Response to Reviews, Manuscript No. WES-2018-22

Dear associate Editor of WES, Mingming Zhang,

Thank you for sending us the two valuable reports. We would like to thank also both referees for their careful reading of our manuscript. Their positive opinion on the importance and interest of our findings in the topic of rotor blade inspection is very encouraging for us.

We have put much effort on revising and adapting the text to the comments and suggestions by the referees. We checked every unclear point addressed by the them and are now confident, that the quality of the article has been significantly improved. Thanks to their remarks, we provide now a clearer focus, motivation and discussion of our study.

Please find attached our answers to the referees as well as the revised manuscript.

Yours sincerely,

The authors

Response to First Referee

0. *This is an interesting paper. The potential application of IRT in surface (or even internal) damages of blades is significant. This study provides some good knowledge on this important issue. All comments are summarized as follows and they can be also found in the attached document.*

   We are grateful to the referee for the careful reading of our manuscript and his/her helpful comments. The referee asked us to consider a number of points for improving our work. Our response to these points and the corresponding changes in the manuscript are discussed in the following.

1. **Main comments:**

1.1. *Please clearly define the objectives and scopes of the work. Right now, the power loss due to surface damages should not be the focus of this study.*

   Thank you for your advise concerning the focus of our manuscript. We put effort into improving the clarity of our scope and motivation, emphasizing now the detection of blade surface damage by means of infrared thermography as the main goal of our work. We softened arguments concerning aerodynamics throughout the manuscript. Details are found in the responses to 'secondary comments'.

1.2. *The simulation part on rotor blades does not serve the main objective of this study and should be reconsidered and preferably a coupled aerodynamic and thermal analysis should be conducted.*

   Thank you for raising this point. We believe that in the original version of the manuscript we might have failed at explaining clearly the motivation for performing the simulations. We have improved now the article in this respect see for example top of page 4 and paragraph 1 of section 2.3.

   Our work focuses on the experimental measurements performed by means of infrared thermography (see 7th paragraph of introduction). The simulation, however, allows a better interpretation of the experimental results. The main reasons for this are the following:

   - The velocity gradients (and consequently wall shear stress gradients) resulting from a blade surface defect lead to gradients of convection thermal transport. This implies a direct connection between wall shear stress gradients and temperature gradients, and it allows a qualitative comparison of both magnitudes. It is worth to remark that our numerical investigation aims at analyzing the flow patterns behind surface defects and is not intended to compute temperature differences caused by the flow.

- The simulations allow to study the flow pattern caused by surface defects on a rotating wind turbine blade in addition to the static experimental test performed in the wind tunnel. Hence, the range of studied applications is extended thanks to the simulations.
- The simulations show that turbulent wedges caused by surface defects and observed experimentally in the laboratory over a static (quasi-) 2D-dimensional airfoil are also found on a rotating 3D wind turbine blade. This gives confidence on the potential of IRT for detecting blade surface defects on rotating wind turbines.
- The simulations show that the impact of the turbulent wedges on the rotor performance is negligible under design conditions. This allows to conclude, that IRT is capable to detect surface defects that otherwise would remain unnoticed, since they do not play a significant role for the mean power output. See last paragraph of Section 3.3.

1.3. *Some wordings are not accurate and need to be rephrased.*

We have improved the manuscript also in this respect.

2. **Secondary comments:**

2.1 *Abstract: Not clear. What kind of nonlinear interaction you are referring to?*

We are grateful for this hint and admit that our wording was vague and could be misunderstood. We have now removed the term 'nonlinear interaction', since it does not really contribute to explain our point.

For the information to the referee: we were referring to flow situations that are highly sensitive to small changes in boundary conditions. Isolated surface roughness can for example suppress laminar separation bubbles, generate stall cells and thus even change the general stall behavior (see cited Corten et al 2001).

2.2 *Too many keywords.*

We reduced the number of keywords from 10 to 5 following this suggestion.

2.3.1 *P2. line 7: This is not an accurate term.*

We now use the term 'initial defect' to be clear.

2.3.2 *P2, line 10: I can not see any correlation between the impact of surface defects on airfoil performance and the demand of a remote inspection. This study does not examine the impact of surface damages on airfoil performance?*

It is true that we did not express our point in a clear manner. Our study is a first attempt to estimate the potential of IRT for detecting surface defects. A detailed analysis of the impact of surface defects on the blade performance is out of scope of the current work. We believe, however, that in future it will be possible to estimate the severity of the impact of surface defects on blade aerodynamics by means of IRT. In any case, we show that IRT is capable to detect surface damages that do not have a clear impact on the power output.

