Review of

"Validation of the actuator disc approach using small scale model wind turbines"

by N. Simisiroglou et al.

(wes-2017-18)

June 24, 2017

1. General comments

The paper compares wake flow and thrust predictions by an actuator disc approach with experimental data from NTNU's Blind test experiments. Specifically, the performance of four different turbulence closure models and five radial thrust distributions on the actuator disc are investigated. The model turbines' performance is analyzed for a single and double turbine setup and different turbulent inflow conditions.

Given the advantages of lower computational effort compared to LES/DES models or fully-resolved RANS model, this approach is considered to be relevant for a fairly accurate modeling of wind farm flows.

The paper follows an elaborate line of reasoning and brings up a number of clear conclusions. However, the manuscript is missing some crucial elements. A comprehensive literature review on the field of wake modeling is not included, neither are the results discussed with respect to the state-of-the-art in wake modeling Due to the lack of a wider context the current manuscript does not yet clearly demonstrate the advantages of the chosen modeling approach over other methods.

2. Specific comments

1. Firstly, I do not completely agree with the chosen structure of the manuscript. The content of chapters "3 Results" and "4 Discussion" is complicated to follow in the current structure.

   (a) I would suggest creating a new chapter 3 called "Precursor simulations" in which the empty tunnel simulations as well as the grid independency study are shortly described. The empty tunnel simulations (Fig. 3 and Fig. 9) as well as the grid independency study (Fig. 2) are rather boundary conditions than actual results in my opinion. Probably, it is not even necessary to show all modeled and measured inflow profiles rather than shortly mentioning that the match is very good (and stating a deviation in %).

(b) In my opinion it would be more straightforward to discuss the results in the actual "results" chapter rather than separating results and their discussion.

(c) A "discussion" chapter, however, still would be essential to include. Therein, the main findings should be discussed with respect to previous findings in the literature. So far, there is only one reference (Laan et al., 2015) included in the discussion, but there is a huge variety of publications dealing with numerical wake simulations by now. It would add great value to the manuscript to discuss the observed effects with respect to other simulations (ACD, but also other RANS approaches (ACL or fully-resolved)).

(d) It could be useful to create a new chapter "Conclusions", starting from l.34 on p.16.

2. Secondly, several aspects of the thrust modeling require some deeper explanation. As the variation in radial thrust distribution is one of the two major parameters varied in this study an in-depth explanation of its modeling is deemed to be crucial

   (a) A more elaborate description of the choice of thrust distributions and the associated parameters is needed. A plot showing C_T (or a) vs. r/R comparing the distributions given by the equations in Table 2 would help to illustrate the approach. How are the parameters b (Table 2) chosen?

   (b) The distribution of the axial induction (or thrust) is not necessarily uniform along the rotor radius, depending on the rotor design and operational state. However, it should be possible to calculate radial distribution of the axial induction factor a and thus the thrust for a given rotor design and operating point. A simple Blade Element Momentum code or turbine modeling tool (FAST, QBlade, ...) should do the job, if the rotor geometry and airfoil polars are available. To my understanding it thus should be possible to define a thrust distribution and eliminate it as a variable.

   (c) Furthermore, It is not clear to me, how the downstream turbine's thrust coefficient $C_{T,T2}$ is calculated in cases B and C. Is it calculated from the fluid-ACD interaction or is the experimental $C_{T,T2}$ value used as an input? Please elaborate on the very short explanation given in l.5, p.7. See also comment 3 (c).

   (d) Can you elaborate on what is meant by "undistributed" thrust? I did not find an explanation on that.

3. Finally, the scientific contribution of this work to the field of numerical wind turbine wake modeling should be stated in a clearer way.

   (a) Elaborate in the introduction why you chose the presented modeling approach. What is the advantage of RANS-ACD modelling compared to other numerical modeling techniques (LES/DES, ACL, ...)? Can you present some numbers justifying this approach with respect to computational effort (time)? Would this modeling approach thus have significant advantages in the modeling of a full wind farm?

(b) As stated in comment 1 (c) already, a discussion of the presented results with respect to state-of-the-art numerical wake modeling is deemed to be crucial. A discussion of both the approach and results by referring to other simulations would set this work into a broader context.

(c) The presented simulations of mean velocity and turbulent kinetic energy show very promising results, especially those simulated in a highly turbulent environment. However, I do not understand how the upstream turbine's $C_{T,T1}$ and especially the downstream turbine's thrust coefficient $C_{T,T2}$ are dependent on experimental information. For the modeling of a bigger wind farm, it would be important to be able to calculate the coefficients based on the information given in turbine data sheets only.

(d) Finally, it should be stated if and how the presented modeling approach is reliable with respect to simulations of other wind turbines and different wind conditions. Which part of the modeling still comprises uncertainties? What would be suggestions for further developments on the proposed modeling?

3. Technical corrections

- p.1: Abstract: state in one sentence which turbulence model performed best under which flow conditions.
- p.1, l.17: "... CFD code and to the..."
- p.1, l.19: "... large wind turbines...", specify what "large" and "small scale" (l.21) is. $D = 10m$ are still model scale
- p.2, l.25: "... design conditions." Specify what these design conditions are (TSR=?)
- p.3, l.21: "... created by a bi-planar..."
- p.5, l.28: "... a triangular and a trapezoidal distribution." (word trapeze/trapezoidal reoccurs at several places in text an tables).
- p.6, Table 2: as mentioned above: a plot showing the different distributions would be illustrative.
- p.7, Table 4: as mentioned above: what does "undistributed" mean?
- p.7, l.1: " ... ACDs are?"
- p.7, l.4: "... thrust coefficients $C_T = (...)$ wind turbine are..."

- p.10, Fig.5 (a) and (b), p.11, l.5 and p.13, l.4: it is first stated that the k-epsilon and the KL k-epsilon model produce similar results on p.11, while on p.13 it is stated that the RNG k-epsilon tends to underpredict the wake recovery. Judging from Figures 5 (a) and (b) I hardly see any difference in the results by the k-epsilon and RNG k-epsilon model.
- p.13, l.1: it is stated that the TKE profiles in Fig. 5(b) are "not successfully predicted by any of the turbulence models". Could you elaborate on reasons for this giving a source from literature? Is this due to the weak performance of RANS in low turbulent environments? Is there an influence of the non-existence of tip-vortex-shedding in ACD models on the TKE profiles?
- p.14, l.1: "...capture the position of the tip vortex apart from the polynomial." Why are there several peaks appearing in Fig. 6(b)? Can you double-check for convergence?
- p.16, l.22-25: "As the purpose (...) from representing differently (...) using different turbulence models." This is a very long and hard-to-understand sentence; especially the "representing differently" part needs revision.