Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2019-66-AC1, 2020

Interactive comment on "Parametric slat design study for thick base airfoils at high Reynolds numbers" *by* Julia Steiner et al.

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Received and published: 23 January 2020

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Reply to the comment

January 23, 2020

General remarks
Dear referee,
thank you for taking the time the review the paper in detail and for giving extensive feedback that helped us improve the draft. An insight from sombody with a more general aerospace based background was very useful for us.

Replies and adjustements to the specific and technical comments are outlined below. With respect to the general comments, most of them are addressed in the specific comments as well. Additionally, I added a couple more references & comparisons to the existing literature: see line 280ff/page 13, line 294ff/page 13, line 352ff/page 16 (expanded below).

- "Comparison with literature: A comparison with the auxiliary slat optimization done by Schramm et al. (2016) and Manso Jaume and Wild (2016) for a 25 % thick base airfoil reveals similar optimal designs. Namely, the obtained slat designs have a large camber, the optimial slat streamwise position aligns with the location of the suction peak on the main element, a stall angle close to 20 degrees and a maximum lift increase of at least 100 %."

- "Nevertheless, despite this somewhat counterintuitive result, the previously mentioned publications (Schramm et al. (2016), Manso Jaume and Wild (2016) and Pechlivanoglou et al. (2010)) use gap width of the same order of magnitude ranging between about 2.5% and 6%."
- "The shape of the optimal main elements is much less sensitive to the aerodynamic efficiency of the design than the optimal shape and position of the slat element. However,this is at least partly a consequence of the structural constraints on the main element, because without the structural constraint especially the pressure side of the main element looked very different. Nevertheless, the main element profiles look very similar to the results obtained by Manso Jaume and Wild (2016)."

Reply to specific comments

page 2, lines 27-43: *The described methods for stall delay (vortex generators and Gurney flaps) are not part of the study. It should be checked if this information is of any benefit for the paper.*
We consider the benefit of this information the option to highlight that the slat element has a different working principle and can be beneficial for different reasons. Furthermore, the beneficial effects of the two afformentioned elements are more widely known and hence to us it makes sense to mention them.

page 2, lines 47-48: *The description of the Circlulation Effect is misleading. The first sentence is not describing the soure of the circulation increase. Instead, the circulation on the rear element induces an upward velocity component at the trailing edge of the preceding element. This has to be compensated by the forward element circulation to achieve the Kutta-condition at the trailing edge.*
This was indeed misleading and has been reformulated to: "The circulation around the main element induces an upward velocity component at the slat trailing edge. In order

to fullfil the Kutta condition at the slat trailing edge, an increased circulation around the slat is necessary. Thus, the circulation of the slat in the vicinity of the main element is increased as compared to the free-standing slat element only."

page 2, line 51: *The description of the Dumping Effect does not describe the origin of the accelerated flow. It must be described that the high velocity at the forward element trailing edge is induced by the low pressure of the suction region at the leading edge of the downstream element.*
This was reformulated to: "This effect is closely related to the circulation effect. The circulation around the main element also leads to a low pressure region around the slat trailing edge. As a consequence the high outflow velocity of the boundary layer of the slat relieves the adverse pressure gradient on the slat element. Hence, separation problems are further alleviated."

page 3, line 57/58: *It is a misunderstanding that the slat increases the lift coefficient at the same flow condition (angle of attack). This is usually not the case as long as the slat doesn't significantly increase the overall chord length of the airfoil system. This would be accomplished by a steeper gradient oft he lift curve vs.angle of attack. The lift created by the slat compensates the lift drop at the main element due to the reduction of the suction peak (Slat Effect). The part of the statement "an increase of the lift for all angles of attack and" should be deleted. Consequently, the text in the following paragraphs has to be adopted (delete "increased lift and" in line 63 as well as the sentence line 64/65).*
The author agrees and this was removed. We do indeed see a lift increase for all angles of attack in our designs, but this seems to be a consequence of the higher apparent chord length.

page 3, line 79: *The reference to the airfoil numbers is a correct citation of the referred*

work by Pechlivanoglou. Nevertheless, the notation of the TU Delft airfoil deviates from other known notations. Further on, the NACA 22 airfoil is at first not widely known and the notation suggests to be a mistake as there is no 2-digit NACA airfoil series as such. So it would be benefcial to refer to its origin (first named in Weick and Noyes, NACA TN 451, but designed and first tested by Weick and Wenzinger, NACA TR 407)
The additional citation for the NACA-22 airfoil was added. The notation for the TU Delft airfoil was modified from DU97W300 to DU97-W-300 which is the notation used within TU Delft.