For clarification, we restructured the text and focused on the recognition of surface imperfections using IRT. See paragraph 7 of introduction.

2.4. *Fig. 1: Please provide more information of this picture, such as how this picture is taken? What are the surface defects? What is the ambient temperature? etc.*

We agree that this information is interesting for technicians/scientists who like to apply infrared thermography. We thus now provide additional information on the experiment in the caption of Fig. 1. Concerning details on surface defects, we added to the main text on page 3 what typical surface imperfections of a rotor blade at this age consist of.

2.5.1 *P3, line 3-4: What condition? What do you mean by saying 'average'?*

Thank you for this comment. The word 'average' is indeed inaccurate. We now refer to the particular rotor blade shown in Fig. 1, which has only been in operation for about two months.

2.6. *3, line 10: This paper is weak on this part. The studies presented on rotor blades only focus on aerodynamics and there is no quantitative link between air flows and thermal footprints. In addition, the studies on power loss do not contribute to the main purpose of the work as emphasized by the title of this manuscript. Ideally, a coupled aerodynamic and thermal simulation should be performed.*

The original version of the manuscript was admittedly not clear enough in this respect. We have improved it now, i.e. paragraph 7 of introduction.

Our main intention with the simulations is to elucidate if the flow patterns observed experimentally behind surface defects on static, quasi 2-dimensional airfoils are also present on 3-dimensional, rotating blades. This is necessary for assessing the adequacy of IRT as a monitoring technique for blade surface defects. Therefore, our focus lies on the flow behavior in the wake of surface defects. This is the reason why we have chosen to perform detailed computational fluid dynamic simulations.

The thermal footprint is caused by convection thermal transport, which is directly driven by the air flow over the blade. The Reynolds analogy allows to relate the wall shear stress obtained from the simulations to the convection thermal transport that a IRT camera would capture. Both magnitudes are therefore very closely interrelated, what makes a thermodynamic simulation non-essential for our purpose.

Since the same flow patterns were observed numerically on the 3D, rotating wind turbine and experimentally (by means of IRT) on the 2D, static airfoil, we are confident that IRT has a great potential as a monitoring technique for wind turbine blades. We have improved the manuscript for explaining better this point (see Section 3.2 and conclusion).

The analysis of the rotor power loss indicates that the use of IRT is accurate enough for detecting blade surface defects that otherwise could not be easily detected, since they do not influence the power output under design inflow conditions. This can be of great importance for preventing small defects from remaining unnoticed. Therefore, we consider that the discussion of the power loss is indeed relevant for understanding the potential of IRT (although we did not explain our point clearly in the first version of the manuscript). This information is now included in the new version (see pages 15 and 16).

2.7. *Fig. 6: Please add the legend for IRT image.*

We added the legend to the figure and complemented some words in the corresponding caption as well as in the main text on page 10.

Response to Second Referee

0. *A good paper with some interesting results from IR-measurements and it is clearly shown that the method is useful in visualizing the effects of roughness on blades. However, I think you promise more in the introduction than you "deliver" in the paper.*

   We thank the referee for the interest and emphasis on the usefulness of the method we present. We agree with the referee that the introduction promises too much concerning performance. We therefore reviewed the introduction to better focus on flow visualization.

0.1. *On p.4 l.2 you mention that you can assess the effective power loss but on p.14 l.17 you state that no significant change in the performance is detected (this is probably true for the turbulators you have placed). But I think it could be interesting to see if you can predict degradation in power (e.g. by adding more turbulators at the tip part so a larger part of the blade is affected).*

   Thanks for this interesting suggestion.

   We have addressed the power loss issue in more detail now with the additional Figure 12 (see also our response to remark #12). In the new version of the manuscript, we explain why the influence of the surface defects on the power output is negligible (see first paragraph of page 16).

   Our goal in this work is to figure out if the use of IRT might help to detect surface defects that otherwise would remain "hidden" (i.e. undetectable by the power output). Therefore, for our study it is more interesting to have a rather small number of turbulators. Placing many turbulators in the tip region could be interesting for studying the influence of surface roughness on the power output. However, that analysis would outrage the scope of our article.

   We have adapted the manuscript for making this point more clear.

