General comments

The work described in the paper is a very interesting design approach. The work as such is done in a good scientific manner. The wording is clear and good to read and understand. The manuscript is clearly improved by the revisions made by the authors.

replies to author's response & specific comments to revision

- 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."

  The description of the Dumping Effect still needs a slight improvement to be fully correct: "The circulation around the main element <u>induces</u> a low pressure region around <u>the main element leading edge, and thus at</u> the slat trailing edge <u>located in its vicinity</u>.

- 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.

  performing a mesh sensitivity study will not unviel the specific difference of the meshing approach. An O-mesh will never be able to resolve the wake over a long distance. It's o.k. for the present work but keep this in mind for future studies.

- 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 4between experiment and numerical predictions is obtained.

  It is still recommended to delete the sentence refering to Sorensen, which is now on page 9 in line 213/214. Or replace the two sentences by something *like* " Although freestream Mach number is low, comperssibility of the air flow may effect the slat flow due to the high acceleration of the flow. Nevertheless, as will be seen later on, good agreement between experimental and numerical results is obtained by an incompressible solver."

- page 8, line 208: *The over prediction of the stall angle by 6o 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.

  A disagreement of 6° in stall onset prediction cannot be "satisfactory". It is still recommended to either replace "satisfactory" by something weaker and to delete the "stall" in the sentence. Acceptable can only be the lift prediction in the linear incidence range

- 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 oundary 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.

  Still, a "substitute" would provide equal qualitiy results. Instead, MSES in this case is expected to provide similar trends (meaning sensitivities not values) at less effort and acceptedly less accuracy - in practice a "lower order" or "low-fidelity" or "fast prediction" method

- 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 doesn't 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.

In this case, this should be mentioned by a sentence in the text: "initial experience showed that the gap converges to its upper bound". It is better to not give the impression, that the fixation was made by luck.

- page 18, line 387: *It should be discussed, whether the reattachment due to wake displacement is in accorcance with Smiths 4th effect (Off-Surface Pressure Recovery)*

  I do not understand your argumentation here, please explain further.

  I refer to the sentence "At some point, the slat wake extent grows so much that the low-pressure area in the wake leads to reattachment of the flow on the main element." It has been well explained by Smith that the pressure recovery in a free shear layer wake flow is more "efficient" than in a wall attached boundary layer. While running into the pressure rise, the kinetic energy of the weakest area in the wake can be eaten up even before the pressure has completely recovered to infinity static pressure. If the shear layer is wall attached, this leads to separation. If it is a free wake, the flow stops and a stand-still area of the flow is observed. The further pressure rise is then solely obtained by pressure diffusion. In consequence, the resulting dead-water area displaces the flow leading to a lower pressure above the wing. This in consequence relaxes the pressure gradient along the main wing and the flow stays attached. In highly affected cases, this can be observed when plotting the trailing edge pressure over the angle of attack. There you may detect a reduction of pressure once the effect sets in.

- 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.*

  ... However, realistically speaking given that RANS overpredicts the stall angle, I don't think that these conditions will actually be reached.

And this is exactly the point I wanted to make. Don't think that this is out of reality. We observed even transonic flows at slat devices at onflow Mach number of M=0.2. Anyhow, the text addition made in the revision is clear and accepted