We would like to thank the reviewer for his constructive comments and recommendations. We have tried to answer to comments below. We have also tried to provide details on the changes we are going to make to the revised text.

Reviewer 2

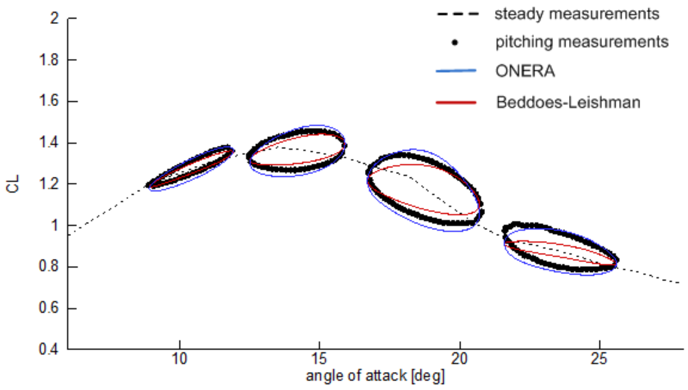
1) Since the paper is submitted for the "Special Issue – TORQUE 2016" on WES, I think that the related TORQUE paper should be referenced within the text along with a suitable description of the differences/improvements between the two articles. In the current state of the manuscript, the TORQUE paper is not mentioned nor is the innovative content of WES paper with respect to the TORQUE one clearly explained. This said, it is important to stress that the entire section 3 ("description of tools"), although new with respect to TORQUE manuscript, describes a standard multi-body code, hence it cannot be strictly considered as innovative. Moreover, in section 4 (Results and discussion) there are more or less the very same results already inserted into the TORQUE paper. Even though the work is clearly interesting and well done, the authors should better describe its innovative content with respect to their TORQUE publication, especially in terms of methodology/results. The comparison between steady and dynamic aerodynamics can be a good point.

We agree with Reviewer's comment that reference to Torque paper should be made in the revised text. Based as well on first Reviewer's comments we have decided to significantly reduce the size of the structural model description which according to Reviewer 2 is not innovative (and we agree on that). We are also planning to extend description of the unsteady aerodynamic model (ONERA model) which will be supported by some validation cases (see below and also answers to first reviewer). In the validation cases we will compare predictions of ONERA model against Beddoes-Leishman predictions and against measured data. Also work analysis of section 4.3 will be extended with additional Beddoes-Leishman results. Work computations will be also performed using unsteady loops predicted with Beddoes-Leishman model. In this way predictions of the two most popular dynamic stall models will be compared and capabilities and limitations of both models will be pointed out. Also possible differences of work predictions in deep stall will be assessed.

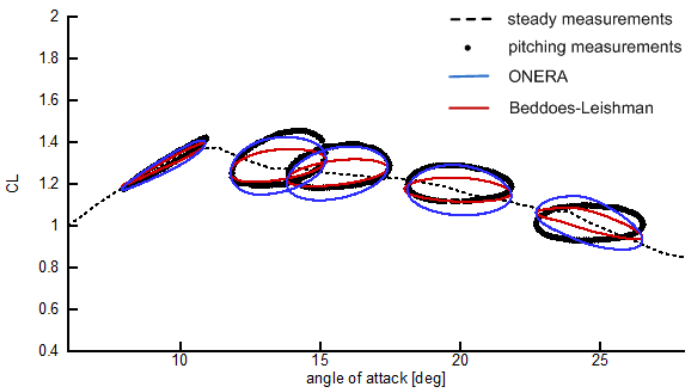
Moreover, innovative contributions with respect to Torque paper will be discussed in the introduction section and these will be:
1) Literature review of major contributions to the topic
2) Detailed description of ONERA model
3) Validation of ONERA model in light and deep stall conditions (relevant to the conditions encountered at standstill)
4) Additional work computations based on Beddoes-Leishman model and steady state aerodynamics

2) As said previously, Section 3 seems to describe a standard multibody solver. If so, probably this section can be removed or better simplified. This can be useful for example to insert in the manuscript a brief description of the ONERA stall model which may help readers comprehend the obtained results.

