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. Answers can be also found in the accompanying pdf of the paper under review. We have also tried to provide details on the changes we are going to make 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.

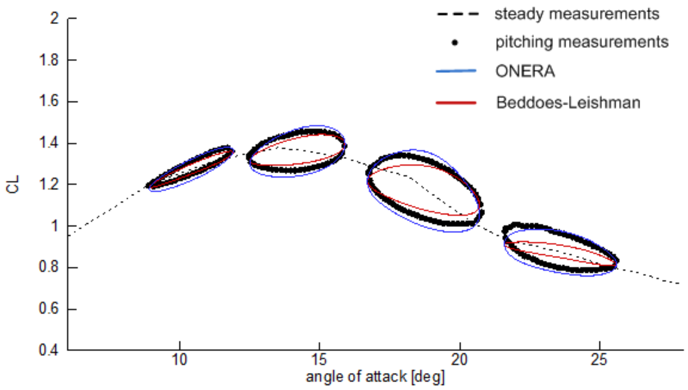
Sentence has been re-phrased as follows,
"This is because in yawed flows, periodicity on aerodynamic loads, introduced as a result of non axi-symmetric inflow conditions, cannot be eliminated through application of Coleman's multi-blade transformation."

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.

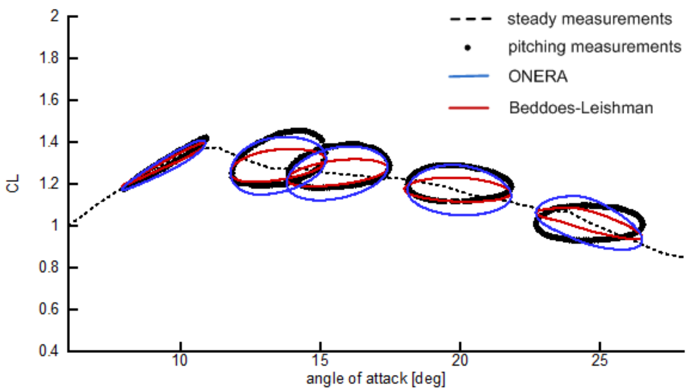
Thanks.

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 will be significantly reduced in the revised text (to about 1 page). A brief description of the ONERA model would be also sufficient given that ONERA is a well established model. Reference to some older publication, where detailed description of the model can be found (e.g. see in paper references Riziotis et al (2004)) could be enough since our original intention was not to compare ONERA against any other model but just to indicate how unsteady aerodynamics in dynamic stall conditions affect stability predictions.
However, since reference to Beddoes-Leishman model has been made at some point of the paper and especially to its feature to automatically switch off at high angles of attack we recognize that the comment is relevant and we are going to extend ONERA model description following Morten's concern. So, a 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.

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, airfoil data used in the analysis will be included. It will be 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." will be moved in the model description part and it will be 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. We will make that 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.

A fair comparison of the two models (based on the above results) shows that overall they both perform well (one does better in some cases the other does better in other, this will be detailed in the revised text) however at very high AoA Beddoes model switches off and considerably under predicts the width of the loops while ONERA continuous to provide reasonable results. The above feature of Beddoes necessitated the analysis with steady state aerodynamics in order to assess the range of the expected results.

As discussed above pitching loops will be included. We are not yet sure whether there will be space to include drag loops as well.

b. I also suggest limiting the structural part.

As already discussed this will be 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.

We agree. We will improve legends and captions in all above discussed figures.

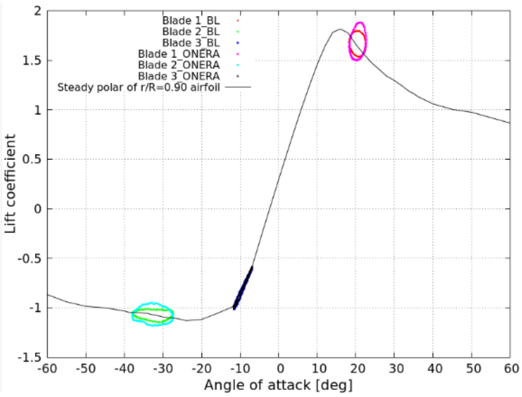
b. It is clear that the understanding of the ONERA model is very important and I strongly suggest that the model section is extended.

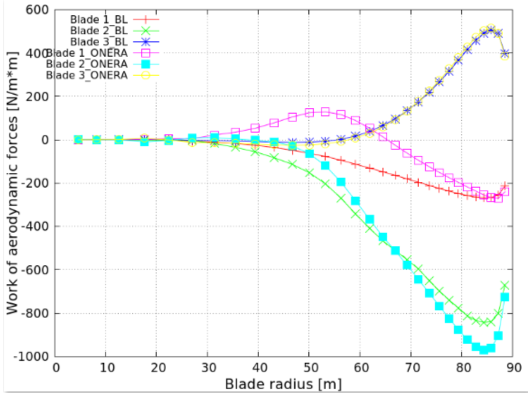
We agree. This will be done.

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.*

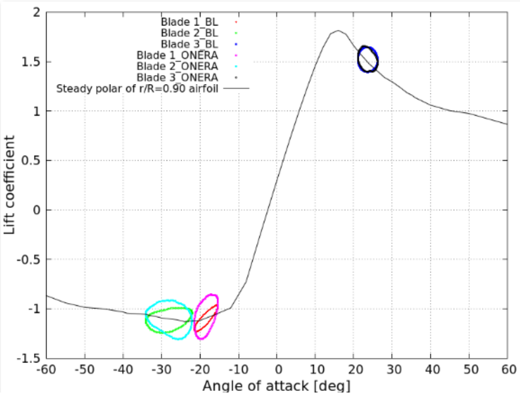
We have decided to include loops and work results that have been also predicted by Beddoes-Leishman model. Unfortunately we can only use aeroelastic modes predicted by the linearized stability tool that employs ONERA model (we do not have the linearized version of Beddoes-Leishman model). However, under the assumption that aeroelastic mode shapes are not considerably affected by different dynamic stall models we can provide a cross comparison of the loops predicted by the two models and also highlight 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

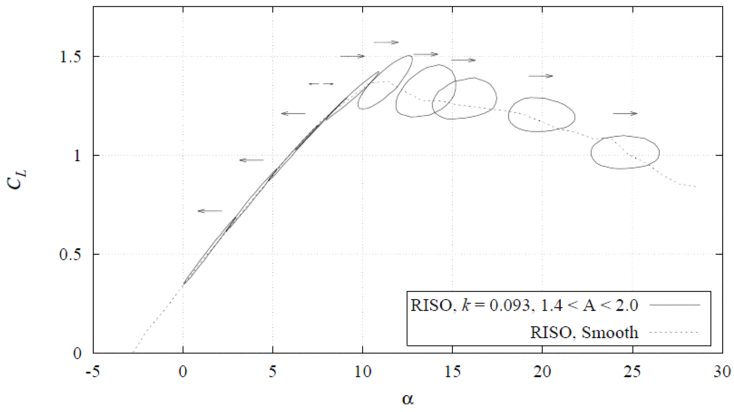
Comment: In line with the pitching cases ONERA model predicts wider loops in most cases.

At the moment we cannot prove that the loops are correct but we can give 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).

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.

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.



We can add some of the above explanations for clarity in the revised text.

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

We agree. The conclusions will be update in order to point out ONERA linearization issues.

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

It will be 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 will be done in the revised text.

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

This will be 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 agree. We could revise to eigenvalue analysis