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.

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cycle of the wind turbine - as opposed to the traditional approach where only cycled averaged values are inspected. Such a				
	formu	lation is particularly useful for analysing aeroelastic instabilities where limit	cycle oscillations are present. Limit cycle	
	<u></u>	vasilis	tes of highly negative damping. Using this	vacilie
	meth		of the rotor without the heavy linearisation	vasilis
	appli	In a servo-aero-elastic system the phase	the full effects of controller wind turbine	-
	inter	difference between the different dofs of	eles during fore-aft motion of the rotor.	
		motion is determined through the mode		
	2 F	shapes. In the example presented in		
	Fort	section 5 which simulates a floating	first variety nitching is the more realistic	
	FOL	turbine with nitch control, the phase	as figure 1). The second variety fore off	
	moti	difference between niteb and ourse	to the influence of the linear translation	
	With	unerence between pitch and surge	to the initial of the inear translation.	
	smal	motion was selected arbitrarily in order to	barisons, the same magnitude and tune of	
	smar	highlight effect on damping. How can	barisons, the same magnitude and type of	
	moti	somebody be sure about the phase		
		characteristics of the dofs if you do not		
	3 (	know the mode shape and then how		
	The	could you know what the mode shape	ndard HAWT and VAWT cases as can be	
	found	looks like if you don't have a linearized	urbine starts moving relative to the steady	
	inflo	model of at least the dynamic system?	wake will display harmonic contractions	
	and	model of at least the dynamic system?	entione mentioned above have focused on	

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.