

Author response to comments by Reviewer 1 of “Local Correlation-based Transition Models for High-Reynolds-Number Wind Turbine Airfoils”

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We thank Reviewer 1 for the many constructive comments and suggestions which have helped to improve the manuscript. We have tried to address most of the concerns as best as possible. We hope that Reviewer 1 would be satisfied by our changes to the manuscript and our responses.

Each issue raised by a specific comment in the report is addressed in detail below. Modifications of the manuscript can be tracked in the highlighted version of the revised article (red = removed, blue = added or modified).

Line 33-34: I am not sure where in the paper of Sorensen et al. 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.

We agree with that the reference is incorrect for the explaining the limitation of 2D RANS-CFD in stall prediction. In the revised manuscript, the reference is corrected to “Ceyhan et al. 2017b”.

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.

We agree with the suggestions and corrected the revised manuscript as shown below:

“Airfoils are typically designed to operate inside a range of angles of attack for maximum performance away from stall in the linear portion of the lift curve. Hence, the generation of training data for airfoil-design purposes requires the accurate prediction of the glide ratio inside the design range of angles of attack.”

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.

We corrected reference format consistently overall the revised manuscript. We follow the rule of in-text citation from the template. Thus, if the reference authors name is part of the sentence structure, only the year is put in parentheses. Otherwise, name and year are put in parentheses.

Line 35-36-37: In this paper of Sorensen, there were only three codes with transition model and all of them used e^N method. Two of them provided results with different grid resolutions. Although it can be seen there is an effect of the e^N 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.

The paper by “Sorensen et al., 2016” only includes the e^N transition results from DTU, NTUA, CENER-WMB codes at 3 and 15 millions and the statement as “Results are not included for the correlation based transition model by Menter and Langtry [15], [16], available in some of the codes, as it fails to correctly predict the natural transition behaviors at high Reynolds numbers.”

We agree with the comments from the reviewer, and these are insufficient evidence for the current statement especially in the word selection “transition model”. To clarify the paragraph, the sentences in the revised manuscript are changed as shown below:

“Hence, the generation of training data for airfoil-design purposes requires the accurate prediction of the glide ratio inside the design range of angles of attack. The variation of the glide ratio near the design points is highly sensitive to the boundary layer transition onset location.”

Also, the stability theory based transition model by Coder (AFT model) is not the subject of this work. We use it as a reference for comparison to state of the art models in Section 3 for validation at low Reynolds numbers where all of the test transition models work well.

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

The e^N based approach uses integral boundary layer (IBL) method which solve for quantities not readily available in general CFD approaches. Extracting the associated IBL quantities, such as displacement thickness, momentum thickness, and shape factor, requires non-local search and line integration operations for the CFD. To clarify the meaning, we added more explanation in the revised manuscript as shown below:

“However, the application of the e^N method within a conventional RANS framework that runs on massively parallel computers is difficult. This is because it involves non-local search and line integration operations for boundary layer quantities (e.g. displacement/momentum thickness and shape factor).”

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

The semi-empirical model cannot guarantee model accuracy for any arbitrary flow conditions because it is based on a limited experimental dataset. Also, solving a linear stability

solver to compute N factor requires additional effort in e^N based model. Additionally, the output of e^N based method should be fed into CFD computation several times until the solutions are fully converged. For better clarification, we changed the sentence in the revised manuscript as shown below:

“Also, additional efforts in communications between e^N and RANS methods are required [7].”

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

We agree with the suggestions and corrected it in the revised manuscript.

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?

The AFT model is not the subject of this work and it is not used as a reference for comparisons (e.g. LCTM or e^N models) at high Reynolds number flows in this paper. But, in the introduction, we just wanted to cite the model as another model available in the literature that is coupled to the SA turbulence model. As a reference model predicting for S809 airfoil at the low Reynolds number flow, the model is briefly introduced at Section 3 in the revised manuscript as shown below:

“AFT2019 transition model was developed based on linear stability theory, which is also widely used in aerospace problems. It solves two transport equations for amplification factor and intermittency.”

