We would like to thank Morten for his detailed review of the paper and his constructive comments and recommendations. We have tried to answer to all comments below. We have also tried to provide details on the changes we have made to the revised text.

Reviewer 1 (Morten Hansen)

Well written introduction to the area.

Thanks.

a. The 10MW reference turbine was created in a national project called "Light Rotor" in the beginning of the InnWind project where it was also adopted. I suggest that you just omit and maybe refer to DTU Wind Energy instead.

As proposed, reference to Innwind.EU has been omitted and instead reference to DTU Wind Energy has been added.

b. In line 25 page 3, you write that the Coleman transformation approach will not work for periodic systems, where I assume that you refer to the yaw misalignment you mention in the sentence before this one. It could be a little clearer.

Sentence has been re-phrased as follows,
"This is because in yawed flows, periodicity on aerodynamic loads, introduced as a result of non axi-symmetric inflow conditions, cannot be eliminated through application of Coleman's multi-blade transformation."

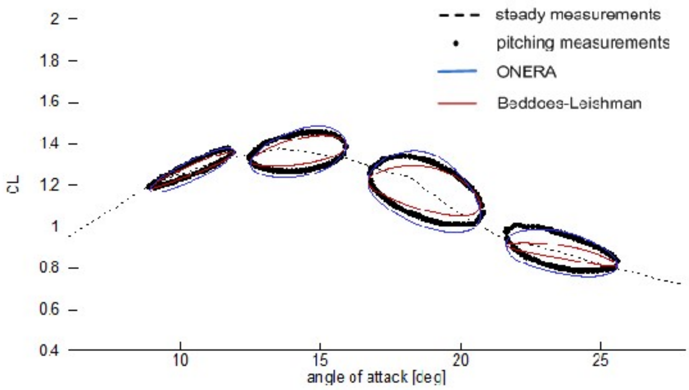
2. Good description of the effects of pitching on the mode shapes, and of the velocity triangle of an slowly idling airfoil in yaw and tilt flow at different azimuth angles.

Thanks.

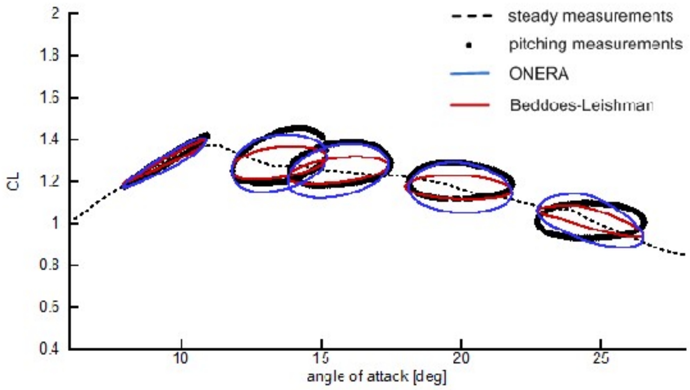
3. In this section, the tools are described. I found the first part about the structural modelling quite lengthy and I am not sure (at least at the time of reading it) if it is needed for the understanding of the aeroelastic stability of an idling turbine. The description of the unsteady aerodynamic models are much more important to my opinion and here only half a page is used.

We agree with Morten's comment that the description of the structural part is lengthy, especially since it does not serve better understanding of the results that follow. So the description of the structural part has been significantly reduced in the revised text (to slightly more than a page).

A new section (3.2) containing a detailed description of ONERA model has been included in the revised text. In addition we have added 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).



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.

a. I suggest extending the aerodynamic part with information that can enable the reader to validate the results, e.g. what time constants are used in the dynamic stall part of the model, what airfoil data is used in the analysis, does the model work well in negative stall? Later in the results section you write "Certain engineering dynamic stall models (e.g. Beddoes-Leishman) automatically switch to almost steady-state aerodynamics at very high AOA (well beyond CLmax AOA) (see Hansen et al, 2004) while ONERA model is fully deployed at all AOA. Therefore, an analysis using steady state polars is meaningful because it provides the range of anticipated damping predictions among different models." This paragraph should be moved to the model section and you should elaborate on the ONERA model "is fully deployed at all AOA. What is meant by this comment? Does it include a valid *model for deep stall? It would also strengthen the paper to show selected lift and drag loops for selected stall-induced vibrations.*