0.2. *You are testing at a relatively low Re-number compare to full scale. Can you comment on this?*

   In Fig. 7, we treat the effect of the Reynolds number, although we remain far below the maximum Re-numbers to be found in modern wind turbines. The flow patterns for very large Re-numbers might be admittedly slightly different. However, our main goal is just to show that IRT is capable of capturing the temperature gradients resulting from the flow disturbances caused by surface defects (the characteristics of the flow patterns being of secondary importance). Our results indicate that IRT has a great potential in this respect.

Nevertheless, further analysis for characterizing the flow patterns behind surface defects at large Reynolds numbers are certainly required and will be studied in the future, as now also suggested in the conclusion.

1. *An idea for a further study: Check the rule of thumb that a defect with a roughness height Re less than 680 doesn't disturb the flow (von Doenhoff, Albert E. and Horton, Elmer A. "A Low-Speed Experimental Investigation of the Effect of a Sandpaper type roughness on Boundary-Layer Transition", NACA report 1349, 1958).*

   That is indeed an interesting paper! Particularly that roughnesses are investigated directly on an airfoil. Unfortunately, the type of roughness they investigate differs from the single roughness elements subject of our study. However, we will keep von Doenhoff's study in mind for a follow up study, where different types of surface defects shall be investigated.

   Following Zhong 2003 (cited in manuscript), who visualized turbulent wedges by thermography and looked at their spreading angles as well as critical Reynolds numbers, we use the terms sub-/super-/critical Reynolds number introduced in Section 3.1. Zhong reports critical values from 500 to 1000. This is in accordance with our results (critical value about 1000, see Figure 6b) and the rule of thumb of von Doenhoff.

2. *p.2 line 10: It is true that a systematic approach has not been found, but there are some groups looking into this: Sandia National Laboratories (e.g. the report SAND2017-10669) or Christian Bak et al. "What is the critical height of leading edge roughness for aerodynamics?", Journal of Physics: Conference Series, vol. 753, no. 2, 2016.*

   To acknowledge the work of Ehrmann and Bak, we now cite them. After studying some of their articles, we see our work as complementary because we focus on visualization of surface roughness whereas they focus on aerodynamics.

3. *p.6 l.21: You have decided to use five turbulators, but have you checked what happens if you use more? In principle the turbulators introduce 3D flow effects and you are measuring 2D Cl and Cd, so one can argue that the flow from the turbulators should also be 2D, hence there should be much more turbulators.*

   The scope of our work is to visualize and distinguish surface roughnesses by IRT. We thus distributed turbulators in a way that turbulent wedges do not interact.

This is now clarified in the manuscript (see page 6, bottom). The aerodynamic measurements were carried out to show that even single surface roughnesses are relevant to investigate. However, the interaction of several turbulent wedges and possible non linear behaviour is for sure an interesting aspect to analyse in the future.

4. *p.7 top: Why aren't you using the same Re in IR and lift measurements? I think you need to comment on that.*

   The referee is right that investigating the same Reynolds number in both experiments is in general favorable for comparison reasons. The scope of the IRT measurements is to vary the *local* Reynolds number with respect to the turbulator to compare with results of Section 3.1. The aerodynamic measurements address reliability and relevance to wind energy given for a Reynolds number in the order of one million. The respective objectives are thus achieved by our experiments. That being said, we admit that the IRT experiments are a compromise because no higher Reynolds number can be realized in our wind tunnel due to removed walls as required for our IRT measurements.

   All in all, the experimental design could be improved but we believe to nevertheless make the point. To convey this to the reader, we say in the manuscript on page 6 line 1 that the appearance of turbulent wedges is to be compared with results of Section 2.1. We now added on page 7 top that Re in the order of one million is aimed for aerodynamic investigations.

5. *Fig 7: Could you compare to the standard turbulent-spot wedge angle?*

   Following the referee's suggestion, we estimate the spreading angle of turbulent wedges on an airfoil for the two Reynolds numbers presented in the manuscript, $Re = 300,000$ and $Re = 600,000$, in Figure 1 of this response. The previously reported 'standard spreading angle' of roughly $10°$ agrees well with the wedge of the turbulator with a height of 1.5 mm at $Re = 600,000$. In conclusion of findings of Zhong 2003 (cited in manuscript), the other turbulent wedges deviate from the value found in literature for basically three reasons:

   (i) the wedge forms three-dimensional, so the footprint of the wedge is in general smaller than the extent slightly above the surface,
   (ii) adverse pressure gradient amplifies turbulent spreading whereas favorable pressure gradient stabilizes the flow, so the opening angle continuously varies along the airfoil,
   (iii) geometrical distortion along the surface of the airfoil complicates determining the real spreading angle based on the arc length.