page 5, line 131: *Is there an explanation why not a GC2 continuity is targeted. Es- pecially in the leading edge region a curvature continous shape would provide smoother pressure distributions.*
This may be taken into consideration for further publications. Nevertheless, the pressure distribution shown in the paper do not show any irregularities possibly because a smoothing step is performed during the meshing routine. Further, the same shape parametrization was also used by Zahle et al. (2012) and Gaunaa et al. (2012).

page 6, line 147: *Please state what the authors assume to be a "reasonable" mesh resolution in more detail*
The mesh resolution is described later in the article on a case by case basis. For the evaluation of the new designs around 300 points were used along both slat and main element surface, y-plus was kept below 1 and the wall expansion ratios of below 1.2 were used for the structured part of the hybrid mesh.

page 6, line 151: *It is unclear, where a local thickness is imposed.*
The local thickness is defined as the thickness at a specific chordwise station of the profile. It is measured perpendicular to the chord line. The exact location of the local thickness constraints where applied are described later on in the template, namely at 15 % and 40 % chord length.

page 6, lines 161-164: *As the optimization framework and algorithms are not described in detail, proper reference and citation has to be given.*
Proper citation is given to the GitHub page of the optimization toolbox that is used. A link to the documentation of the toolbox was added in the bibliography. Additionally, a reference to a paper that explains the algorithm is added.

page 7, lines 173-174: *provide citation of the reference to the codes used. For Open-FOAM make sure to refer also the code version and check-out date as opensoure software tends to be changed very rapidly, but the reported results shall be reproducable.*
OpenFOAM-plus, version 1806 was used to obtain the results. For MSES a modified inhouse version was used. The references for OpenFOAM and MSES were already there, but in the fluid model validation section only. These were added in the framework description as well.

page 7, line 176: *Please refer to the airfoil correctly. The airfoil is called NHLP 90 L1T2 (see Woodward Lean, AGARD CP515) and it has been published by Moir as test case A2 for CFD validationdescribed in AGARD AR 303. The correction shall be propagated throughout the manuscript (e.g. page 8, line 201, caption of fig. 4 on page 9)*
This has been corrected in the entire draft.

page 7, line 182: *Are the six chord lengths sufficient in the view of the authors to eliminate effects on the boundary condition - or is there a vorticity correction at the farfield boundary employed?*
The sensitivity to the domain size was checked and it was bascially non-existant for the given domain size. Drela himself notes in the MSES manual that: "It must be stressed that the exact values of these grid parameters are not very important, since high-order vortex+doublet farfield representation makes the solution extremely insensitive to the

location ofthe outer grid boundaries." A note was added in the revised draft to include the vorticity correction at the farfield boundaries: "The mesh farfield distance was set to six chord lengths around the airfoil and a vorticity correction is used in the farfield."

page 7, line 191: *It is stated that O-mesh topologies are applied although Pointwise is used. Please state, why not a C-mesh is used that would allow an improved capturing of the slat and main airfoil wakes.*
This is a good remark and should be considered in further publications. However, in this paricular case with the automated meshing procedure using an O-mesh was more practical. Further, a thorough mesh sensitivity study was carried out.

page 8, line 200: *The authors suspect the experiment to be the reason for the deviations, but it could be the missing resolution of the airfoil wakes, too.*
At this point we cannot rule it out completely, but we have performed simulations on the main element only with an O-mesh at a Reynolds number of 2 million and we were able to match the experimental drag and lift coefficients below stall very well. So it seems unlikely that the O-mesh configuration is causing this large discrepancy.

page 8, line 203/204: *This is a mistake. The Reynolds number in high-lift mulit- element airfoil cases is based on the "clean chord", which is the cruise airfoil with high-lift system retracted.*
This was a mistake in my reporting, because I did not run the simulations myself. I have corrected it in the text after confirming with the coauthor and checking the original paper.

page 8, line 204/205: *This is another - more common - mistake. Although the Mach number is relatively low, a look on the pressure peaks of this case unveils that the slat suction peak (although not shown here but reported in AGARD AR 303 or AGARD*

CP 515) gets into sonic speed conditions! Therefore, the choice of an incompressible solver for this airfoil is more than questionable.
Indeed, the choice of an incompressible solver for this benchmark is not entirely proper. Nevertheless, this is only a validation case and a satisfactory match between experiment and numerical predictions is obtained.