In the description of the ONERA model information such as constants used in the dynamic stall part of the model have been included. It is also explained that by its definition the model can work equally well at negative stall conditions. The model has already been tested in VAWT applications and for low tip speed ratio values and predictions have been found to be in good agreement with measurements (see Shi, L., Riziotis, V.A., Voutsinas, S.G, Wang, J., (2014),"A consistent vortex model for the aerodynamic analysis of vertical axis wind turbines," Journal Wind Engineering and Industrial Aerodynamics 135 (2014) 57-69)

The paragraph "Certain engineering dynamic stall models (e.g. Beddoes-Leishman) automatically switch to almost steady-state aerodynamics at very high AOA (well beyond CLmax AOA) (see Hansen et al, 2004) while ONERA model is fully deployed at all AOA. Therefore, an analysis using steady state polars is meaningful because it provides the range of anticipated damping predictions among different models." has been moved to the model description part and it has been discussed in conjunction with the results of ONERA and Beddoes model for the above pitching cases.

We are not suggesting that ONERA model is fully validated in deep stall conditions. Apparently no dynamic stall model exists that it is thoroughly validated in deep stall. This has been made explicit in the revised text, not only in the introduction section where it is already discussed,

"The analysis is confined to yaw angles within the range [-60^{o}, +60^{o}]. This is the absolute upper limit up to which engineering dynamic stall models can be trusted. Outside this range deep stall conditions are encountered that cannot be properly addressed by engineering aerodynamic models. This is because engineering models lack the appropriate tuning in such deep stall conditions."

but also in the ONERA model description section where the following comment has been added

"Despite the reasonable agreement obtained in the above presented cases it cannot be easily supported that ONERA model is fully validated in deep stall conditions. Apparently no dynamic stall model yet exists that it is thoroughly validated in deep stall and this is mainly because of the lack of relevant measured data of combined heaving-translation motion that could serve tuning purposes."

As discussed above pitching loops have been included as validation cases.

b. I also suggest limiting the structural part.

It has been done.

4. Interesting results. I have several comments:
a. The quality of the plots and captions could be improved. In Figure 9 and Figure 12, the legends are not describing all plotted curves. It is not clear from the caption of Figure 11 what the dots are representing. The low damping of flapwise modes in Figure 13b is very difficult to see.

The quality of the above figures has been improved.

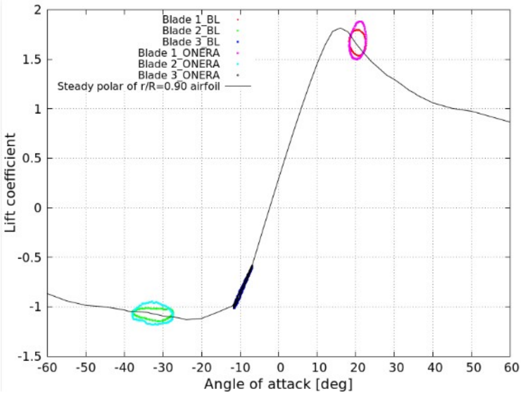
b. It is clear that the understanding of the ONERA model is very important and I strongly suggest that the model section is extended.

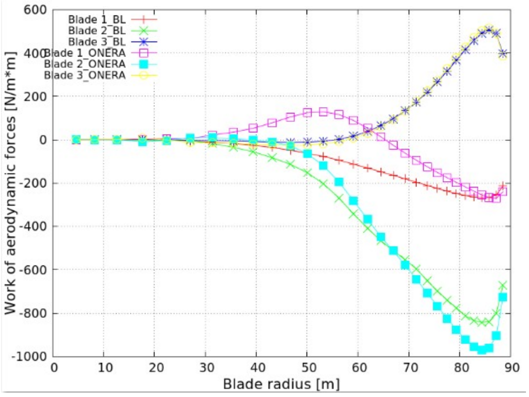
This has been done.

c. The very strange looking red lift loops in Figure 25 and 26 seem to indicate a deficiency of the ONERA model. They look strange because they are almost perpendicular to the static curve. I could not find a comment on these strange loops from the authors and I strongly suggest that you explain them based on a better presentation of the ONERA model in the model section.