We agree with the referee that investigating the spreading angle is for sure a relevant topic and particularly interesting within the complex pressure environment along an airfoil. A combination of PIV and IRT could uncover fluid dynamic principles. On the other hand, we see such investigation as an independent study and out of scope of defect identification on rotor blades.

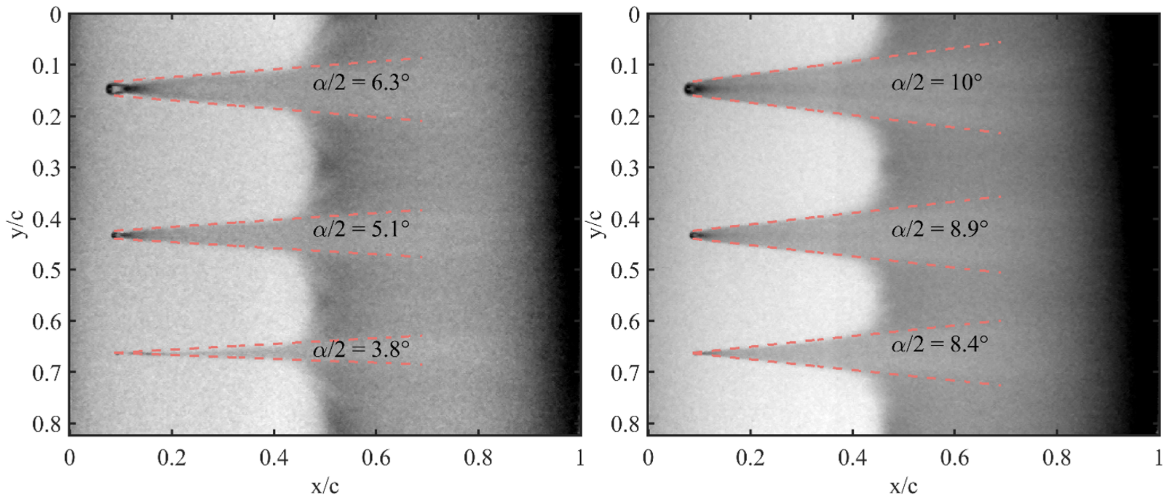

Figure 1: Estimation of spreading angles of turbulent wedges on a DU 91-W2-250 airfoil induced by turbulators ($h \in [0.5; 1; 1.5]$ mm) for $Re = 300,000$ (left) and $Re = 600,000$ (right).

6. *Fig. 8: Caption text and legend doesn't match. What is delta sigma?*

   We now explain the meaning of $\Delta\sigma$ in the caption of Figure 8 as well as in the main text on page 12 (bottom). $\Delta\sigma$ refers to the difference in standard deviation of lift/drag/moment between the clean and the modified airfoil.

7. *p.12 l.3: I am not sure that turbulence easily exceeds stall. Normally you try to avoid that by having a sufficient margin to max Cl.*

   The referee is absolutely correct that stall shall be avoided by margin to $c_{L,max}$. This margin is given by AoA of maximum L/D ratio (where the profile is operated on a wind turbine) and $c_{L,max}$. As presented first by Timmer 2003 (doi:10.1115/1.1626129) and also confirmed by our aerodynamic measurements (Figure 8 of manuscript), the DU 91-W2-250 wind profile shows maximum L/D ratio at an AoA of 8° and maximum lift at an AoA of 10°. Because of the small margin, the stall behavior of wind profiles is in general gentle (no sudden stall) but stall angle is easily reached either by a gust or vertical shear of the atmospheric boundary layer. This is confirmed by measurements carried out during the *'DAN-AERO MW Experiments'* (see Figure 20 in the final report http://orbit.dtu.dk/files/4698040/ris-r-1726.pdf), where fluctuations of the inflow angle of 4° occur in intervals of a few seconds. Therefore, we believe that particularly the post stall behavior of a wind profile is relevant with respect to fatigue.

8. *p.12 l.12: I don't think the comment about the increased fluctuations in post stall is relevant, as the airfoil will probably not operate at such high angle of attack.*

   Because this comment concerns the same point as comment # 7, i.e. fluctuations of AoA, we would like to refer to the previous reply.