Therefore, we chose to delete the current sentence regarding AFT model here.

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?

The machine-learning approach to airfoil design will only use the polar data generated by the CFD solver and hence is independent of and agnostic to the choice of the transition model. This approach will only require accurate prediction of the quantities of interest relevant to design just like any other design approach.

Line 125 - paragraph. This paragraph seems incomplete. what is y_{sep} and G_{onset} ? Why these are not needed? What is the advantage or disadvantage?

In the manuscript, the γ_s and G_{onset} are the variables for explaining main differences between current two-equation implementation and Langtry-Menter (2009) models. However, the details of the differences between the models are already explained in the previous study which is already cited as [5]. We think detailed explanations of the differences are redundant in this manuscript. Thus, we changed the statements in the revised manuscript as shown below:

“Details of the current implementation of the transition model compared to $\gamma - \overline{Re_{\theta t}}$ model by Langtry and Menter (2009) are shown in the previous study (Medida, 2014; Jung and Baeder, 2019).”

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?

The two-equation model retains the primary features of the Lantry-Menter model (2009). However, the following changes were made by Medida et al. (2014):

1. New correlation for $Re_{\theta t}$,
2. Constant freestream turbulence intensity,
3. Modified production and destruction terms in the intermittency equation,
4. Omission of the separation-induced transition modification,
5. Destruction term in the SA model not scaled by intermittency.

We use the implementation and correlations from Medida et al. (2014). No additional modification or new calibration especially for high Reynolds numbers are made in this paper.

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?

We agree with the comment. Thus, in the revised manuscript, the sentences are changed to prevent any possible confusion.

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

Based on this and another similar suggestion from Reviewer 2, we have reorganized sections 3 and 4 into validation of the turbulence model and the transition models. Section 3 now contains only validation results for the SA turbulence model using the fully-turbulent flow assumption, while Section 4 contains the validation results for all simulations using the transition model. This is only reflected in the output of *latexdiff* program for the response to Reviewer 2.

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

We agree with the suggestions and deleted the sentence in the revised manuscript.

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

Both simulations used enough fine meshes for the fully turbulent flow simulations, thus the both predictions have minor mesh dependency. From the reference (Bak et al., 2013), the enough fine resolution mesh was used for EllipSys2D to ensure mesh independence; 512 cells around the airfoil and initial wall normal spacing of 5×10^{-7} chord. In the current simulation, 400 points around the airfoil and initial wall normal spacing of 2×10^{-6} chord

which correspond to $y^+ = 1$ were used as already discussed in the Methodology section. Also, the minor mesh dependency in the current study is shown in Appendix B in the revised manuscript.

In terms of the other solver parameter, both simulations neglected any compressibility effects. In the current study, a freestream Mach number is set as 0.1 to represent incompressible flow condition. Otherwise, EllipSys2D is an incompressible solver. In the revised manuscript, the sentences are added as shown below:

“Both predictions used enough fine meshes for the fully turbulent flow simulation, thus there is minor mesh dependency on both predictions. Also, both simulations neglected compressibility because EllipSys2D is a incompressible solver.”

Figure 3: Legend should be HAM2D and not Fully turb

The legend is fixed as HAM2D in the revised manuscript

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

In this study, we did not run OVERFLOW ourselves. Instead, we used data from the previous studies (Coder, 2019; Hall, 2018). For better clarification, the sentence is corrected in the revised manuscript as shown below:

“We show validation of the aerodynamic performance prediction against experimental data [8] as well as previous simulation results using NASA’s OVERFLOW code from Coder [2] using SA-neg turbulence model with AFT2019 transition model.”