page 8, line 208: *The over prediction of the stall angle by 6° seems pretty large as the main motivation of the work is based on the prediction of the stall delay by a slat which is mainly the shift in stall angle.*
This is a well known shortcoming of RANS turbulence modeling, but at this point higher-fidelity simulations are too expensive to be used for design cases. Nevertheless, we assume that at least the tendencies - so the sensitivity of lift and drag to changes in the profile shape - are somewhat captured and this is what is important for design optimization.

page 9, line 214: *The conclusion that MSES can be used as a substitute for RANS CFD is weak and not supported. MSES is not able to capture confluent boundary layers at all. Due to the small gap and since the optimum slat position is very close to the position where the confluent boundary layer gets dominant (see Woodward and Lean, AGARD CP515, 1993) an optimization procedure neglecting this effect is likely to predict gaps that are too small.*
The claim that MES is a substitute is based on empirical observations made here, and is not generalizable to other designs more typical for Aerospace applications, in particular with respect to gap width. We have weaked the statement in the main text a bit. Furthermore, the gap width of the obtained designs tended to converge towards the upper bounds, hence confluent boundary layers are not a concern here (even though MSES can not model them). Plus, we use CFD which can predict confluent boundary layers for the performance assessment post-optimization. So if the optimal

gap width obtained from the optimization using MSES was to small, the CFD analysis would make that clear.

page 10, line 223: *It is fully unclear why the most sensitive parameter for slat design - the gap - is fixed at the beginning. Additionally, the chosen values seem large. According to Woodward and Lean (1993) an optimum gap is strongly depending on the slat angle and can go down to 2-2.5% chord length. In the further (line 230 and following) the reason for the change in performance is most likely more related to the slat angle than the gap. It is consitent, that the optimal slat deflection angle is lower for the higher gap. At least concerning lift, it does not seem that a maximum lift coeffcient is clearly detected.*
Initially, for the preliminary assessment, we also tried to fix the chord length and leave the gap width variable. However, this just resulted in the gap width converging to the upper bound of the gap width. Then, for the actual design cases, the gap width was initially left variable, but the optimal gap width tended to converge to upper bounds as well. Hence, at some point in order to save on computation time it was just left fixed. But we agree, that indeed the gap width chosen for the preliminary optimization are large. Nevertheless, for the actual designs a gap widths of 2 and 4% were used, respectively.

page 10, figure 6: *It is not consistent (and not expected by the reader) to show MSES results in these diagrams. Above it was mentioned, that the designs were optimized by MSES but the performance prediction for the evaluation is done with RANS. Especially, there is no clear max. lift coefficient prediction in the shown data.*
Since this is only a preliminary assessment neither maximum lift nor RANS results are presented.

page 11, line 258ff: *an important description needed to understand the figures and*

the conclusions should not be placed in an appendix.
The figure was moved into the main text.

page 12, figure 8: *This figure is a collection of all optimization data. It is not very explanative as it overlays too much information. It contains already data (of the integral design) that hasn't yet even been introduced and is decribed much later. This figure should be divided for the different design methods and commented accordingly in the text.*
The figure contains a lot of information such that comparison between the different configurations and the different design procedures is possible. Hence, we propose to leave the figure as is. But we have added a note in the main text to clarify that some of the information in the figure will be discussed later on.

page 12, lines 263-265: *This would be a good option to highlight a common result with previous work (see General comments). This result is also in line with the results obtained by Manso Jaume and Wild for the superimposed slat optimization.*
This is a good remark. An additional section was added (already printed in the general remarks section).

page 12, lines 263-265: *The statement on the sensitivity of separation on the slat shape is not supported by theory. In constast, a cambered plate is less likely to separate at high angles of attack than a flat plate. Additionally, closing the gap increases the slotted airfoil effects in both directions. In fact, as the slat is moved veritcally, the Slat Effect and the Circulation Effect are expected to get stronger. Only the Dumping effect is expected to be reduced due to the reduction of the suction peak due to the strengthened Slat Effect. In consequence, the slat load is increased (higher circulation and higher trailing edge pressure) resulting in a more cambered airfoil to be more suitable to achieve the circulation without separaton. To verify this, a comparison*

of the pressure distributions is needed. This conlclusion has therefore to be reworked.
The statement was reworked on (line 287ff, page 13): "Aerodynamic theory indicates that reducing the gap width while avoiding confluent boundary layers leads to an increase in the coupling between the slat and the main element: the slat and the circulation effect are expected to get stronger whereas the dumping effect may be a bit weakened. However, the optimized slat for the lower gap width is less aggressive and the configuration produces less lift, has lower glide ratios and stalls roughly at the same angle of attack. [Nevertheless, this agrees with the literature...]". Indeed, the designs resulting from the reduced gap width are less aggressive which is contrast to the expected increase in the positive coupling between the slat and the main element. Some remarks about this have also been added in the conclusion. It may be interesting to do further investigation into this in a follow-up study.