9. *Fig. 9: Is it possible to include a zoom of one of the wedges so it is easier to see the vorticity direction?*

   We are including the requested figure in Fig. 2 of this document. The figure represents an iso-surface of the chordwise component of the vorticity. The two counter-rotating vortical structures described in the manuscript are here also clearly visible. However, we are not including this figure in the manuscript since we consider it a bit redundant (see also Fig. 9 and 10 of the manuscript).

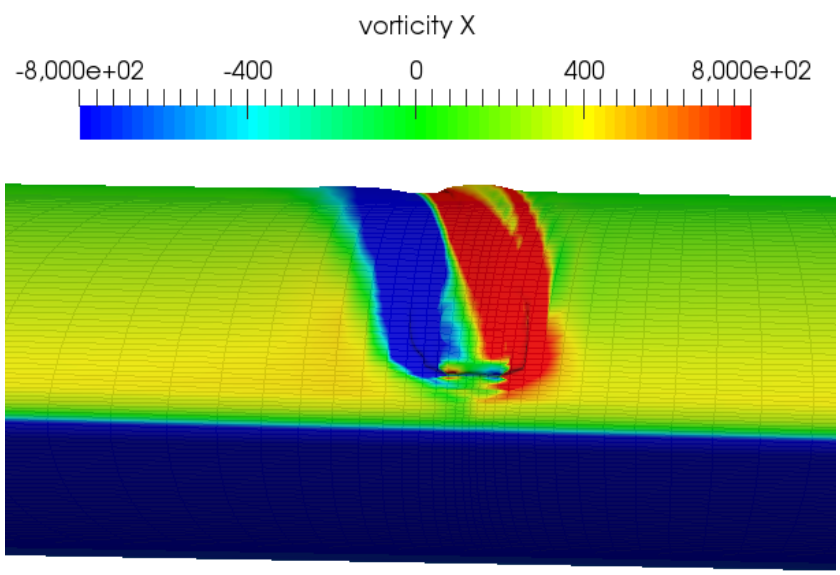

Figure 2: Iso-surface of the vorticity in the chordwise direction.

10. *p.14 l.3: You reference to Section 3.2, for the subcritical, critical and super critical*

Re numbers. But they are not defined explicitly. I think it would be good to do that, either in Sec. 3.2 or in the caption of Table 1.

We improved the manuscript according to this suggestion, i.e. we changed the wording in the main text on page 14 l. 14 and explicitly refer to the critical Re in the caption of Table 1.

11. *p.14 l.14: I don't fully understand the comment about the missing laminar to turbulent transition on the blade. Unless the Re number is very low you should always see transition on the blade. It is probably further aft because of the lower AoA and the thinner profile, but it should be there. Or is the flow tripped already at the LE (is the flow turbulent?). Comparison with Fig. 7 shows that the wedge is much less visible in the turbulent region of the flow. So Fig. 11 suggests that the transition is closer to the LE on the inner part?*

    The referee is of course right. The turbulence model that we used originally ($k - k_L - \omega$) was not providing satisfactory results with respect to the laminar-turbulent transition. We have repeated the simulations, trying to obtain better results with the same turbulence model, but we have faced again the same problem. Therefore, we have re-run once more the simulations, this time using the turbulence model $k - \omega$ SST (assuming of course fully turbulent flow (see 2nd paragraph in section 2.3 and last paragraph on page 14). All figures of the manuscript presenting numerical results have been adapted to the new results.

    Regarding the comment about the inner part of the blade, it is worth to recall that this figure represents the wall shear stress. The fact that the wedge is less visible in the inner part of the blade is just due to the fact, that the circumferential speed is lower in that blade region.

12. *p.14 l.17ff: Can you show some more data about the performance of the wind turbine with and w/o turbulators. Perhaps plot the radial contribution to power with and w/o the turbulators? Can you extract the Cl and Cd changes for the section with roughness and w/o? Could be interesting to see the effect on the turbine?*

    We have included now the figure 12 representing the pressure coefficient C_p over the blade section $r/R = 0.94$, corresponding to the location of a surface defect. This location is representative for all the other surface defects. In the same figure, we also display the C_p over 2 sections located 5 mm above and below of the mentioned location. From this figure, it is clear that the influence of the surface defect on the C_p distribution is twofold: enhancement of the suction peak and steep pressure rise just after the suction peak (creating a local valley). Both effects counteract each other. As a consequence, there is no significant change in the produced power (see also last paragraph of section 3.3).

13. *Table 1: Can you give the chord Re numbers as well?*

    We have included that information in the new version of the manuscript (see Table 1).