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

AFT2019 model is based on linear stability theory rather than local correlations is coupled with the SA turbulence model. In general CFD approaches, it solves two transport equations for amplification factor (n) and intermittency (γ). The amplification factor transport equation was originally derived based on Drela-Giles model [3]. For more details, the sentence is added in the revised manuscript as shown below:

“AFT2019 transition model was developed based on linear stability theory, which is also widely used in aerospace problems. It solves two transport equations for amplification factor and intermittency.”

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

The typo is corrected in the revised manuscript.

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.

We used OVERFLOW simulation results from previous studies (Coder, 2019; Hall, 2018). We clarified the source of the results in the sentences for the revised manuscript.

Line 204: What are the “known limitations of 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.

While there are several “known limitations of 2D CFD-RANS”, we are specifically referring to the over-prediction of the stall angle for airfoils in this sentence. The current two-equation model capability for capturing the laminar separation bubble at mid-chord was already validated in a previous study by Jung and Baeder [5].

As shown in Fig. RC1-1, the surface pressure distribution are plotted using different resolution meshes and the predictions are compared to experimental data [8] at 1, 6, and 9° angles of attack. As an indicator of the laminar separation bubble, a small region of flattened surface pressure is well observed, especially on the lower surface in both experiment and the predictions.

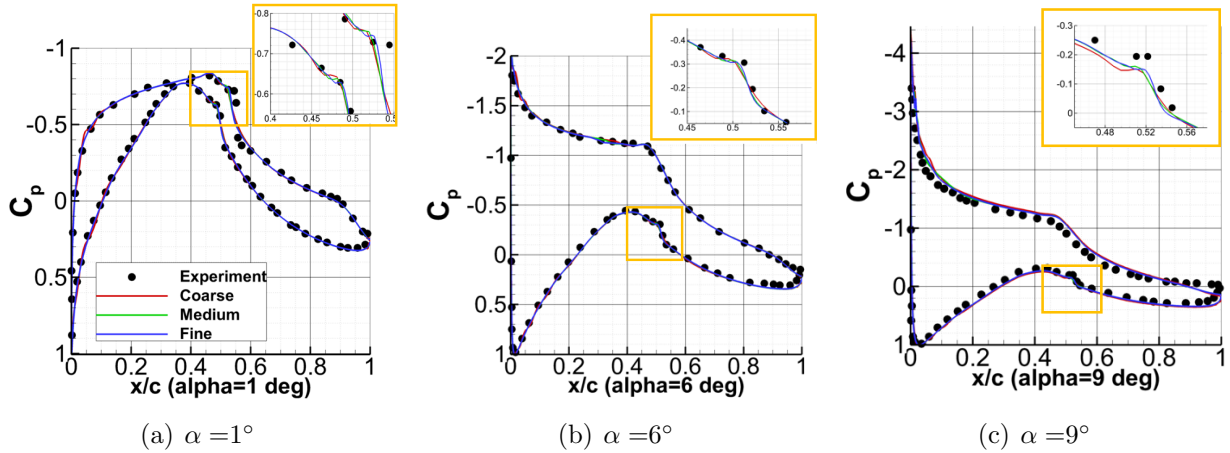


Figure RC1-1: Surface pressure distribution for S809 airfoil at $Re = 2 \times 10^6$ from Jung and Baeder [5].

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

We revised the manuscript as shown below:

“Figure 4 (b) shows that the drag predictions from HAM2D using the fully turbulent approximation are in excellent agreement with the OVERFLOW simulation results (Coder, 2019) at the same flow condition over the full range of angle of attack while showing a slight

underprediction in the drag bucket compared to the tripped boundary layer experimental data.”

