Responses to Reviewer Comments: Report #1

We would like to thank the reviewer for the effort in reviewing and commenting on this article. The comments provided have helped to improve the manuscript considerably.

Please find below our responses to each comment highlighted in** blue **and the modified line, figure or table was also mentioned in each case.

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1. *General: However I believe the methods section needs to be improved for the work to be accepted.*

This section has been improved according to Q3 and further explanations has been added to insure clear methodology

2. *General: It would be very hard for others to reproduce this work reading the paper in it's current state.*

Unfortunately, we do not have a permission to share the airfoil geometry. It was very hard in the first place to get such data. We understand your concern, but we believe the methodology and the conclusions are beneficial to be shared with the scientific community. Moreover, the benchmark cylinder case is fully reproduceable. For the airfoil cases, we improved the description of each setup to help others to apply it on different cases in the future.

3. *Sections 2.1.1 and 2.1.2. : I don't think it's necessary for the authors to explain the turbulence models used: these are well known to readers experienced in CFD and can be researched by the interested reader in provided references if necessary. It would be preferable to briefly go over advantages and disadvantages of each model and to elaborate on why they were used in this specific case.*

We agree with the reviewer and this part has been reformulated, see new Sections 2.1.

4. Sections 2.1.1 and 2.1.2.: *Also, the choice of using two turbulence models in the same study seems odd, so further explanation on this aspect should be provided.*

Two different turbulence models were used to fit the different rwf's. Momentum rwf defines a relationship for turbulent kinematic viscosity, accordingly was used with SA turbulence model. On the other hand, DLR and Colebrook rwf's give relationship of k and omega at the wall, hence they were used with k-omega SST turbulence model. Also, it is very common to use SA in the IDDES hybrid models which seems to be promising for future work to use such a model with rwf. The main goal of this paper is to study the combination of widely used turbulence models in combination with rwf's to study the roughness effects. (Currently Sections 2.1)

5. Figure 2 : *The scanned data points shown in figure 2 for the two ice-profiles appear to be quite far apart. From what is stated in section 3.2 the scanned profile is used to calculate the average sand grain roughness. Please justify the expected impact of the number of scanned points on the modeling.*

The shown circles in this figure are just markers to differentiate the two profiles from each other. Since the real number of points is more than 1600 points for profile A and more than 2800 points for profile B, exact representation of each point was not shown. This figure has been updated with two different colors instead of markers to avoid any confusion.

6. Section 3.2: *How can one differentiate between roughness and geometry? In other words what part of the geometry can be justifiably smoothed and included in the roughness wall functions and what part must be retained? A robust criterion is not provided here. See final comments for further remarks on this.*

The smooth surface was generated using cubic spline. The cubic spline was selected in this case to make sure that the ice surface is smooth enough to avoid complex final shape and enable the usage of a large first wall cell. In case of a higher degree smoothing, the surface will keep some of its roughness and generating a mesh for such a surface will not result in a good quality cells near the walls. This methodology is comparable with the method used in the EN ISO 4287 standard for surface roughness measurements and calculations. The first cell height of each grid was indicated in Table 1. Also, this method has been highlighted further in the paper.

7. Section 3.2: *Assuming roughness elements with a conical shape seems quite semplicistic. Please justify this choice*

This assumption was based on scanned 3D ice surfaces shown in the article: “Convection from Surfaces with Real Laser-Scanned Ice Accretion Roughness and Different Thermal Conductivities” by Hawkins et al. that suggested either conical or hemispherical roughness elements and the article “Ice Roughness and Thickness Evolution on a Business Jet Airfoil” by McClain et al. In both articles, the scanned ice surfaces can be assumed to conical shapes to be able to calculate the parameters necessary to calculate equivalent sand roughness height. The two articles have been cited in the paper

8. Section 3: *Are rough wall functions used on the entire airfoil or just on the iced part? If it’s the first, how does using rough wall functions on the entire airfoil influence the results? Perhaps it would be useful to check that all the wall functions proposed give reliable and consistent results for an airfoil with no icing.*

The rwf's were used only with the ice surface. The ice profile was separated as a different boundary and the rwf's were applied to them separately. If the rwf’s are used with Ks = 0, the results should reduce to zero velocity shift. Also, the rest of the airfoil is smooth compared to the ice surface. Accordingly, any Ks value will be unrealistic. If the roughness of the non-iced surface is present, the same wall functions could be definitely applied. It that case, it should represent the roughness of the coating or/and erosion.

