2nd review of "Hai Bui et al. Implementation of a Simple Actuator Disc for Large Eddy in the Weather Research and Forecasting Model (WRF-SADLES-V1.2) for Wind Turbine Wake Simulation.

Summary

The authors implemented substantial changes in the manuscript. Overall, the manuscript has been improved. However, a few critical changes and a few other minor changes should be done before publication. Some of the points were already mentioned in the 1st review but not corrected properly.

Critical comments (general):

- In the AD+R and ALM the momentum theory is not used, because this part is taken care by the CFD simulation itself (corresponding mainly the effect of the turbine induction), and therefore it is wrong to say that they use the BEM theory (where the M stand for the momentum theory), they only use the BE theory. In the text, many wrong references to « BEM » are still present. This was already discussed in the 1st review. The text must be modified accordingly, e.g. lines 25,63,212,255. Also, « BE theory » seems a better formulation than « BMT » as proposed line 25.
- The idea that the simple AD (SAD) has weaker resolution requirements than the AD+R is not supported by the literature. In fact, the authors already agree with this, since they write « at least a few grid points across the rotor » for the AD+R (line 64), which is exactly the resolution targeted for the SAD in this paper (tens of meters). This argument can therefore not be used. The argument regarding the availability of the turbine data (line 65-66) is correct and sufficient. The text must be modified accordingly, e.g. lines 72,474.
- Multiple studies have shown that the rotation is not critical in LES with Actuator Models. This was already discussed in the 1st review. The critical difference between the SAD implemented here and the AD+R used in the PALM simulations is the force distribution on the rotor (homogeneous or heterogeneous respectively) and the way these forces are computed (using the thrust coefficient or the BE theory respectively), not the rotation. A SAD like model can have rotation included using the power coefficient, and we could imagine an Actuator Disk model based on the BE theory but without rotation (so similar to the AD+R of PALM but without rotation). The text must be modified accordingly, e.g. lines 114-115,246-247,477-478. Line 242, remove « (e.g. simple actuator disk versus actuator disk with rotation) », not needed and most likely not the most important point.
- The paragraph lines 111-116 is still very confusing. Referring to my 1st review: « The idea expressed in lines 113-118 [lines from V1], that the rotation affects the wake recovery which justify the use of an additional subgrid-scale turbulence term since the rotation is not explicitly included, is not correct. ». In fact, the authors already agree with this, in lines 443-454. In this context, lines 443-448 are well adapted and lines 111-116 should be adapted using lines 443-448.
- I would suggest moving lines 443-448 to lines 111-116, lines 449-454 at line 298, and lines 455-467 at line 397 (with the appropriate modification needed).
- In section 3.2, Lines 228-252 and 265-279, make strong statements which are difficult to justify based only on contours, e.g. « slower wake recovery » line 231. Profiles are better suited for these types of statements. To solve this issue, the profiles (Figs 5 and 7) could be presented at the same time as the contours (respectively Fig 4 and 6) and discussed together. Also, the profiles for the induction optimisation should be added (contours could be removed if needed, profiles are more important than contours for a precise discussion). Fig 3 and 4 could be merged and discussed all together.

Critical comments (specific):

- Lines 231-232: « indicating potentially stronger turbulence activities », in fact, it is the opposite, slower recovery and smaller rate of wake expansion are in general associated with lower background turbulence intensity. Must be corrected or reformulated.
- Line 27: « rotating circular disk », not appropriate, must be corrected.

- Line 145: « within the interval […] chosen boundaries », unclear, must be reformulated.
- Line 233-234. The idea behind the sentence is very strange formulated as such, change « which is used » with « for ».
- Lines 275-276: « shear production », not appropriate, Line 283 « shear production of turbulence », not appropriate. Must be reformulated.
- Lines 301-302: not clear how the spectra are computed, from what I understand, not sure it is appropriate for the targeted goal. Clarify.
- Lines 405-406, Fig 12: I am not convinced by the « limited resolution » argument (referring to spatial resolution I guess), the flow from the simulation looks like there is no turbulence, any comments on that?
- Lines 482-483, « independence from model resolution »: does not seem supported by the results and does not seem correct, must be changed/removed.
- Fig 1: the vertical resolution in Fig 1.a. still do not match the legend (dz=30m) and Fig 1.b. This was already discussed in the 1st review.
- Fig 10: remove « alongside», Fig 11: modify second sentence of caption, with « , data from […] » similar to caption of Fig 10.

<u>Additional comments:</u>

- Line 3: « downscaling of large eddy simulations », no so clear in the context.
- Line 20: « dynamic loading », no so clear in the context, not really needed.
- Line 36: « such as cyclones an fronts », no so relevant, not really needed.
- Lines 57-58: « (Fitch et al., 2012) only requires », replace with : « the method used by Fitch et al. ».
- Line 74: « the LES downscaling approach », formulation could be improved.
- Line 75: « necessary for generating turbulence », replace with something like « allows faster development of turbulence ».
- Lines 79-80: could be improved, e.g. with something like « an idealized case is used to compare the simulation results from WRF-SADLES with PALM ».
- Lines 89-90: does not look appropriate with Section 5. Section 6 is missing.
- Lines 97-98: « The wind speed at the rotor […] », introduce this at line 108, before actually using the wind speed at the rotor in the equation.
- L115: « we adopt », replace with « we test ».
- L 144: « improve the realism of turbulent representation », not appropriate.
- L172-173: « models, known for their realistic simulation », not clear (we could think the statement also includes WRF-SADLES), improve formulation, could be simply removed.
- L173-174: indicate also the resolution with respect to the rotor diameter », i.e. how many points per rotor diameter.
- Line 184: « an aspect ratio of 2:1 », add the directions (could be vertical at this moment in the text).
- Line 193: « zonal direction », not clear.
- Line 207: reference in bad format.
- Line 222: express time in hours like for the WRF simulation.
- Line 230: « suggesting a dependency of momentum fluxes on model solution », not sure to follow, may need more explanation or reformulation.
- Line 232: « conversely », not adapted.
- Line 241: « methodological », maybe « numerical »?
- Line 245-246: improve, e.g. with something like « PALM simulation exhibits stronger wake expansion than the WRF simulation ».
- Line 250: « but one possibility is related », not very elegant, could be improved.
- Line 252: « (2D) », remove parenthesis.
- Line 253: « agree around 50% », not adapted, remove.
- Line 280: « vertical structure », modify with « «vertical profile ».
- Line 290: « brings them closer », not elegant.
- Line 388: « LLJ) », remove parenthesis.
- Line 398: »UTC., », remove « . ».

- Line 416: « become fully developed after about two kms », justify the statement or make less precise statement.
- Line 452: « we imply », not appropriate.
- Line 465: « model crashes », not so elegant.
- In figures 4,6,7, « similar than in fig […] », copy-paste the appropriate description, each figure should be self descriptive.
- Fig 5, « evaluation point » maybe not so clear (not really a point since it's a profile), « location » could be better.
- Fig 12, « radial wind speed », not so clear to me in this context.
- Fig 14: the layout in panel in (b) could be incorporated in Fig 9, and the y axis of Fig (a) and (b) could be rescaled.
- Table 2: « real-data », not very elegant.

Remarks/questions:

- Why only potential temperature in the perturbation method and not the wind speed components?
- Why using a precursor simulation in PALM and no precursor in WRF simulation ?