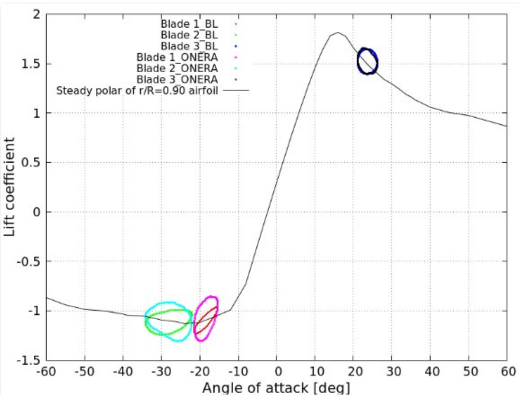
We have included loops and work results that have been also predicted by Beddoes-Leishman model. Unfortunately we can only use aeroelastic modes predicted by the linearized stability tool that employs ONERA model (we do not have the linearized version of Beddoes-Leishman model). However, under the assumption that aeroelastic mode shapes are not considerably affected by different dynamic stall models we have provided a cross comparison of the loops predicted by the two models and also highlighted possible differences in work results. The above exercise will hopefully add confidence to ONERA loops.

An example comparison is shown below (we haven't yet collected all results). BL stands for Beddoes-Leishman model.





Azimuth 20deg



Azimuth 100 deg

Comment: In line with the pitching cases ONERA model predicts wider loops in most cases.

At the moment we cannot prove that the loops are correct but we have given some qualitative answer to the comment,

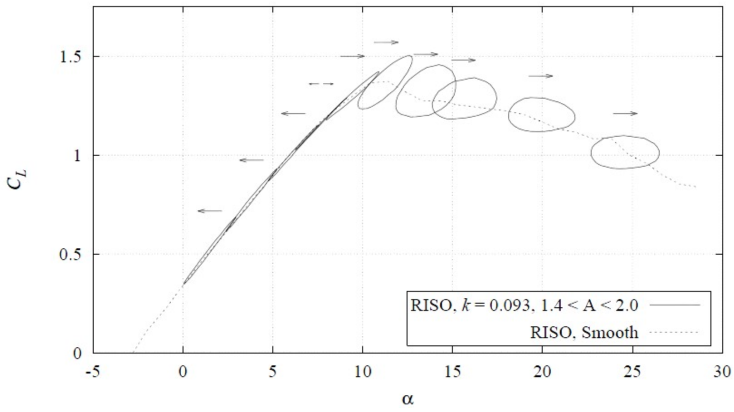
1) the shape of the loop of figure 25* (blade 1, red line) can be explained through the trace plot of figure 24*. It is seen that the corresponding modal displacement is dominated by edgewise motion with low coupling with the flapwise direction (about 20%). As a result of the low flap coupling the range of variation of the AoAs is low. The high width of the loop can be explained by the relatively high reduced frequency of the motion which is about k=0.14 for the specific case (frequency 0.8Hz, chord at 80m, 2.3m, wind velocity 42.5m/s). The above comment has been added in the revised text.

2) For the loop of figure 26* (blade 1, red line) it is typical to obtain such loops that are not following closely the steady state curve when the AoAs are lying in the CLmax/min region and for low amplitudes that are not leading to fully attached flow at any part of the oscillation. Similar results we get also with Beddoes-Leishman model as w=one can see in the above plot.

(*) Figure numbers refer to the first draft of the paper.

Typical loops from,
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,

could be an evidence. See below the two loops that lie in the vicinity of CLmax.



5. The conclusions does not clearly mention the problem with the linearization of the ONERA model which could be important.

The conclusions have been updated in order to point out ONERA linearization issues.

Editorial changes:
1. I suggest to write "nonlinear" instead of "non-linear" and "non linear".

It has been changed everywhere in the text.

2. You could consider start a new sub-section on page 5 after talking about the changes in mode shapes when pitching the blades out to feather, which similarly could have its own sub-section starting in line 13 on page 4.

It has been done in the revised text.

3. Same subsection division could be done for the tools section.

It has been done as well

4. I do not like the term "Eigenvalue stability simulations" (line 29 on page 14). Eigenvalues may say something about stability of an equilibrium but they are computed using an eigenvalue solver and not simulated.

We have revised to eigenvalue analysis

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 have made 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.

