big-picture view of how the models perform across the QoI. This is often used in conjunction with the box-and-whisker plots, and the interpretation of these plots and the average QoI metric have been clarified in the text. Additionally, the rotor torque plot has been replaced with OoP blade-tip deflection. Though some of the remaining quantities follow similar trends, the authors feel it is important to include these plots as they are of structural quantities that are of interest to the community.

  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.

  AUTHOR RESPONSE: The deviation that was seen between BEM and OLAF results in the 0-degree yaw misalignment cases was due to the core size used in the OLAF simulations. This parameter has a significant effect on the OLAF results, and a value was chosen to best match the CFD results. These results have been redone so that they more closely match the BEM results, since that is what most people would expect to see. The discussion has been updated to reflect this change.

- 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.

AUTHOR RESPONSE: Further details of the box-and-whisker plots have been included in the text. In summation, each dot represents a separate QoI.

- 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.

  AUTHOR RESPONSE: Unfortunately, the available OpenFAST outputs are the normal and tangential forces, thus those are what are presented in the paper. The axis issues have been fixed.

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

  AUTHOR RESPONSE: These plots were actually mislabeled, and induced u- and v-velocity is shown instead of induction. Upon review, the authors do not believe that these plots add much to the discussion, especially after redoing the OLAF results to more closely match BEM. As such, this plot has been removed from the paper.

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

  AUTHOR RESPONSE: This change has been made.

- 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).

  AUTHOR RESPONSE: See above comment.

- 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.

  AUTHOR RESPONSE: See above comment.

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

  AUTHOR RESPONSE: Yes, this change has been made.

- 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?

  AUTHOR RESPONSE: Yes, this change has been made.

- 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.

  AUTHOR RESPONSE: This change has been made. Found this error in a few parts of the paper, and it has been fixed throughout.

- 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.

  AUTHOR RESPONSE: This statement is referring to the overall trends and has been clarified in the text.

- 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.

  AUTHOR RESPONSE: A description of the tower models for each code has been added to the text. For the note about the shear, this is a good point and has been mentioned in the text.

- 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.**

  AUTHOR RESPONSE: Grid lines have been added to all plots. With these added grid lines it is more apparent that this spike is indeed at 180 degrees, which is when the blade would pass behind the tower.

- 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.

  AUTHOR RESPONSE: Upon further inspection, this secondary bump is present in the BEM results for the no shear case but to a much lesser degree. The text has been modified accordingly.

- 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?

  AUTHOR RESPONSE: Yes, this reference has been change.

- 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.

  AUTHOR RESPONSE: Yes, there was indeed an error in the processing and these results have changed.

- 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.

  AUTHOR RESPONSE: With the change in OLAF results, these comparisons have also changed substantially throughout the document and the BEM and OLAF codes no longer have differences of 10% at 0-degree yaw misalignments.

Reviewer #2

The paper compares aeroelastic simulation results of a newly developed lifting line model against a standard BEM model and an actuator line model, all coupled to the same structural module. The paper is relevant for the wind community and it is of great interest to a broad audience that employs the open source tools of NREL. Despite the fact that the level of innovation and originality of the reported work is relatively low, still the paper provides some useful information about the level of agreement of the predictions of various aerodynamic options of different fidelity.

Therefore, in my opinion the paper deserves publication, however after major revision is performed in the first submission.

Several, less or more important, specific comments can be found in the accompanying pdf file (performed directly on the body of the paper). The most important changes are though collected and listed below:

1) In the reviewer opinion the main shortcoming of the reported work is the way that results are presented. More specifically,

- starting from the simplest zero yaw case, the difference between the BEMT and the other two models is substantial. I would not expect such a high difference between the three methods given that they all share the same set of polars. In the reviewer opinion some further investigation is needed before proceeding to comparisons of more complex conditions. The first thing that should be highlighted and explained is the very big difference in the induction of Figure 8c which of course drives differences in the mean values of loads. Why the BEMT model over-predicts induction by more than 10% compared to OLAF (most probably also compared to the CFD – btw velocity predictions by SOWFA are missing).

AUTHOR RESPONSE: The deviation that was seen between BEM and OLAF results in the 0-degree yaw misalignment cases was due to the core size used in the OLAF simulations. This parameter has a significant effect on the OLAF results, and a value was chosen to best match the CFD results. These results have been redone so that they more closely match the BEM results, since that is what most people would expect to see. The discussion has been updated to reflect this change.

In Figure 8c, velocity predictions are not available for the SOWFA results.

- when dealing with the yawed cases mean value (0P) is not so important (besides some explanation of the mean value differences will be provided by the analysis of the 0 deg yaw case). What is more important is the 1P harmonic (in terms of magnitude and phase). I don't see a reason why one would compare sdv in a periodic response case. Therefore my strong recommendation for the yawed cases is to compare 1P amplitude and phase. I would mainly focus on flapwise moment as differences in blade loads drive differences in the loads of all other components. Note that the information provided in the plots is vast and sometimes becomes confusing while the key differences are not really highlighted.

