WES-2023-109 – authors response

Going Beyond BEM with BEM: an Insight into Dynamic Inflow Effects on Floating Wind Turbines

F. Papi, J. Jonkman, A. Robertson, A. Bianchini

Dear Reviewer, dear Editor,

Thank you for your time managing and reviewing our work and for your qualified feedback. Based on the Reviewer' suggestions, we have done our best to improve the paper.

We have provided detailed answers to your comments below, in blue colored text for your convenience.

Best regards,

F. Papi, J. Jonkman,
A. Robertson, A. Bianchini

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Reviewer #1 – M. Hansen

I am still not quite convinced that including the structural velocities from e.g. surge in the momentum equations is correct, unless the inertia term from accelerating the mass enclosed in the control volume is somehow also considered. But as written in the paper this is debatable.

The Reviewer is absolutely right. The approach of treating structural velocities as if they were variations in relative inflow is formally incorrect. When approaching this problem, we found it surprising that despite this, BEM shows remarkably good performance in many cases if compared to experiments and higher-order models. This is indeed the spirit with which this work was approached and the motivation behind the title "Going beyond BEM with BEM". That being said, we acknowledge that some paragraphs could have been phrased better, and thus the following was done to improve the paper:

1. Introduction: a paragraph was removed, as it partially overlapped with section 2.1 and could add confusion. When describing the paper objectives, the following was added: "In particular, the predictive capabilities of the—formally incorrect—approach of considering structural velocities as apparent inflow in the BEM balance and using a dynamic wake model to account for the changing conditions on the rotor in such a scenario are evaluated."
2. Section 3.2: the end of this section was significantly restructured, to better highlight the limitations of the approach of considering structural velocity in the momentum equation that emerged during the study.
3. Section 3.3: the end of this section was streamlined. Some of the considerations were moved to the conclusions, as they refer not only to the results presented in section 3.2 but to the information presented throughout section 3.

4. Section 4: conclusions were amended in many places, with the main rationale being to better highlight the performance of BEM in relation to how it is formulated (i.e., with structural velocities in the momentum balance). In particular, it is explained more clearly that this approach is found to work well in the tests at rated wind speed. Less so in LC 3.26, because such a model cannot distinguish between VRS and flow reversal on the rotor.

In my opinion a dynamic wake model as e.g. Oye model is modelling the inertia of the flow when changing the loads for a fixed rotor. An open question is, if it is good enough to apply such a model for a dynamically surging rotor and only consider the structural velocity in the blade element part and keeping the control volume fixed.

Thank you again for the interesting comment. Indeed, this remains an open question. In this paper, the problem is approached from a somewhat similar, but also quite different perspective: i.e., is it good enough to treat structural velocities as apparent wind variations, and how does a dynamic wake model handle such a scenario? We believe that the perspective of this paper is clearer thanks to the changes to the manuscript detailed within the previous point. We have highlighted the point of view the Reviewer raised in the conclusions: “Because structural velocities are included in the momentum balance as apparent wind variations, BEM and DBEM cannot distinguish between flow reversal and VRS, and therefore, despite the good prediction of aggregated rotor aerodynamic forces, differences with respect to higher order theories in the prediction of spanwise quantities are apparent. This highlights an area of possible future improvement in BEM-based engineering tools. For instance, the ability of a dynamic wake induction model to model the inertia in the flow of a floating rotor if structural velocity is not considered in the momentum balance, and possible improvements to these models in such a scenario, remains an open question.”

In the same line I don’t consider Eq. 1 on page 5 as being part of the momentum equation, but purely blade element theory as this is only an expression for the apparent inflow at the rotor plane. The momentum equation would be an equation to solve for the actual axial induction, a, in this expression.

This is true, Eq. 1 is not part of the momentum balance in a strict sense. However, it does model the contribution of the wake on the relative inflow at the rotor. The Reviewer is anyhow right in the fact that, as presented, it may introduce confusion. Therefore, we now refer to this equation as an expression for the “axial velocity at the rotor plane”.

It could perhaps be mentioned that the conditions in Table 1 on page 9 correspond to the optimum TSR=7.4 for the DTU 10 MW reference turbine.

This is again a good observation. We have edited the caption of Table 1 accordingly.

On page 11 line 280 is stated that the ALM and the LLFVW only use static airfoil data. Why not include a dynamic stall model as this should give better results. Is there a reason for this ?

In the tests performed in this study, and during the UNAFLOW campaign at large, apart from the inner part of the blade, which is responsible only for a fraction of the rotor thrust, the angle of attack is below the stall angle. Therefore, it is important to capture unsteady attached-flow effects, rather than dynamic stall. Both of these effects are included in the dynamic stall models in OpenFAST, which are based on the Beddoes-Leishman dynamic stall model. In particular, attached-flow unsteady effects are modelled according to Theodorsen’s theory, which contains a circulatory part, and an apparent mass part. The reduced frequencies at play in this study are, however, low, and therefore, the contribution of apparent mass effects is negligible. Circulatory effects are already embedded into ALM and LLFVW theory in an indirect manner, as both models generate unsteady vortices in the wake of the blade. To avoid accounting for these effects twice, dynamic polars were not considered in the ALM and LLFVW models. We have improved the explanation on page 4: “In this work, inflow angles are mostly kept below stall, and thus attached-flow unsteady aerodynamic effects have the largest impact on results. These are mainly caused by two effects: added mass and shed vorticity, with the latter being by far the most relevant at the reduced frequencies analyzed in this work. The widespread consensus is that these effects are intrinsically included in higher-order aerodynamic theories such as LLFVW and ALM and do not require dynamic polars to model this effect. Therefore, static polars are used for the aerodynamic models in this study.”

Various step cases are investigated, but is a step change in rotational speed relevant for a modern wind turbine having an enormous angular inertia moment making quick changes in rotational speed very difficult.

The Reviewer is right. As rotors increase in size, their rotational inertia also increases. In fact, if the IEA 15-MW rotor is compared to the NREL 5-MW rotor, the rotational inertia has increased more than 8 times, while swept area has increased a comparatively small 3.6 times. In the study, however, we are interested in evaluating how changes in surge and in rotational speed combine. Therefore, the step-changes that are simulated are purely conceptual and should be interpreted strictly in this sense. This has been also clarified in the paper.