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

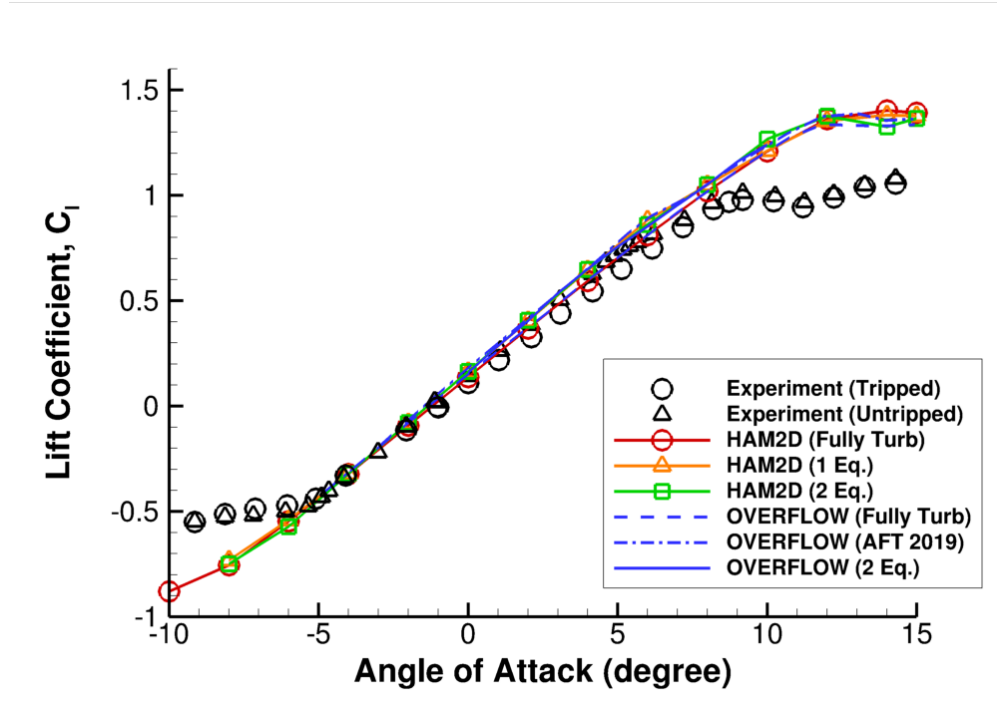


Figure RC1-2: Lift coefficient

The experimental C_l results in the manuscript are from the untripped condition as the label already showed. As suggested by the reviewer, Fig. 4 (a) has been modified to include both tripped and untripped experimental C_l data as shown in Fig. RC1-2. Also, the comparison is extended towards the negative angles of attack. We updated the description of the results to explain the new data comparison in the revised manuscript as shown below:

“Figure 4 (a) shows that all simulations predict the lift coefficient well in the linear region of the lift polar. In detail, slightly higher lift coefficients from the untripped experiment than the tripped one are captured using either one- or two-equation model. Otherwise, the predictions significantly overpredict the maximum lift coefficient due to the known limitations of 2D CFD-RANS.”

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.

The legend of OVERFLOW is changed as recommended in the revised manuscript.

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

The sentence is changed for better clarification in the revised manuscript as shown below:

“We compare the airload with measurements in both fully turbulent and free transition conditions. The effect of the choice of transition model on the prediction of the transition onset location is analyzed. Finally, the sensitivity of freestream turbulent intensity on airload predictions using two-equation transition model is shown.”

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

The sentence was made based on the free-stream turbulence intensity study in Fig. 9 from the manuscript. We intended to mention that the drag prediction is sensitive to the freestream turbulence intensity level at the higher Reynolds number. Thus, reducing the turbulence intensity further will reduce the current deviation with experiment regarding the drag coefficient and L/D. We decided to delete the sentence here because it will be discussed later for Fig. 10 from the revised manuscript. The sentences for Fig. 10 is also revised in the revised manuscript as shown below:

“The prediction of the lift-to-drag ratio is highly sensitive to the inflow turbulent intensity level. Also, the sensitivity becomes stronger at the higher Reynolds number, which results in the best correlation with the experiment using the lowest intensity (Ti3) as observed in a previous study using the e^N transition model (Ceyhan et al., 2017b).”

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

The statement was deleted in the revised manuscript from the answer of the previous comment.