AUTHOR RESPONSE: The authors agree that a more in-depth analysis of this case is warranted, and will be considered in future work, particularly when an FFT analysis is available. At this time we are unable to produce the additional simulations that would be required to further analysis the points made here, but we will certainly focus on this in future work.

- From all the results presented in 4.1.1 the most important information is provided in Figure 8c, which explains the mean value differences of BEMT against the other two models and in Figure 9b which shows the azimuthal variation of the flapwise moment and explains again the difference of the various models in terms of the azimuthal variation of the induction (of course a plot with the 1P amplitude and phase variation vs. yaw angle would be much more instructive). Then all the rest would be probably expected and linked to the deviations in the rotor loads, as all simulations are based on the same structural code. In this case the authors are limited to a superficial discussion of the percentage change of the different loads and they do not go deeper to the discussion of how the difference in the azimuthal variation of the flapwise moment affects and justifies differences in the loads of the components that are not rotating. For example, the higher amplitude of the flapwise moment predicted by BEMT justifies the increased mean tower yaw moment and the lower mean justifies the lower tower bottom bending moment. Looking in figure 9b, nobody would expect tower moments predicted by the different models to be the same. Furthermore, the information of how much they differ is not so useful either. What is more useful again is to explain the differences in figure 9b in terms of the predicted induction by the different models (varied azimuthally this time).

AUTHOR RESPONSE: The authors agree that a more in-depth analysis of this case is warranted, and will be considered in future work, particularly when an FFT analysis is available. At this time we are unable to produce the additional simulations that would be required to further analysis the points made here, but we will certainly focus on this in future work.

- Time averaged loads and induction distributions of Figure 8 are completely pointless (apart from the 0 deg yaw plots). What is more relevant in this case (for the yawed cases) is to compare distributions at different azimuthal positions (of course after periodicity has been reached). For example with a step of 90deg.

AUTHOR RESPONSE: These results have actually been removed, as it was decided they not contribute much to the results.

- When it comes to turbulent flow predictions sdv is more relevant but what would be also interesting for the reader is to see how the energy is distributed to the various frequencies. Azimuthal averaged plots don't offer much more than those presented in 4.1.1. However, comparison of FFTs would be definitely preferable.

AUTHOR RESPONSE: The authors agree that including an FFT comparison would be interesting. However, the simulations were run with an output timestep of 1s to reduce computational time, which is insufficient for computing FFTs. This is certainly something that will be included in future work.

2) The introduction section needs to be improved. First, the context of existing engineering models is much different than that presented by the authors. There are several engineering hybrid models that account for the effects that standard BEM models omit and these seem to be neglected by the authors. Some references on improved BEM models are missing. Furthermore, the type of CFD analysis performed should be detailed in the introduction section (actuator line modelling).

AUTHOR RESPONSE: Thank you for bringing this to our attention. We used the actuator line model in SOWFA. This has been added to the introduction:

"These comparisons provide better understanding of OLAF, BEM, and actuator-line formulation of SOWFA (Churchfield 2012) relative performance when subjected a range of inflow conditions, both simple and challenging."

Additionally, more details of BEM models and available corrections have been added.

3) With regard to the theoretical model no explanation is given on how the authors deal with the violation of Kelvin's theorem when truncating the near wake. Is this maybe the reason why the "far wake extent" results don't seem to converge in figure 2?

AUTHOR RESPONSE: The violation of Helmholtz's first law is addressed at the end of the first paragraph in the Overview of Olaf section.

4) The total number of simulated revolutions is not provided (if I'm not wrong). This will probably explain why computational time differs so much between the different wind velocities (I suppose they correspond to different number of revolutions but the same distance travelled by the wake).

AUTHOR RESPONSE: The extent of the near- and far-wake segments are defined separately. The near wake is defined as an angle, which translated to number of revolutions. Our recommended near wake length is 2 revolutions. This was incorrectly entered into Table 4 as "2D", and has been corrected. The far wake is defined in terms of number of points and is based on the distance travelled instead of the number of revolutions. Thus, as the wind speed increased the recommended distance remains the same but the number of points simulated is reduced because fewer points are needed to cover this distance. Note that the computational times shown in Figure 3 are only for wake discretization and near-wake extent. We have included the simulation time figures for varying far wake extent. The times weren't previously included because they were just linear.

5) I would be nice to provide the equations for all the filter functions you apply in the treatment of the wake (since a sensitivity analysis is performed on the tuning parameters of these filters).

AUTHOR RESPONSE: Thank you for your comment, we have now added the formulae of the Vatistas and core spreading model to the document.