

Interactive comment on “The flow past a flatback airfoil with flow control devices: Benchmarking numerical simulations against wind tunnel data” by George Papadakis and Marinos Manolesos

Anonymous Referee #2

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The primary purpose of the paper is to describe a computational study of the effects of various trailing edge passive flow control devices for a flatback airfoil, including an investigation of the physical processes involved. A secondary purpose is to validate several CFD turbulence modeling approaches for this flow by comparing results to experimental data. The paper is mostly successful at both, although some important details are neglected.

Specific Comments: 1. It is not clear how the trailing edge devices were included in the CFD model. Was the grid modified to wrap around the new geometry? Or is there an immersed boundary technique applied? The authors should strongly consider showing

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at least one example of a mesh for one of the device configurations.

2. IDDES is named as the hybrid RANS/LES turbulence model, but then this is abbreviated to DDES. This creates confusion as to which model was actually used and how it was applied. IDDES is capable of modeling the outer part of attached turbulent boundary layers in LES mode, while in DDES the attached boundary layers are always modeled in RANS mode. IDDES and DES have different model equations. Please clearly state which model implementation was used and, if IDDES, whether the attached TBL regions were quasi-steady RANS regions or LES (I strongly suspect the former, given the stated grid resolution).

3. At $Re_c=1.5e6$, one might expect boundary layer transition to play a key role in predictions of lift and drag, and possibly the wake region. How was transition handled?

4. It would be very illuminating to perform at least one simulation at the experimental aspect ratio, to study any end effects, if present. Absent this, quantifying the span-wise correlation length of velocity fluctuations in the wake would give confidence that the span-wise extent of the domain is long enough to at least approximate the large-aspect ratio case. Another way to explore this issue would be to see if spanwise periodic BC's give different results?

5. I had difficulty reconciling the high experimental wake fluctuation amplitude with the modest experimental Reynolds stress field for the flap-only case.

Technical Corrections: 1. Sentence on "misalignment" on the bottom of page four is unclear. 2. Are there any experimental measurement uncertainties available to improve the validation exercise? 3. The term "loads" is used to describe mean aerodynamic loads, which may make sense to the wind energy practitioner. However, loads can also be unsteady so consider using the term "mean loads".

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