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

The reference is corrected as (Ceyhan et al., 2017b) in the revised manuscript.

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

Yes, the results in Fig.8 is about DU-00-W212 airfoil as shown in the caption of the figure. The results which labeled as “1Eq. Tran” and “2Eq. Tran” are from the current prediction. The reference predictions was obtained from the previous study (Sorensen et al., 2014) using EllipSys code which coupled with different transition models: LCTM, e^N model, and e^N -BP model (with bypass transition). To prevent any confusion, the sentence is changed in the revised manuscript as shown below:

“These predictions are also compared with those from EllipSys2D using the k - ω -SST turbulence model and different transition models: $\gamma - \overline{Re_{\theta t}}$ (LCTM), e^N model, and e^N -BP model with bypass transition (Sorensen et al., 2014).”

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

The sentence is deleted and the corresponding paragraph is re-written in the revised manuscript as shown below:

“The prediction of the lift-to-drag ratio is highly sensitive to the inflow turbulent intensity level. Also, the sensitivity becomes stronger at the higher Reynolds number as observed in a previous study using the e^N transition model (Ceyhan et al, 2017b).”

Line 293: Which both quantities?

The statement was deleted from the answer for the previous comment.

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.

We thank the reviewer for bringing this to our attention and agree with the reviewer. Upon further review, we found that the reference for the airfoil polars in the NREL 5MW turbine is the the DOWEC 6MW Pre-Design report [6]. Page 14 of this report states that

“Ruud van Rooij of Delft University of Technology provided 2D measured coefficients for the DU-airfoils for a Reynolds number of 7 million. Table 2 shows that the Reynolds numbers (and aerodynamic performance) for the blade part outside rotor radius 20 m are significantly more for nominal operating conditions. Even more so, the airfoil data for the NACA 64-618 as copied from appendix IV of Abbott and Von Doenhoff, apply to a Reynolds number of 6 million”

Further research into this [9] revealed that this is not true and that the airfoil polars were “synthesized” using RFOIL [10] for the Reynolds number of 7 million using correction factors on the basis of a comparison of RFOIL calculations and measurements at 3 million from the Delft wind tunnel in the clean configuration. However, we feel a comparison to this data is still relevant as it highlights the differences between the one-equation and two-equation transition models for the prediction of the glide ratio in the design range of angles of attack. Based on this, we have updated the discussion in the paper as follows:

“The predictions of HAM2D using both transition models for the airfoils in the NREL 5 MW turbine [4] are compared against data available in the DOWEC 6MW pre-design report [6] for Reynolds numbers of 6 and 7 million. Our understanding is that the reference data for the DU airfoils [9] were “synthesized” using RFOIL [10] for the Reynolds number of 7 million using correction factors on the basis of a comparison of RFOIL calculations and measurements at 3 million from the Delft wind tunnel in the clean configuration. According to the DOWEC 6MW pre-design report, the reference data for the NACA64-618 airfoil is obtained from appendix IV of Abbott and von Doenhoff [1].”

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.

We agree with the comment, Thus, the sentences are deleted in the revised manuscript.

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

Typically, the integrated airload of the airfoil becomes more sensitive to the transition onset location as the onset location closes to the leading edge where large suction peak occurs. As the thickness of airfoil increases, the onset location moves towards the leading edge due to higher adverse pressure gradient. This is why the difference in integrated airload increases for the thicker airfoils between two transition models. The additional sentence was included in the revised manuscript as shown below:

“This might be because the onset location typically moves towards the leading edge for the thicker airfoils due to the higher adverse pressure gradient at the same angle of attack.”

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

We agree with the reviewer and have addressed this in a discussion earlier.

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

The lift and drag values at each angle of attack are linearly interpolated between the free-transition and full-turbulent results using the 70/30 ratio. The text in the paper has been adjusted to add the same information

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

We clarify that the underprediction is with respect to the predictions using the e^N transition model used in EllipSys2D (Ceyhan et al, 2017b). The description in the revised manuscript is changed as below:

“The one-equation model predicted much lower lift-to-drag ratio than the predictions from other transition models in the linear portion of the lift curve due to earlier transition onset.”