As already stated the description of the structural part will be significantly reduced to about 1 page length.
Moreover detailed description of ONERA model will be included in the revised text. In addition we are going to add validation results against tunnel measurements for pitching airfoils at high angles of attack (see results in the following figures) that indicate performance capabilities and limitations of the two models (ONERA and Beddoes-Leishman). To our opinion this is also relevant to the discussion that follows.



Results for pitching NACA 63415. Measurements from reference Bak, C., Fuglsang, P., Johansen, J, Antoniou, I., "Wind Tunnel Tests of the NACA 63-415 and a Modified NACA 63-415 Airfoil," Risø-R-1193(EN), Risø National Laboratory, Roskilde, Denmark, December 2000



Results for pitching FFA-W3-241. Measurements from reference Fuglsang, P., Antoniou, I., Dahl, K.S., Madsen, H.A., "Wind Tunnel Tests of the FFA-W3-241, FFA-W3-301 and NACA 63430 airfoils," Risø-R-1041(EN), Risø National Laboratory, Roskilde, Denmark, December 1998

Although the above tests are relatively old (about 20 years old) they are well suited for the purposes of the present study because they concern operation within stall regime. Usually, dynamic stall tests run from fully attached to fully separated flow conditions over one period of oscillation. This is not representative of the dynamic stall conditions encountered by an idling blade as explained in the paper. The idling blades experience high AoA variations over the period of their rotation (low frequency variations) but small amplitude oscillations over periods that correspond to their natural frequencies. So the above tests, that all concern low amplitude oscillations, match very well the conditions encountered by the idling blade and the simulations performed under work analysis section. One issue could be that the above tests correspond to a pitching and not to a combined heave/translation airfoil motion as one would prefer to have. However, to our knowledge there are no similar tests for plunging/translating airfoils.

3) Pag 3, line 25 to end of page. Clearly, for anisotropic rotors, stability analyses based on the Coleman transformation does not provide exact solutions. But it is important to stress that it keep giving good approximations as some papers underlined (e.g. Skjoldan Hansen, 2009, referenced in the present paper). Here, what the authors have applied is the so-called "Time-freezing stability" (fix the rotor at a specific time instant (or even azimuth angle), compute the linearized model and in turn the frequencies and damping factors). For a generic periodic system, results obtained with this procedure are typically not accurate. I would say that between time freezing-based and Coleman-based analyses, the latter is to be preferred. However, given the very low rotor speed, the time-freezing analysis may give meaningful results, but this is an open point. The authors should clarify this aspect also because, as *highlighted in the results, some modes may have negative damping in an azimuthal range and positive in another. In this case, how is the resulting behavior? Damped or undamped?*

Skjoldan and Hansen performed Floquet analysis on systems with "moderate" periodicity such as rotors with mass imbalance and rotors operating in shear flows. They actually performed Floquet analysis on the Coleman transformed system (which is periodic in case of anisotropic rotors) and finally came to the conclusion that anisotropy does not substantially affect damping characteristics of the above systems. This could imply (but it is not recommended by the authors) that standard eigenvalue analysis could be instead performed on the Coleman transformed system for some "averaged" over one revolution coefficients. However, if this could be done, it would be only in cases of "moderate" periodicity. To the authors opinion extending this approach to cases of 30 deg or even 60 deg yaw angles would be very questionable. The above discussion will be added to the introduction section as an explanation of the reasons why time freezing approach is preferred over the eigenvalue analysis on the "averaged" Coleman transformed system.

On the other hand reviewer is right that the proposed approach cannot always give a definite answer to the question damped or undamped? Such a case of course is when a mode is positively damped in one part of the revolution and negatively damped in another, as also described by the reviewer.
However, such an answer is also difficult to be provided even through Floquet analysis (or any other type of analysis), given the strong nonlinearity of the aerodynamics in dynamic stall and most importantly the effect that turbulent wind has on the behavior of the system. It is noted that turbulent wind cannot be easily accounted for in any stability analyses. An example of the above mentioned uncertainties is already given in the paper. Under uniform inflow conditions (wind speed 42.5m/s and yaw 30deg) the system is unstable, see figure 19 where vibrations continuously grow with time (they lead to divergence after a certain time). This is in agreement with the eigenvalue analysis results which predict that M7 remains negatively damped throughout the revolution. Also, the frequency of the predicted negatively damped mode M7 agrees with the PSD of the edgewise moment which exhibits a peak at the frequency of 0.8 Hz. For the same case and for turbulent inflow conditions vibrations do no lead to critical instabilities of the system. Amplifying edgewise vibrations are observed in part of the time series of figure 10 which after some time they decay. This is because, as a result of the turbulent wind, operation moves continuously in and out the negative CL slope region as indicated in the CL-AoA loops of figure 11.
So, as discussed in the conclusions section, the eigenvalue tool is not considered as a standalone tool for assessing stability behavior of the idling turbine. It should be used in conjunction with nonlinear tools. The aim of the proposed computational environment is to identify conditions that favor instabilities which could be the starting point for improving blade structural dynamics or/and aerodynamics.
Moreover, through the comparison of the eigenvalue analysis results against time domain results the authors believe that it has been proven that the "freezing" approach can provide meaningful results in cases of low idling speeds.