9. Figures 4 and 5: *Please explain these figures better. If the "fine" and "coarse" grid refer to cases where wall functions are used vs cases where they are not the authors must explain the difference in the size of the elements surrounding the airfoil. The coarse grid differs not only for the boundary layer but also in the flow-field. In my opinion this can significantly influence the results.*

More explanation and grid data have been added to the next submission. (Currently Figures 5 and 6)

10. Figures 4 and 5: *The meshes appear different in the fine and coarse cases. "Fine" seems to use Cartesian cut-cell meshing, coarse uses polygonal cells. Please explain the differences.*

The fine mesh was used to simulate the flow around profiles in case of fully resolving roughness. This should give an indication about the benefits of using rwf's since using them will lead to minimal deviation from fully resolved roughness case while using a coarse computational grid. Currently (Figures 5 and 6)

11. Section 3.3: *Was a mesh independence study performed?*

Several grids with different first cell heights were studied in order to ensure the closest fit to Cl curve with the coarsest grid possible. However, the mesh test was carried out according to the criteria explained in the article "New Near-Wall Treatment for Suspended Sediment Transport Simulations with High-Reynolds Number (HRN) Turbulence Models" by Liu. These criteria have been further explained in the paper. The study is not mentioned since we have experimental data and fine mesh to give more confidence to our results Section 3.3

12. Table1 : *Why different Reynolds numbers?*

Since the roughness mainly effects on the detachment and re-attachment of flow on the surface, different Reynolds numbers should be a good idea to test the capabilities of each model. The different Reynolds numbers were provided by the experimental measurements.

13. Table1: *I suggest including the total number of elements of the mesh as well.*

It has been added to the revised version in Table1.

14. Figures 7,9 & 11: *The "Exp." Dataset appears to have duplicate data points for some AoA's. For instance, Fig. 9 AoA=12, 2 "Exp." Points are clearly visible. Please explain.*

The Experimental data includes two sets of measurements: one set in case of increasing and the second set for case of decreasing AoA. The big differences in Cl values occurs only in post-stall AoA's due to high separation. The data has been plotted with error bars instead. (Currently Figures 8,10 & 12)

15. Figures 7,9 & 11: *Also, why are these figures relative to maximum lift? This could accentuate or diminish the difference between models depending on the value of maximum lift.*

We are not allowed to share the Cl values, but we reached an agreement with the experimental data owner to share the value normalized to Clmax. All values in each case are divided by a single value which is the Clmax of experimental measurements to maintain the correct relationship between the different cases. We have double checked the data to make sure that the relationship is correct. (Currently Figures 8,10 & 12)

16. Section 4.4: *As stated previously it is hard to judge differences in lift given the relative nature of figure 11. However, as it stands the differences seem pretty substantial even in the linear region.*

The Cl values is now recalculated taken into consideration only the summation of pressure forces since the experimental Cl values were calculated using pressure taps. A better agreement was shown in some case. However, some other cases had fair agreement due to the effects of the ice profile on the flow. A better understanding of these differences is shown in the Cp distribution results. Section 4.4

17. Figure 12: *here seems to be a considerable difference in figure 12b between momentum model and the others. This is not reflected in figure 11 at AoA=4°, why?*

Because in some cases, the deviation in Cp values on the upper and the lower surfaces can cancel each other and result in a summation of forces close to the experiments. Accordingly, the agreement analysis in Sec. 4.5 and Fig. 14 only use the Cp distribution as a criteria for agreement. A similar justification for these results was mentioned in line 273-277. (Currently Figure 13)

18. Line 240: *This phrase is not clear, please revise it.*

Noted and will be re-phrased. (Currently Line 247-250)

19. Section 5.1: *Authors state that many icing profiles should be measured and averaged to get a good reference shape of the airfoils. What about the roughness values? How can those be estimated and used in computations?*

The ice formation phenomenon is a stochastic phenomenon. This means that if the same airfoil was exposed to the same conditions many times, there should be small differences in the exact roughness shapes. Accordingly, to have an accurate simulation, many profiles should be considered and then average. However, this is not practically possible. Having said that, the estimated roughness should be less sensitive to such variation, since the estimation is kind of spatial average.

20. Line 260: *I suggest to edit figure 12 to include all of the tested roughness models. This would allow to compare if and where separation is predicted. Also, separation in this case seems like it is a consequence of the shape of the ice and not relative to roughness. If a model cannot correctly predict this separation can this be caused by excessive geometry smoothing? In other words, could agreement be improved is a different smoothing strategy was adopted? Or if no smoothing was adopted and roughness height measured differently?*

The separation bubbles are only visible in case of fully resolved flow around roughness because other rwf compensate the effects of these separation with a mathematical model to change the different turbulence parameters accordingly. Accordingly, there are only one figure can be generated with clear separation bubbles for each model at each AoA. Also, showing this figure in the article aims to given the reader some idea about the effect of irregular shapes on the surface. (Currently Line 268)

Line 263: *This is a good point. What kind of wall function is used in the non-iced part of the airfoils?*

for nu-t wall function: based on Spalding's law.

For k: wall function: zero-gradient with simple modifications (called kqRWallFunction in OpenFOAM v6).

For omega: based on Menter, F., & Esch, T. (2001). Elements of industrial heat transfer prediction. In Proceedings of the 16th Brazilian Congress of Mechanical Engineering (COBEM), November 2001. vol. 20, p. 117-127. (called omegaWallFunction in OpenFOAM v6).