# Review of WES-2021-23 manuscript

## General Overview

WES manuscript 2021-23 provides potentially interesting research results of the usage of two equation correlation based laminar-turbulent transition models implemented in Spalart Almaras turbulence model for the wind turbine airfoils at high Reynolds numbers. Authors compare the results both with the experimental data of a few airfoils and with the other CFD results in the literature. These comparisons can provide good insights and possibilities on the usage of two equation transition models for the predictions of the high Reynolds number flows around large offshore wind turbine airfoils. However, some of the experimental data used are questionable, some sections are not well structured, additional literature beyond the wind energy applications are necessary for the transition models and there are also many points that need clarification and correction. All these are needed to be completed and clarified before this manuscript can be considered.

Recommendation: Major revision

## Specific Comments

Line 33-34: I am not sure where in the paper of Sorensen et all 2016 this is stated. The main goal of that study was to make sure the codes produce consistent solutions in terms of domain size, grid resolution, convergence criteria etc. Moreover, there were no experimental results to compare. Another look at the literature is required to support this statement.

Line 34-35: accurate prediction of the glide ratio near design points.

Line 35: what/where is the design operating point? Normally, the airfoils are designed for a range of angle of attacks not for a single point.

Line 35: Please check cross-reference formats throughout the paper. Is it Sorensen (2014) or (Sorensen 2014)? I won't point out all of them but please check and correct the whole paper for this.

Line 35-36-37: In this paper of Sorensen, there were only three codes with transition model and all of them used eN method. Two of them provided results with different grid resolutions. Although it can be seen there is an effect of the eN transition prediction in the glide ratio and trends with Re numbers, I don't think this is a sufficient evidence for this statement; especially since in that paper there is no experimental results to compare. How about the other transition models used in the literature? For example, as you have also pointed out, Sorensen 2014 showed that the correlation based transition model of Menter was giving wrong trends wrt increasing Re number.
There is also another stability theory based transition model used in the paper from Coder. How about this one? It is recommended to check these statements in this paragraph and support with more evidence.

Line 40: "is difficult" - why it is difficult? Do you mean it takes longer to converge? Or something else? Please clarify

Line 41-42: what is wrong with computing N from semi-empirical models or from a stability solver? Please clarify.

Line 44: ... in coupling it with ...

Line 45-46: How about the suitability of this method? What is right/wrong with it? Was it used for the wind turbine airfoils applications?

Line 74: how about the suitability of the method for the machine learning process as mentioned in the beginning as the goal of the study?

Line 125 - paragraph. This paragraph seems incomplete. what is ysep and Gonset? Why these are not needed? What is the advantage or disadvantage?

General question about the implementation of the two eqn. transition model: was there a need to modify the original correlations or a new calibration to obtain better results for high Re number cases? Should we expect any influence?

Line 164: Why start with "However"? If the turbulence intensity is not a variable in the SA turbulence model and the current study is also assumes that the measured turbulence intensity is constant everywhere, this should be convenient for the SA turbulence model right?

Line 170: Validation. This section is hard to follow. A few subsections would help the reader to find its way easier.

Line 189-190: "... This implementation of the SA model..." this sentence seem redundant. We already know this.

Line 183: What can you say about the comparisons about the grid resolution and other parameters used?

Figure 3: Legend should be HAM2D and not Fully turb

Line 191: in this paragraph it is not clear whether you run Overflow yourself or you used the data from the transition modelling workshop.

Line 194: What kind of transition model is this AFT2019?

Line 195: "... Test flow condition is at free stream... " I don't understand this sentence. What is two equation Mach number?

Line 202: Do you also use the simulation results from Coder 2019 paper? As stated above it would be good to clarify which simulation comes from where and be consistent with how you refer to it in different places in the paper.

Line 204: What are the "known limitations if 2D CFD RANS"? How about whether or not these codes were able to resolve the laminar separation bubble at these simulations? Experimental results from TU Delft Wind tunnel as used by Somers https://www.nrel.gov/docs/legosti/old/6918.pdf show this bubble clearly and how lift actually continues to rise once post stall after the bubble is gone. I would expect a more elaboration on this physical phenomenon and how these codes were/were not able to capture this.

Line 206: Again in this paragraph, please make sure you refer to the correct references and address where the simulations come from consistently.

Figure 4a: Is the experimental results are for the tripped case? I think both tripped and untripped Cl should be available. Although the difference was small in the case of TU Delft experiments.
How about the Cl results for the negative angle of attack values?

Figure 4 legend of Overflow. It used SA turbulence model so there is no need to write SA again in the legend. Instead best to keep the names of the transition models only and add "fully turbulent" when transition is off.

Line 227: it says you will first study the effect of freestream turbulence intensity but these are not the first results.

Line 271: Ti3 is the lowest turbulence intensity given in the experiment in Table 2. Do you mean you need to lower this also?

Line 272: Laminar drag bucket is not visible in Fig.9.

Figure 6: Pires et all. 2016 does not have any simulation results??

Figure 8: are these still DU212 airfoil? Where do these results come from?

Figure 9 and Line 292: How can you tell that the L/D prediction is highly sensitive to the transition onset locatio; where do you see it in Figure 9?

Line 293: Which both quantitates?

Line 298: TU Delft wind tunnel cannot reach 6 or 7 million Re numbers for airfoil polars. It can go max. Re number of 3.3 million. You should check your reference.

Line 306: "By using either the one- or two-equation
transition model, lower drag coefficients were predicted at around 0 as a result of laminar boundary layer detection. This results in a better agreement in lift-to-drag ratio against experimental data compared to the fully turbulent simulations." These sentences are repetition. You have already emphasized it in the validation section when transition model is used, you have better drag prediction.

Line 311: why is the difference between two transition models increasing going to thicker airfoils? Could you elaborate this?

Figure 10: The experimental data cannot come from TU Delft wind tunnel. You should check your reference.

Line 318: Do you mean for 70% of AoA, the results are from transitional and 30% of Aoa, they are from fully turbulent results?

Line 326: how can you say that one equation model underpredicts the L/D from Fig 11? there is no experimental data...

Line 330: I thought the name of the transition model in HAM2D was Gamma-ReThetat-SA? Are you using also the original Gamma-ReTheta formulation then?

Typo in "EllipSys2D" overall.

Line 340: How can you tell that two equation model predicts transition more accurately by looking at the Fig.11 and Fig.12? There are no experimental results there. And in Fig12, EllipSys results are also with two equation transition model.

General remark on the results section: Since the figures are far from the text, it is difficult to follow it. I also have the impression that not all figures are used in the text; especially of Fig. 11. Perhaps a synthesis can be made for the ease of the reader. Another remark is the legends. Please add HAM2D in the beginning so that it is clear which results we should be looking at.

Line 346: another confusion in the name of the 2eqn. transition model.

Line 349: where does it say these turbines are commercially relevant? Is there a reference for that?

Line 354: They used 2eqn. model in this reference, not 1.

Starting with Line 356: How about the trend with increasing Re number? In the reference Sorensen 2016, they show that two equation model shows increasing drag with increasing Re number. How about in your results? Although you state it is better but from which Figure I can see this?

Line 361: In the reference Ceyhan 2017b, there are no two equation model results. I think you refer to the turbulence intensity values. If so, please rephrase this sentence.

Line 362: Why do you say there is a limitation on the two equation model? How does being sensitive to the turbulence intensity make the model limited?

Line 368: why Galilean invariant formulation makes a model more desirable? From what I can see in these results, there is no reason to use 1eqn. model for these applications.

Line 456: Yilmaz 2017 and Ceyhan 2017b are the same references?