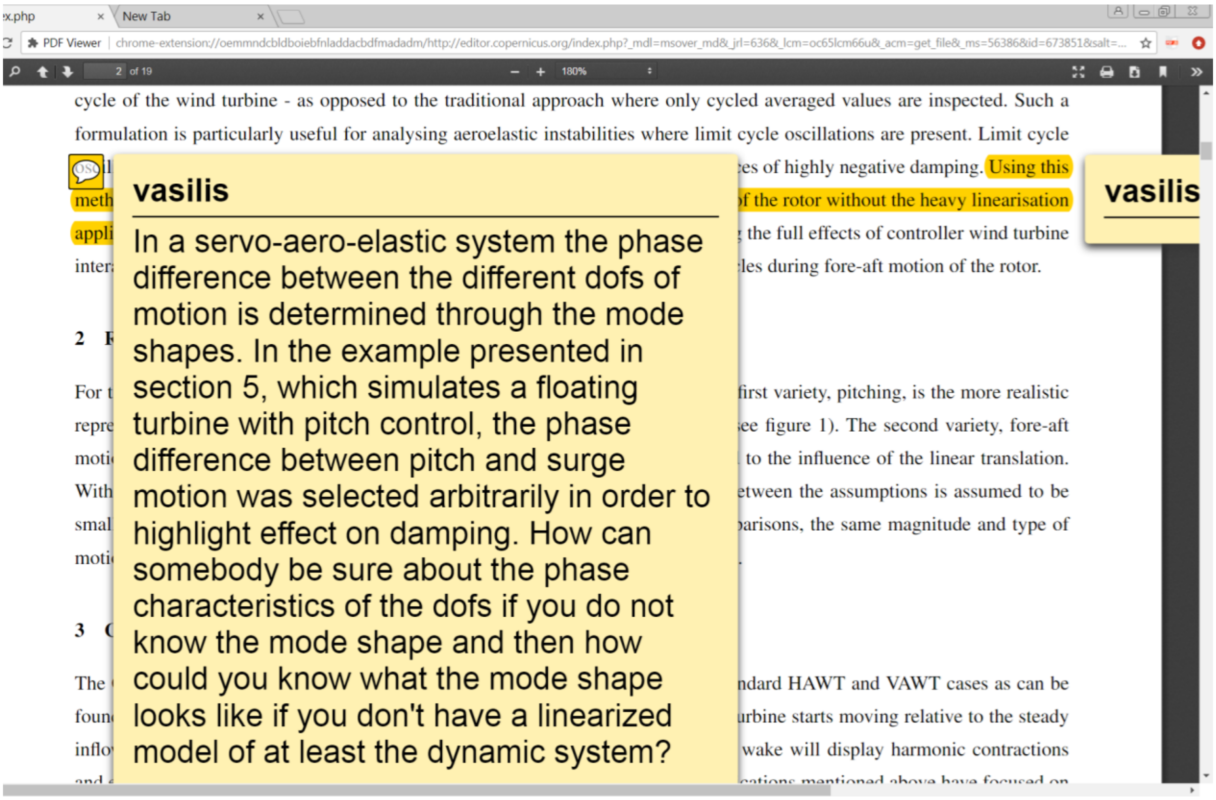
Comments for Reviewer 1:

Thank you for the detailed review. It has been difficult to find people to look into the mathematics in detail. I have tried my best to answer your concerns though I am concerned that the written response might not be enough.

I have first gone through the comments on the paper and then responded to your 3 points.

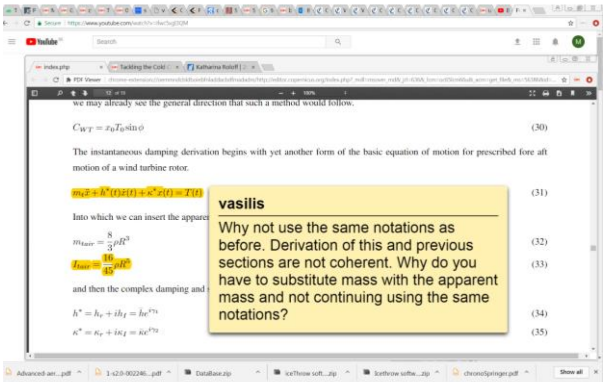
In the case of typos I have corrected without comment. i.e. a missing negative sign or missing -ve infinity.