page 13, figure 9: *The line legend of figure 9 introduces an undescribed configuration A* in addition (same for fig. 14, and in figs 15-17 configs B*, C*, D*). The meaning and origin is perfectly unclear. It can only be assumed from later reading that this configuration related to the integral design that is described much later (starting from page 15). The pressure distributions shown in the right hand side are not discussed at all in the text.*
The legend text has been adjusted to include also the A* configuration.

page 14, line 298: *To be precise, none of the airofils is optimized for maximum lift coefficient. The airfoil optimization only targeted a high lift coeffciient at a high angle of attack (here AoA=*20° *). An airfoil stalling at* 19° *could have a higher max- imum lift coefficient than one not stalled at* 20° *. To do a maximum lift coefficient optimization it is necessary to detect the naximum lift coefficient of an airfoil by varying the angle of attack.*
The wording has been adjusted to say maximum lift at the design angles of attack.

page 14, line 310: *Here it is stated that experimental data for the clean airfoil would be available for comparison. Such a comparison would have been an asset in section 3.1 regarding the validation of the methodology.*
We already present two validation cases for multi-element airfoils, we consider this to be sufficient for the publication. Otherwise, the length of the paper is excessively increased.

page 15, line 318 *it should be highlighted that - in contrast to the integrated design work of Manso Jaume and Wild, where the suction side contour of the slat is the contour of the original main airfoil - the integral design here is not restricted by the clean airfoil shapein the same way. This underlined the originality of the present work and its relation to previous work.*
The following adjustment was made in the introduction section: Second, the results of an integral design procedure are shown for thick main elements up to 50 % using a variable spline discretization for both the slat and the main element contrary to the simpler parametrization used by [citation].

page 17, lines 334/335: *It is mentioned that the main airfoil shape is a consequence of the structural constraints. But it is more expected that this is an implicit result of the main airfoil shape optimization. Due to the higher curvature, the suction peak is more locally concentrated (improving the Dumping Effect and stabilizing the slat flow) and the trailing edge position therefore placed close to the maximum curvature - which is now much furhter upstream. The intragrated design by Manso Jaume and Wild shows a similar main airfoil shape, and there, no structural constraint has been imposed.*
While the structural constraints did not have much of an influence on the leading edge and the suction side, the pressure side looked very different without the structural constraints. Namely, the maximum thickness was very far forward followed by a steep

decrease in thickness. Hence, we added this remark to the main text. The comparison to Manso Jaume and Wild with respect to the shape of the main element was also added in the main text.

page 17, lines 358-360: *The discussion describes a "larger leading edge radius". It would be better to descirbe the leading edge as "more blunt". Further on, it is not a larger leading edge radius that shifts the suction peak to the front. A larger leading edge radius alone would reduce the suction peak but not move it. The present description is misleading as the curvature which is responsible for the interaction with the slat is higher (and the radius smaller) and imposes a reduced pressure at the slat trailing edge.*
Indeed, a larger leading edge radius for the same maximum thickness (location) would have been more accurate. We replaced it to say more blunt.

page 17, line 364/365: *It is mentioned that the design angle does not account for lower side separation. This is correct, but anyhow, no clear indication of a lower side separation is seen for the designs with slat even at lower angles of attack.*
The reasoning there was incorrect. We removed the sentence. Since in fact, the shape of the main airfoil obtained from the integral design procedure are less likely to separate on the pressure side than the aft-loaded reference wind turbine airfoils.

page 17, lines 367/369: *This is a very late explanation for a figure that had been placed on page 12. It is necessary to split-up fig. 8 and to place the related illustrations close to the dicsussion in the text.*
Again, splitting up the figure makes it more difficult to compare the trends. But as already mentioned we have added some clarification in the main text when the figure first appears.

page 17, lines 371: *As already discussed "more rounded" suggests a smoother curvature distribution, while the opposite is the case for the integral designs.*
This has been corrected to more blunt.