Part of the above discussion will be included in the conclusions section.

4) According to the eigenvalue analyses, mode M7 is unstable as it has negative damping for each azimuth angle. From Figure 19, however, it seems that the system is stable, as there is no divergent behavior in the signal. Of course, the vibrational content is amplified because of stall and unsteady aerodynamics, but the system itself is stable. Please, clarify this point.

To the authors opinion there is a clear divergent behavior. Perhaps the time scale of the plot is misleading but to the authors opinion it can be clearly seen even with this time scale that in every revolution, amplifying vibrations (due to operation in stall) reach higher amplitude. This can be seen in the repeating patterns that every time they re-appear with increasing amplitude.

5) Section 4.3 (Work computation results) is really important for the completeness of the manuscript. I like it a lot for two main reasons. First it gives an alternative way of verifying the stability and second (but probably more important) because, being the wind turbine model with the unsteady aerodynamics fully nonlinear, the computation of the work results to be a methodology more *appropriate to study the system stability. To this end, this section can be extended a bit. For example, the vibrations experienced in steady inflow at 42.5m/s and yaw 30 deg (see figure 19) can be imposed to the blade and the work can be computed within a rotor revolution to consider the system periodicity. This seems more significant than imposing the motion obtained from the time-freezing eigenvalue analysis.*

As discussed earlier we are planning to extend this part by including work predictions obtained also using Beddoes-Leishman model (see response to reviewer 1).
We cannot easily see the point of calculating the work of the aerodynamic forces by imposing the overall motion obtained through nonlinear analysis. This will be clearly negative as overall a divergent behavior is computed by the non linear tool (unless this was not clear before the previous comment was answered). Moreover, it will be positive if the system exhibits a decaying response.
What is important about work computations with imposed motion following the shape of the mode is that they provide information at the level of the mode similar to the eigenvalue stability analysis. So, it can be directly compared to the eigenvalue analysis with the advantage that aerodynamics is introduced in a non-linear manner. So, only structural dynamics are considered linear (through modal representation).

Minor comments:
• Pag 4, line 15: How did you compute the eigenvalues displayed in Figure 1?

Eigenvalues and mode shapes have been computed with hGAST. Reviewer is right that reference is missing. It will be added.

• Figure 2, 3 and 4: is it possible to plot the underformed configuration with a solid line in order to ease the comprehension of the modal shapes?

We will update the plots.

• Pag 16, line 9: Why are damping factors and frequency independent of the azimuth? Is the gravity considered in the model? In fact, one may expect different frequency depending of the position of the blade induced by the azimuthal dependency of gravitational loads (i.e. the blade, when upward, is compressed by its own weight, hence its stiffness lowers. The opposite when the blade is downward)

Gravity is not included in the analysis. It will be pointed out in the description of the model. Reviewer is right that some variation of the frequency would be expected as a result of the effect of gravity. It is not clear that such an effect would be visible with the scale used in the figure but as already said it is not taken into account anyway.

• Figure 24. Is the azimuth angle assumed fixed? Why not using the simulated outputs? (see also point 5 of major comments).

Yes the azimuth angle is assumed fixed. See answer above.

• Pag 25, lines 18, 19: "Blade's 1 motion" should be substituted with "blade 1 motion" or "motion of blade 1"
• Pag 28, line 5: Substitute "Eigen value" with "eigenvalue"

We will consider both editorial comments.