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

Yes, the name of two-equation model in HAMS2D is $\gamma - \overline{Re_{\theta t}}-SA$. Thus, the sentence is corrected in the revised manuscript.

Typo in "EllipSys2D" overall.

Over the entire revised manuscript, it is corrected as "EllipSys2D"

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 Fig. 12, EllipSys results are also with two equation transition model.

In Fig. 12, the two-equation model in HAM2D predicts more delayed transition onset locations than the other predictions including EllipSys2D (two-equation). We think the delayed predictions of onset locations might results in good agreement with e^N method in terms of lift and drag coefficients as shown in Fig. 11 (d) for the same case from the manuscript. The different results between the two-equations in HAM2D or EllipSys2D might be from not exactly same formulations of the two-equation models. The detail of the difference can be found in the previous study (Medida, 2014; Jung and Baeder, 2019).

As suggested by reviewer, the paragraph was changed in the revised manuscript as shown below:

"The delayed onset locations from the two-equation model in HAM2D than other LCTM predictions might explain the good airload agreement with e^N method as shown in Fig. 12."

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.

We thought showing all results for different thickness airfoils can show general performance of one and two-equation transition models although the trend is similar between the airfoils. However, we agree with the comments. For better readability, only DU25-A17 and NACA64-A17 airfoils from DU airfoil series and FFA-W3-301 airfoil from FFA-W3 airfoil series are left in the "Results section". All of the results for the other airfoils are moved to the "Appendix A: Additional Results".

Over the revised manuscript, "HAM2D" is added in the beginning of the legend at each figure.

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

We agree with the comments. Thus, over the revised manuscript, current two-equation model for SA turbulence model is named as $(\gamma - \overline{Re_{\theta t}} - SA$ or two-equation model) and one equation model for SA turbulence model is named as $\gamma - SA$ or one-equation model. Otherwise, the two-equation model by Langtry and Menter (2009) for SST turbulence model which was implemented in EllipSys2D is named only as $\gamma - \overline{Re_{\theta t}}$ model over the revised manuscript.

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

Any description of commercially relevant turbines has been changed to modern, open-source, MW-scale turbines in the revised manuscript.

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

For more clarification of this sentence, the sentence is changed in the revised manuscript as shown below:

“The one-equation transition model fails to predict the natural transition behavior at the high Reynolds numbers ranging from 6 million to 15 million due to early transition onset, as reported in previous study for $\gamma - \overline{Re}_{\theta t}$ model (Sorensen et al., 2016).”

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?

For DU00-W-212 airfoil, the drag coefficients at varying Reynolds number are compared with experiment at 4° angle of attack where the maximum L/D ratio occurs as shown in Fig. RC1-3. It is shown that the drag coefficient from the experiment decreases from 3 to 9 million Reynolds numbers, and then it increases until 15 million Reynolds number. However, the variations between the Reynolds numbers are minor. For the two-equation model, the variations between the Reynolds numbers are also minor as experiment though the drag increases as Reynolds number increases as shown in Fig. RC1-3 (a). Otherwise, the drag clearly increases as Reynolds number increases in the one-equation model prediction, which is opposite trend with experiment as shown in in Fig. RC1-3 (b).

Also, the predicted drags are broken down into viscous and pressure drag components. As a result, the viscous drag component is dominant over the pressure drag at all Reynolds numbers from both transition models. This also indicates the importance of transition onset predictions because the skin friction is much higher in a turbulent than laminar boundary layer.

In the revised manuscript, Fig. RC1-3 is added in the Section 4 and the corresponding statements are changed or added.

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.

We intended to mention the same observation with the previous study using e^N based transition model. Thus, the sentence was changed as shown below:

“The predictions from the two-equation transition model exhibits a strong sensitivity to the free-stream turbulence intensity at the high Reynolds number, as previously observed from the e^N based models.”