page 18, lines 374/375 (more likely lines 369/370): *This conclusion is in contrast to a previous statement, where it was concluded that the optimal shape of the slat is not as sensitive to the main airfoil optimization and by this not as affected of the auxilary or integral design method (page 17, lines 356/357).*
Well, we said the shape of slat is not very sensitive to the main airfoil, the location and to some extent also the orientation is. But we have clarified the wording on (page 18, line 375-377): "First, the shape of the slat element excluding angle and position is not very dependent on the shape of the main element, but highly dependent on the optimization boundary conditions.".

page 18, lines 383-385: *Basically, to show the stalling behavior it is necessary to look at the pressure distribution just at stall onset. Best is a comparison with a very low AoA step before and after maximum lift. The used step of $8°$ is too large and - depending on the stall onset angle - the stall is developped over the entire configuration but not showing the onset (main wing or slat or both at the same time).*
That is why the flow fields are shown in the appendix. However, I added two plots with the pressure and skin friction coefficients in the main text. The following answers will also clarify a bit further.

page 18, line 387: *It should be discussed, whether the reattachment due to wake displacement is in accorcance with Snith's 4th effect (Off-Surface Pressure Recovery)*
I do not understand your argumentation here, please explain further.

page 18, lines 387-389: *As the stall onset is of primary interest, the discussion of the flow fields should not be placed into an appendix.*
I left the flow fields in the appendix, but I added two plots with pressure and skin friction coefficients before and after maximum lift in the main text.

page 19, line 393: *At least here at the end of all shown pressure distributions, it is necessary to conclude about the suitability of the incompressible solver. Although the flow speed is not mentioned (missing in the case description in Table A.1) the level of the pressure coefficient is less than -15 and imposes the need to check whether this assumption is still valid.*
I calculated the free stream Mach number for the NREL 5MW turbine up to about 40 % span and it is below 0.1 which would result in a Glauert correction factor of $c_P/c_{P0} \approx 1.005$. Hence, we did not consider compressible effects, since this is also usually not done for wind energy applications. But I also reran some of the cases with not just accurate Reynolds number, but also accurate Mach number scaling using again the incompressible solver. Then, I checked the maximum Mach number as predicted by the incompressible solver and indeed for the clean cases at angles of attack with lift coefficients above 4, the local Mach number at the slat suction peak approach 0.45, which is not ideal. However, realistically speaking given that RANS overpredicts the stall angle, I don't think that these conditions will actually be reached. Nevertheless, I added this paragraph before the conclusion: "Some of the designs show very high suction peak values for the slat which is an indication that locally compressibility effects may not be negligible despite the low freestream Mach number of $Ma_{\approx}0.1$. Calculating the local Mach number from the incompressible flow field for all the CFD cases shows that for the designs and angle of attack configuration where the lift coefficient is higher than 4 the Mach number locally approach 0.45. This is indeed very high and it is recommended that in future publications, compressibility effects should be considered if a design optimization for high lift is carried out. Nevertheless, given that turbulence model of the CFD solver is expected to overpredict the stall

angle, it is not certain that such high Mach number will actually be reached in real life." I also added the freestream Mach number to the table with the design parameters.

page 19, lines 395-397: *The description reverses cause and effect. The high suction peak on the main airfoil is the reason for the low trailing edge pressure at the slat - remind Smith's effects.*
The sentence was reversed.

page 21, line 428: *To conclude on the importance of the gap it should have been used as a design parameter. The limited information on the gap variation (two values only) doesn't allowto draw such abbreviation general conclusion. From other airfoils in literature it is known that the gap is even the most sensitive parameter.*
The sentence was modified to: "A reduction in the gap width did not offer any benefits, but only two gap widths were investigated. Possibly, the sensitivity to this parameter warrants further investigation." But as already mentioned, in initial investigation whenever the gap width was left variable the optimal design would converge to the upper bound even when using bounds up to 10 %. Hence, at some point it was fixed to keep structural loading in check. Also, when looking at the references from Manso Jaume, Schramm, Zahle/Gaunaa (also unpublished work), Pechlivanoglou and Schramm gap widths of the same order of magnitude were used (or obtained from optimization).

Reply to technical comments
All the remarks were implemented. With the exception of multi-figure captions as the draft template explicitly asks to remove them. But then later on add them in the full publication. Then the Subsection formation is according to the template guidelines.