In this approach, I am prescribing motion in surge direction in this formulation. It is not the same approach as described in Advanced aeroelastic modelling of complete wind turbine configurations. The method is described in detail for the Airfoil undergoing pitching motion by Corke and Bowles. The method derived here is based on theirs.

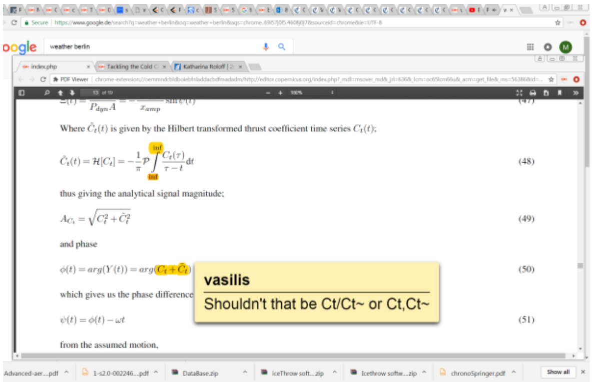
The unfortunate part of the instantaneous damping approach is that you have to re-derive the damping equations for every DOF you want to investigate.

I have clarified in the text to only mention „linearization of the aerodynamics“.

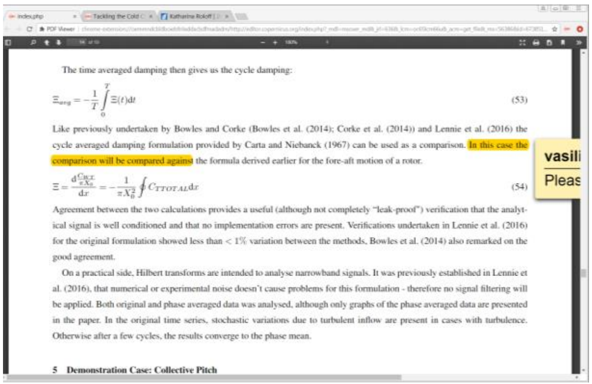


I have consistently used the K^* notation I thought? I checked and found no inconsistency. I used it as a complex variable on two occasions.

The equation appears to represent a mechanical system. It is the aerodynamic system simply using the mechancial analogy of aerodynamic stiffness and damping.



The arg (argument) operator takes care of that.



It was for this verification step among other reasons that I included the previous laborious derivations.

1) I accept your point regarding the derivation. I would like, however, to keep it there. When I first attempted to understand the derivation from Bowles and Corke, it was almost impossible to track what they did until I dug out the old papers. I believe it to be valuable to have all of the derivations

laid out as a solid foundation for the extension that I made, it may be laborious but I think it is very difficult to understand otherwise. Finally, as the journal isn't a print journal with a page limit. I would prefer to leave it there.

However, regarding your suggestion to include another test case I have done this and there is now a new test case looking at the response to a non-harmonic pitch manoeuvre. I hope that this addresses your concern about the 30% extra content.

2) Regarding equations 46-47. I believe the discrepancy comes about because the system I am modelling is not a free response system after initial excitation (which is what I believe your method was?). In this method, the Hilbert transform and analytical system are used to estimate the instantaneous phase between the prescribed motion and the response it produces (in this derivation Thrust force on the rotor). $\xi$is in fact not periodic because the variables inside the sin function represent the difference between the phase of the prescribed motion and the response again ($\phi(t) - \omega t$). As we are expecting the thrust force to only drag behind the platform translation by a reasonable amount, we can see that $\sin(\phi(t) - \omega t)$ won't be periodic.

3) Regarding the necessity to linearize. Again the prescribed motion means that we are not looking at the structure in any modal sense. One weakness of the current derivation is that I am assuming platform translation as the DOF movement of interest. That doesn't then rely on a linearization but an assumption of a different kind.

If I was to extend this model into modal coordinates then it would be necessary to linearize first. I would then have to perform the modal transform on the aerodynamic terms and that would require the prior knowledge of the modes.

Comments for Reviewer Two:

Thank you for the review.

I have scaled back the excellent agreement.

In absolute terms the spread between the different methods remains about the same. It depends on which part of the cycle you inspect, at the peaks of the harmonic the discrepancies are slightly larger though the effect is minimal.