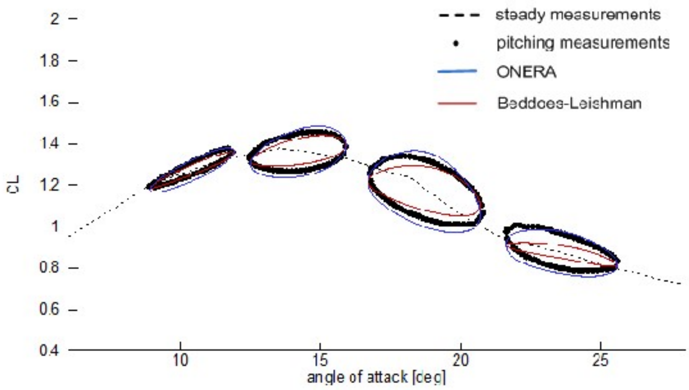
We agree with Reviewer's comment that reference to Torque paper should be made in the revised text. Based as well on first Reviewer's comments we have reduced the size of the structural model description which according to Reviewer 2 is not innovative (and we agree on that). We have also extended description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

Moreover, innovative contributions with respect to Torque paper are discussed in the introduction section where the following paragraph has been added,

"The present work is a follow up of the work presented in Wang et al (2016). The dynamic stall model used in calculating the aerodynamic loads in deep stall is critical for the assessment of the stability limits of an idling turbine. The innovative contribution of the present work in relation to the previous work presented in Wang et al (2016) is that in the present work emphasis is put in describing in detail the ONERA model employed in hGAST aeroelastic code for the prediction of the aerodynamic loads in dynamic stall (Sect. 3.2). Moreover, validation of the model in deep stall conditions is presented in the same section. Predictions of the ONERA model are compared to measured data for pitching airfoils in deep stall, well beyond the CLmax AOA, and to results of the state-of-the-art Beddoes Leishman model. Finally, work computations are performed in Sect. 4.3 using both unsteady aerodynamic models as well as steady state aerodynamics. This way, the range of anticipated damping predictions in idling operation is better explored."

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 has been significantly reduced to slightly more than 1 page length.
Moreover detailed description of ONERA model has been included in the revised text in section 3.2. In addition validation results against tunnel measurements for pitching airfoils at high angles of attack have been added (see results in the following figures) that indicate performance capabilities and limitations of the two models (ONERA and Beddoes-Leishman).



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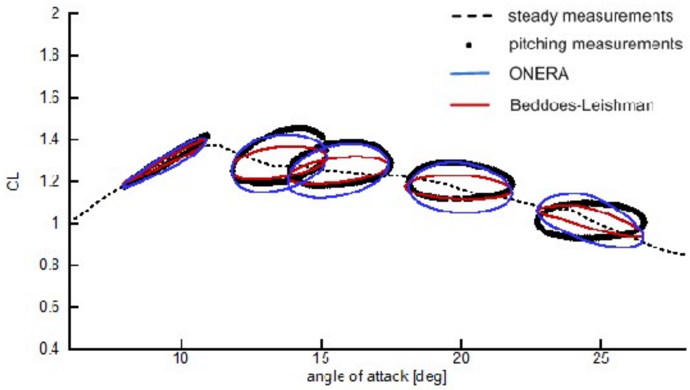

Results for pitching FFA-W3-241. Measurements from reference Fuglsang, P., Antoniou, I., Dahl, K.S., Madsen, H.A., "Wind Tunnel Tests of the FFA-W3-241, FFA-W3-301 and NACA 63430 airfoils," Risø-R-1041(EN), Risø National Laboratory, Roskilde, Denmark, December 1998

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 following comment has been added in the introduction section

"As an alternative 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 definitely questionable. On the other hand by noting that at very small rotational speeds (~1 RPM) low order harmonics (up to 6p) are not expected to interact strongly with the natural frequencies of the turbine, as a second alternative, non-rotating (static) analysis can be performed at different azimuth angles within the sector [0o, 120o]. This is the approach followed in the present work."

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.

The following comment has been added in the conclusions section.

"It is noted that the proposed eigenvalue stability analysis cannot always give a definite answer to the question whether the system is damped or undamped. A case that such an answer cannot be given is when a mode is positively damped in one part of the revolution and negatively damped in another. However, this definite answer is also difficult to be given even by Floquet analysis (or any other type of analysis). This is first because of the strong nonlinearity of the aerodynamics in dynamic stall but most importantly because of 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 uncertainty is already given in the paper. Under uniform inflow conditions (wind speed 42.5ms-1 and yaw 30o) the system is unstable, as shown in Figure 22. Vibrations continuously grow with time and 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 13 which after certain time 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 14. So, 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."

(*) Figure refer to the first draft version of the paper

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 have extended 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, reference has been 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 have updated 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 has been 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 have considered both editorial comments.