Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2019-64-AC1, 2020

Interactive comment on "Investigations of aerodynamic drag forces during structural blade testing using high fidelity fluid-structure interaction" *by* Christian Grinderslev et al.

Christian Grinderslev et al.

cgrinde@dtu.dk

Received and published: 17 March 2020

The authors would like to thank the reviewers for reading this article and providing positive feedback. A special thanks to Leonardo Bergami, who even without being assigned referee, took time to provide very helpful suggestions, which will improve the paper.

Reviewer 1 - Peter Greaves

I think this is a really great piece of work, and seems to settle a question which we've had here at ORE Catapult for a while, about whether the blockage effects the drag. It also confirms for me that the effective drag is much higher than steady state conditions (which was known already but it is good to see some work from real CFD experts that confirms this). I am a complete CFD novice (taught entirely by YouTube) but I have attached a pdf on some more recent work I've done with CFD to try to bottom out the influence of aerodynamic drag. The results will not be particularly reliable (because of my lack of expertise!) but I think the trends are worth further investigation. We have never published this work, but if you want to use it then please feel free. Hopefully we will get it into a paper at some point in the future so it can be properly cited. Once again, well done on a very good piece of work. I am very confident is a significant contribution to our very niche field! Please also note the supplement to this comment: https://www.wind-energ-sci-discuss.net/wes-2019-64/wes-2019-64-RC1-supplement.pdf

Thank you for your positive feedback. It is nice to see that the study is deemed relevant in the field. Additionally, thank you for the attached document which support our findings in the article.

Reviewer 2 - Anonymous Referee

The paper is well written and well structured. The results are presented in a precise way.

Thank you for reading the article and giving positive feedback.

Short comment - Leonardo Bergami

Dear Authors, thank you for a very interesting paper. Two (plus one), very brief comments/questions for your consideration:

1. Structural Damping Role. I could not find in the paper any mention to the role of structural damping modelling. I would guess that through the coupling with Hawc2 a Rayleigh structural damping model is included, and this would also contribute to a decay in the oscillation amplitude, similarly to aerodynamic damping effects. Structural damping values are typically rather uncertain input parameters in the model, and either from simulation and measurements, it might be difficult to split the damping contribution between structural and aerodynamic sources. What are your thoughts on this, how sensitive are the simulation results to the structural damping choices, and what and how is the structural damping level chosen?

The damping was tuned through Rayleigh damping in HAWC2 to 1% log dec for the first and second modes, as stated in section 2.4. This is a value conservatively based on the reference (Post, 2016). As mentioned in the comment, it is difficult to assess this value as it cannot be directly separated from the aerodynamic damping in experiments. To assess the sensitivity, HAWC2 simulations were conducted using structural damping of 0,1,2 and 3% log. dec. For the test, a CD of 5.3 was used. Along with this, a test without aerodynamic forces (CD=0) and 1% log. dec. was conducted to show the impact of aerodynamic forces compared to the structural damping. The resulting tip displacement can be seen in the Figure 1. As seen, the structural damping has little effect on the overall decay of the tip displacement. This study will be added to the appendix of the manuscript.

2. Effects of aerodynamics on eigen-frequency estimation. Did you quantify any eventual bias of the blade eigen-frequency response due to the aerodynamic forces interaction? The increased aerodynamic drag might cause an increased "(aerodynamic) added mass" effect, which could potentially lower the frequency of the blade response

(being now, not completely free, but subject to aerodynamic forces, higher than predicted by BEM). It could be interesting to show in the paper how much much (if any) the response frequency is shifted in the FSI response compared to the "structural-only" eigen frequency solution.

No significant change in frequency due to the added mass was observed through the simulations. Likely, this possible effect will also change along the decaying vibration of the blade, as is seen with the effective drag. For oscillations with a constant amplitude, this effect might be more prominent.

3. (very minor terminology clarification) I would guess that since the blade is nonrotating, there is actually no induction modelling active in the FSI-BEM model (correctly). Some may argue that in this case, it wouldn't be a "Blade Element Momentum" model, as 1D momentum theory and induction modeling are not used. Anyway, just a matter of naming.

This is a good point. The "BEM" notation will be changed to "BET" as Blade Element Theory, which should be more accurate.

Thank you for your work, best regards. Leonardo

Thank you very much for your nice comments and suggestions. They will definitely improve the article.

Printer-friendly version

Discussion paper

Changes to manuscript

The sensitivity study of structural damping has been added to Appendix B, and is referred to in section "2.4 Structural dynamics"

Notation of "BEM" (blade element momentum) has been changed to "BET" (blade element theory) throughout the article.

Interactive comment on Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2019-64, 2019.

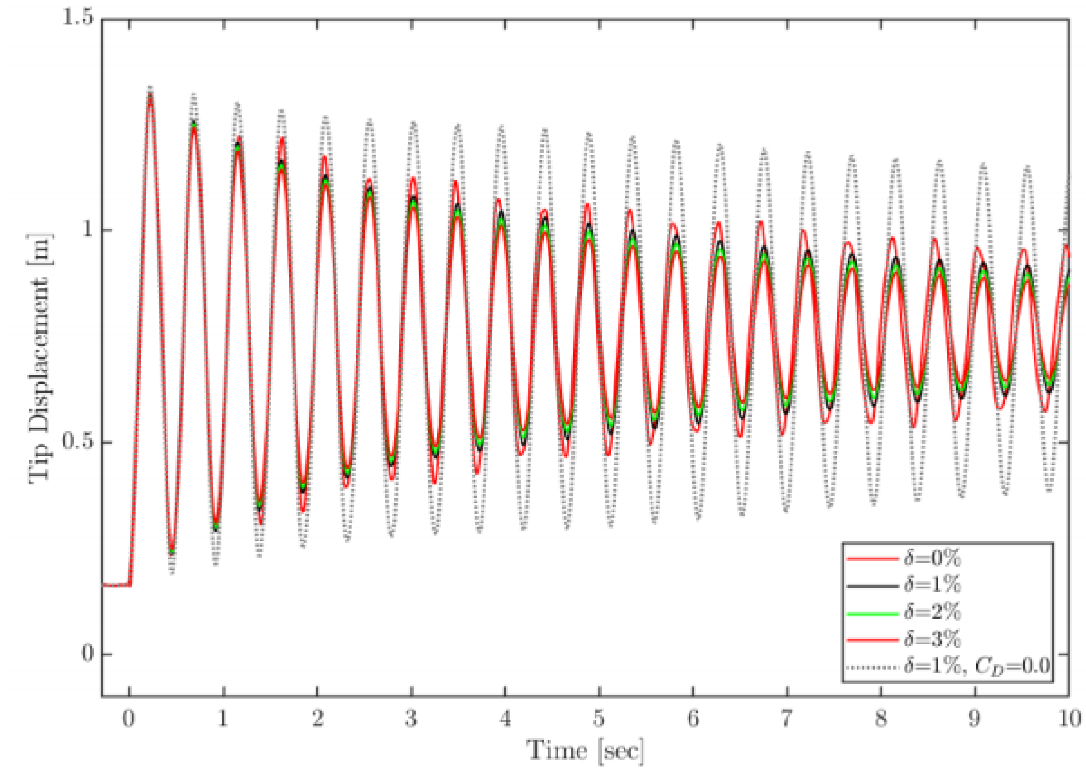

Fig. 1.

Printer-friendly version

Discussion paper