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?

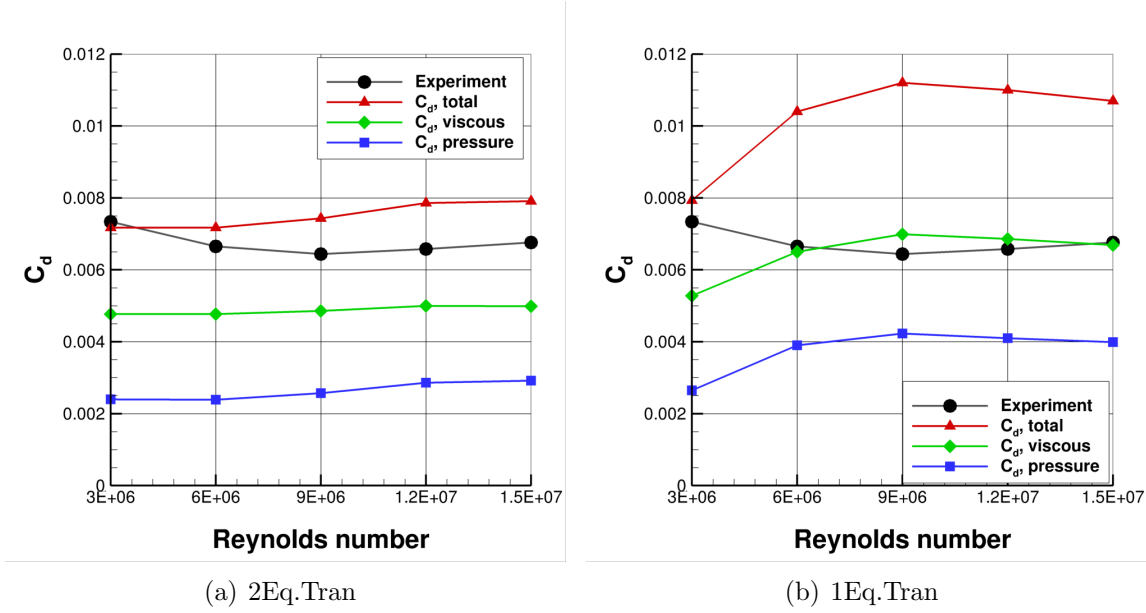


Figure RC1-3: Comparison of drag coefficient for DU00-W-212 airfoil at 4° angle of attack and various Reynolds number

We meant the under-prediction of lift-to-drag ratio at Reynolds numbers greater than 12 million as the limitation. For more clarification, the sentence was changed and moved to the previous paragraph in the revised manuscript as shown below:

“At high Reynolds numbers from 12 million, the two-equation model also somewhat underpredicted the maximum lift-to-drag ratio compared to the results from- c^N -based transition models.”

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.

A transition model would be more accurate with Galilean invariant formulation in simulating any rotating bodies (e.g. blade). The formulation one-equation transition model satisfy this Galilean invariant, otherwise the two-equation model does not. Therefore, once the current limitation at high Reynolds numbers is resolved for the one-equation model, it would be more desirable for general cases than two-equation model. Recently, Field-Inversion Machine-Learning approach was developed by others (Holland et al., 2021) to resolve limitations in RANS CFD method (e.g. post stall region). We think the approach can be applicable for the current transition models for the limitation at a high Reynolds number.

To clarify the meaning, the sentence is changed in the revised manuscript as shown below:

“However, the formulation of one-equation transition model satisfies Galilean invariant which is desirable in a simulation with rotating bodies (e.g. blade). Therefore, in the future, we plan to improve the performance of the one-equation transition model using the Field-Inversion Machine-Learning approach which was validated for the SA turbulence model (Holland et al., 2021).”

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

Yes, two reference are the same. The reference is unified as Ceyhan 2017b over the revised manuscript.

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