Wind Energ. Sci. Discuss.,
doi:10.5194/wes-2017-6-RC1, 2017

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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Full HAWT rotor CFD simulations using different RANS turbulence models compared with actuator disk and experimental measurements" *by* Nikolaos Stergiannis et al.

Anonymous Referee #1

Received and published: 20 March 2017

The authors present a study of RANS simulations of the double rotor experiment of NTNU. Results are presented using blade-resolved simulations in a rotating reference frame, as well as using an ADM. Four different eddy-viscosity models are compared. Unfortunately, I cannot recommend this manuscript for publication. First of all, the authors fail to provide a decent literature survey on blade-resolved simulations of wind turbines. In fact, they do not cite a single reference (making it appear as if they are the first to do blade resolved simulations), though current state-of-the art include, e.g., adjoint-based aeroelastic rotor resolved optimization (see, the work of Mavripilis at Wyoming – the same group also performs wind-farm simulation using full rotor models),

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Discussion paper

the work of Bazilevs (2011; full aeroelastic rotor simulations), the work in Stuttgart on blade resolved DES, just to name a few. Moreover, given the current state of the art in this field, I fail to see what the innovation is in the current manuscript. The authors do not identify this: there is no real research question or hypothesis, and consequently no relevant conclusions follow. Moreover, the one conclusion they formulate (k-omega models are the best), is not really substantiated as they do not include tower or nacelle in their simulations. Finally, there are many inaccuracies, and trivial statements – see further below for some further details.

Some more detailed issues

- page 1, line 21: don't understand that phrase. . .

- page 2, line 3: 'wake effects can cause total annual power losses up to 30%' What wind farm are you talking about; give reference and details. Seems rather big (unless maybe extreme case such as Lillgrund)

- page 2, line 18: 'it is common and acceptable to use, with certain limitations, the assumption of steady state approach to resolve a time-varying (unsteady state) problem when one is interested in the mean values of the flow.' What do you mean by that? What limitations? Please substantiate

- Eqs 1 and 2: these are not the correct RANS equations

- Section 2.4: belongs in appendix at best

- page 5, line 18: 'However, in operating conditions, the flow across the rotor is very complex, . . ., and also other flow characteristics related to non-uniform inflow conditions, atmospheric boundary layer and so on'. True, but last situations also do not occur in you blade-resolved simulations (where you use uniform inflow); moreover, they could be accounted for in ADM, e.g., the rotating ADM by the group of Porté-Agel.

- page 10, line 5: '. . . approximately 6000 iterations and were run for 10000 iterations'. Really?? And that for a residual reduction of only 4 orders of magnitude?? At this number of iterations, why not do DES – these would typically require between 10000 and 100000 time steps?

- connected to previous point: what is the computational time, number of processors, possible parallel efficiency, etc.

- page 10, line 7: 'Full rotor simulations are capable to represent the flow field close to the wind turbine (Fig. 3), but they are over-estimating the velocity deficit at the far wake (Fig. 9)' Why? No real explanation seems to be given in the paper

- number and order figures according to occurrence in the text

- page 11, line 12: 'are in agreement with the contours of vorticity (Fig. 7a) in which the strong effect of the shear stresses at the edge of the disk area is apparent' I expect the effect of shear stresses only to become apparent more downstream – near the disk, solution will behave more like potential flow.

- page 11, line 16: what recirculation zone – do you find a recirculation zone near the ADM? Did you correctly implement the model?

- page 11, line 18: '. . . is a strong connection between the production of turbulence and the wake recovery' Sorry, but this is really a rather trivial statement

- page 15, line 8: '. . ., we expect equal production of turbulence from the two rotors'. How so? Can you explain why you expect that? Not clear to me, since shear as well as turbulence levels of at second turbine are different, so also production will be different

- why not show comparison with experimental results of the second wake? Why also not consider the closer spaced cases?

- page 18: it is concluded that the k-omega models are best. Unfortunately, no tower or nacelle is modelled, so not sure that this conclusion will hold once you would add them

- page 18, line 17: '. . . ADM was observed also by other studies (. . ., Sanderse et al

2011, . . .)' Sanderse et al is a review paper and not an ADM study

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