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 Reviewers, dear Editor,

Thank you for your time managing and reviewing our work and for your feedback. Based on the Reviewers' 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 #2 (M.O.L. Hansen) comments:**

The topic of investigating the performance of BEM codes for FOWT is very interesting and of big practical importance, but not very new. A very similar study was made 10 years ago by

J.B. de Vaal et al., Effect of wind turbine surge motion on rotor thrust and induced velocity, Wind Energy (2012)

We thank the Reviewer for pointing out the importance of this topic. We indeed did not cite the work that the reviewer pointed out in our manuscript. This was only on oversight from our side, and the work of J.B. de Vaal et al., that we were aware of, is now included in the initial discussion phase. Despite the existing body of scientific literature on the subject, we believe that there is still widespread lack of consensus surrounding the topic of FOWT aerodynamics. While the conclusions of this work are in many ways similar to those of J.B. de Vaal et al.'s work, we believe this is relevant as they are reached for a different rotor design, validated by more recent experiments and higher order models such as ALM and LLFVW, and also account for pitch motion in addition to surge motion.

The conclusion in the submitted paper is reconfirmed, that a BEM code with a proper dynamic inflow model and an empirical Glauert model for high thrust coefficients used on FOWTs performs quite well for the exposed structural oscillations (amplitudes and frequencies). It would be really nice, if the expected range of frequencies and amplitudes for the DTU 10MW rotor (pitch and surge) was included in the paper for different foundation types. This will also show if going above possible frequencies in the PoliMi tunnel is of practical importance for a real FOWT.

We thank the reviewer for this comment. For semi-sub and spar platforms natural frequencies in pitch and surge are around 0.005-0.05 Hz. Wave excitation range is typically 0.05-0.5 Hz (with 0.08-0.2 Hz being most common). Tension Leg Platforms (TLPs), on the other hand, usually feature higher natural frequencies in pitch, roll and heave, approximately in the 0.5 to 5 Hz range, although amplitudes are typically small. Assuming reduced-frequency scaling is appropriate for this kind of aerodynamic phenomenon, it would make sense to go above the PoliMi test frequencies for this rotor. In fact, while the experimental apparatus is limited to approximately 2Hz, going up to 4Hz at model scale is well within the wave excitation range at full scale. It's important to note that reduced frequency is computed based on rotor diameter and wind speed as $f_r = \frac{fD}{U}$, therefore the appropriate frequency range at model scale varies based on working condition (wind speed) and turbine diameter, both at full scale and model scale. We have added a comment regarding the typical frequency ranges of utility scale FOWTs at lines 228-232.

The paper discusses different ways reported in the literature of how to treat the momentum equation in case of a dynamically oscillating wind turbine rotor. In basic fluid mechanics the conservation of momentum is used to determine an unknown force by keeping track of the total momentum deficit out of a control volume and including an inertia term in case of unsteadiness. The mean position of the turbine is not moving, so the control volume and velocities at the boundaries should be in a fixed frame of reference and not include the velocities of the rotor. These should in my opinion only be used when evaluating the angles of attack for the blade elements.

The Reviewer has raised an interesting point. From our understanding, this is somewhat of an open debate. We attempted to convey this in the introduction of this work. The Reviewer is suggesting, if we interpret correctly, that the structural velocity should be included only in the "Blade Element" part of the momentum balance and not in the "momentum" part. AeroDyn does not make this assumption, although the topic has been debated internally at NREL many times. Instead, structural velocity is included in both sides of the BEM balance in AeroDyn. As the Reviewer will agree, if no inertia terms are considered, however, including the structural velocity only in the "Blade Element" part of the BEM equations still violates the momentum balance. In fact, if the relative velocity at the actuator varies, axial induction will also vary, and thus the wake velocity is time-dependent, violating the steady-state assumption in which the momentum balance is formulated. Simulations and experiments have indeed shown that velocity measured downstream a surging rotor varies with time. (Cioni et. Al., 2023). Moreover, Boorsma and Caboni (Boorsma and Caboni, 2020) and Mancini (Mancini et al., 2022) have shown how including the velocity due to actuator motion in both sides of the momentum balance improves agreement with respect to LLFVW simulation that include it only in the "blade element" part. We have not yet encountered a "BEM" model that includes inertia terms in the momentum formulation. What we have observed throughout this work, is that in most realistic conditions, the BEM formulation implemented within AeroDyn, which is common to many wind turbine simulation codes, works well, despite its formal inconsistencies. This motivates the title of the paper "Going Beyond BEM with BEM". As the reviewer pointed out, we believe this topic is of practical importance, as it may help FOWT modelers make an informed decision on the modelling theories they use to approach such a problem. The introduction has been modified to reflect this discussion.

On page 5 and 6 the LLFVW is described as a dynamic vortex model where the vorticity is shed as vortex rings. Note that the coefficients in the Øye Dynamic Inflow model are actually calibrated from a similar dynamic ring vortex approach and the results using an unsteady BEM are therefore expected to be similar to the LLFVW output.

We thank the Reviewer for pointing this out. We were indeed aware of this but had not pointed it out in the paper. This consideration is now included in line 141 and 142 of the revised manuscript.

The axial induction velocities from the LLFVW shown in Figure 7 are very small and in the order of 0.03 m/s and compared to the inflow velocity of 4 m/s correspond to an axial induction factor of around a=0.03/4=0.008. If this is true then there is practically no induction for this case and what is the corresponding CT ?

The Reviewer is right. Due to a bug in the plotting script, a scale factor was erroneously being applied to induced velocities. Both figures 7 and 8 have been corrected and replaced. Thank you for catching this mistake.

