

Wind Energ. Sci. Discuss., referee comment RC2
<https://doi.org/10.5194/wes-2021-45-RC2>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on wes-2021-45

Anonymous Referee #2

Referee comment on "2D Numerical Simulation Study of Airfoil Performance" by Nasser Shelil and Fahad Awjah Almehmadi, Wind Energ. Sci. Discuss.,
<https://doi.org/10.5194/wes-2021-45-RC2>, 2021

The manuscript describes a series of numerical simulation of a single wind turbine airfoil. Although the setup tries to match current practice, it seems that it falls short behind its ambition. The description reads like it is a study on the impact of several control parameters of the specific numerical solver. It is emphasized more than ones that majorly the selection of the turbulence model is one of the major results. In the text then, parameters like air speed, air temperature and turbulence intensity are outlined - unfortunately without any reflection of their physical relevance.

Anyhow, there is some doubt on the numerical setup. The provided mesh images don't show a mesh that is reasonably capable to capture viscous boundary layer effects. Judging on turbulence model by lift and drag coefficient only is much too crude. It neglects the option of compensation of local errors in integral values. For such an analysis, at least pressure distributions, even better velocity profiles normal to the wall must be analysed and compared. And - aside that an inviscid Euler simulation is not a turbulence model - the paper fails completely to explain, why all RANS simulations are by 27% far off the experimental data, while an inviscid one is close by.

At the end, in the conclusion mainly standard knowledge on airfoil characteristics is provided. Such knowledge has been published in summary long before (e.g. Aboot & Doenhoff 1949). It doesn't justify a peer review publication. For such, the study must be more focused on the turbulence models with a proper detailed comparison including pressure distribution, velocity profiles, separation onset. And, it must provide more conclusive results than those which have been cited. The grid convergence study has to be quantified (e.g. Richardson extrapolation, $y+$ -distributions) as there is strong doubt on the suitability of the meshes as plotted.

For detailed comments see the attached PDF. It is strongly required to perform a language check.

Please also note the supplement to this comment:

<https://wes.copernicus.org/preprints/wes-2021-45/wes-2021-45-RC2-supplement.pdf>