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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Low-order modeling for transition prediction applicable to wind-turbine rotors" *by* Thales Fava et al.

Anonymous Referee #2

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This study was aimed at developing a tool for predicting boundary layer transition over wind turbine rotor blades. It is a pleasure to see that this approach, which has been seen a fair amount of success in the aerospace sector being extended for further applications. Despite, being of a lower-order this method is elegant and the flow physics is better represented, as opposed to the more computationally expensive, higher-order RANS methods which in fact have been demonstrated to be inferior here. The parametric analysis has helped in systematically assessing the effect of different conditions on the transition mechanisms that operates in such a complicated flow. Whereby, both two-dimensional and three-dimensional modes appear to operate. The analysis of the two geometries shows that there are still further potential benefits achievable through shape optimisation to delay transition and this kind of tool will be beneficial for such

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Discussion paper

exercises. Despite the few comments and remarks that follows below this manuscript is strongly recommended for publication.

Further comments:

1. Introduction – The linear stability of the flow over swept wings could have been further reviewed considering that the 3D boundary layer and PSE formulation was initially made for these kind of problems.

2. Equation 29 – subscript 1 missing for the streamwise coordinate and γ has been used both for the frequency and as intermittency factor in the RANS modelling.

3. Overall, the discussion is quite thorough, however a bit prolix. For instance, a lot of effort has been placed in describing the shape of the velocity profiles but what are their implications in terms of boundary layer transition. As already reported on swept wings the highly inflectional nature of the profile of the transverse component (in this case u2), in the in-board region in figure 6(a) and 6(b) is also an indication of the potential crossflow instability. In fact, there is very little discussion on the behaviour of the mean flow and the possible route to transition, mainly focussed towards the end of section 4.2.

4. Similarly, the discussion on the flow over geometry 1 and geometry 2 are completely segregated. Since the topology of the flow is quite different from each other it will be interesting to compare them right from the beginning and this will already set the scene for how the modification of the mean flow by varying different parameter will favour a particular route to transition.

5. The sentence starting at line 349 and ending at line 350 is a bit of a contradictory statement.

6. Line 353 – "These differences arise from pressure distribution from XFOIL not exactly matching those from RANS, although they are close to each other". Any idea why there is this mismatch?

7. Figures 10 and 11 can be combined and similarly figures 15 and 16

8. Figures 12 and 17 can be rearranged so that they do not occupy a full page.

9. The angle η could be described in the schematic in Figure 1 for readers who are not used to the 3D flow topology, may be just sketch of the flow topology such as the development of the skin friction lines. In fact, why not show the skin friction lines from the RANS simulation also to complement some of the arguments about the three-dimensionality being more pronounced on some part of the rotor.

10. The sentence starting from line 402 could be rephrased to be more explicit.

11. The splitting of the near-wall lobe of the TS eigenfunction is also observed in the presence of the large adverse streamwise pressure gradient; therefore it might be worth tying this with the strong 2D mode amplification.

12. The attachment line has been mentioned on quite a few occasions however there is no mention of whether it is below the threshold for contamination, keeping in mind that the leading edge radius of curvature can be quite considerable in the inboard region of the rotor.

12. Although the method developed here will be useful in the design and optimisation of wind turbine rotor blades, the linear PSE has its limitations which D. Henningson and A. Hanifi will definitely agree, therefore it might be worth mentioning those, just to keep the reader informed and avoid any bias within the transition community.

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