On page 14 is reported a time constant for the dynamic wake LLFVW computations of around 3-4 seconds. In the Øye Dynamic Wake model the time constant is approximately the rotor diameter divided by the free wind speed and in the case of the UNAFLOW wind turbine should be around tau=2.4/4=0.6 seconds. That is the LLFVW model responds quite much slower to a dynamically changed force than will the Øye model. Since both the Øye dynamic wake model and LLFVW are based on a similar vortex ring model for the shed vorticity what is then the reason for this difference in time constant ? An Actuator Line simulation that through the N-S equations resolves the real physics and inertia of the wake response could be used to check these LLFVW results.

The Reviewer has a good point. The description the Reviewer is referring to was not precise. The time induced velocity takes to reach the new equilibrium value in Figs 7 and 8 is confused with rotor thrust. We have corrected the text (L360 and 379). In addition, to confirm the LLFVW results, we have run ALM simulations for the step tests in blade pitch and rotor speed. Although the magnitude of the step change is slightly different, and the overshoot of Fx slightly lower, the time the ALM simulations take to readjust to the new equilibrium thrust values is similar to LLFVW. The ALM results are included in Fig. 7 and 8 and in the text (L360-365)

The result shown in figure 12 is interesting. Here the simulations show that the axial induction factor for a pitching motion of amplitude between 1 and 2 degrees at a frequency of 0.1Hz and at a wind speed of 5 m/s can be as high as a=4 near the blade tip, meaning that the blade will experience a velocity from behind of about 3 times the wind speed. This is estimated to occur at wave heights of 10-13 meters. Is it a realistic scenario to have a wind speed of only 5 m/s at wave heights of more than 10 meters ? And how should a Glauert correction be when the axial induction becomes so large corresponding to a thrust coefficient way above 2 ? And is it the free wind speed or the apparent wind speed taking the structural velocity into account one should use when computing the thrust coefficient CT in a BEM based model ?

The Reviewer is again spot-on. Such high waves in low wind speed conditions are unlikely. We have highlighted this in the text, also in response to some internal members of our teams that have read the draft and raised similar concerns. We think this case represents a limit case, and is useful in this work to "stress test" the aerodynamic models and find their limits. However, despite being unlikely, we believe that this condition is not unrealistic. In a recent study, some of the authors have analyzed environmental conditions from a European site (Papi, 2022), which is known to have severe met-ocean conditions. We have found the 50-year extreme significant wave height at the site through statistical extrapolation techniques to be in the order of 8.5 meters at 5 m/s. The highest 10% of waves is generally 30% higher than the significant wave height and maximum wave height can be up to double the significant wave height. We have changed the text to reflect this in lines 495-503.
The BEM model we used in this study includes structural velocity in the momentum balance (to compute the thrust coefficient) and Buhl's (Buhl, 2005) implementation of the Glauert correction. This method has shown excellent agreement to the higher order theories in terms of its capability to predict global rotor forces (Fig. 11), proving that this approach, although theoretically incomplete, is still a

viable industrial tool. On the other hand, if we look at blade loads, especially for blade #1 that experiences the largest degree of relative inflow variation, BEM falls somewhat short of the higher order theories. This most likely indicates that there is some room to improve these models. It is possible that the effect of the blades moving through a varying induced velocity field, that the Reviewer brings up in the following point, may be relevant here. This study is already quite long, and we do not think it is the appropriate place to attempt to develop and test a new BEM formulation, but we have added a discussion in this regard based on the points raised by the reviewer in lines 543-548.

It is well known that BEM becomes inaccurate for large blade deflections for a bottom fixed wind turbine. This is because the blade elements are moved away from the rotor plane where the induction is computed combined with a strong streamwise gradient of the induced velocity near the rotor. This effect can become very severe in the case of a floating wind turbine where the position of the rotor plane is continuously moving along the wind direction and how to possibly treat this in a dynamic BEM code should also be discussed in a paper like this. The question is whether the induced velocity field follows the oscillating FOWT or the rotor moves in a velocity gradient fixed in space. This effect is very important and depends on the time constants of the rotor oscillation compared to the inertia (time constants) of the flow as also discussed in the paper by J.B. de Vaal et al.

We wish to thank the Reviewer for pointing this out. In the present AeroDyn, the momentum balance is performed separately on each blade element, in the blade-element reference system. Therefore, the streamtube effectively follows the blade position. As such, the blades do not move in a velocity gradient field that is fixed in space. The LLFVW and in the ALM models however are able to capture this effect in as much as it occurs. It is important to keep in mind (and the Reviewer is certainly aware) that, as stated in section 2.1, there is effectively no such thing as "rotor plane" or "streamtube" in AeroDyn, which is only loosely based on BEM as a theory and, in our opinion, fully qualifies as an Engineering Model. What we find interesting is that this implementation, despite being applied in conditions in which it should be invalid, works well in practice. This being said, we do believe the Reviewer has a point: if the oscillation frequency is high enough respect to the timescales and inertia of the flow, the wake will likely not have enough time to react and thus the rotor will effectively be moving in a velocity gradient fixed in space. We have included this consideration in the introduction in lines 69-72. In Fig. 4, we have shown results for varying oscillation frequencies. From a global rotor load perspective, we do not see the differences between BEM and higher order models increase significantly until we reach 8 Hz, at which point the returning wake effect becomes important. This is because, despite the timescale of the flow being around 0.6 s, the tested amplitudes are too small to appreciate this effect (0.33% of rotor diameter). In Fig. 12, however, this may play a role. We have included this in the discussion in lines 547-548.

**References**

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