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WIND ENERGY SCIENCE DISCUSSIONS

# *Interactive comment on* "Scale-adaptive simulation of wind turbines, and its verification with respect to wind tunnel measurements" *by* Jiangang Wang et al.

**Anonymous Referee #2**

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article

### Summary

The article describes the potential of using an scale-adaptive URANS model based on the k-omega-SST model as an alternative to LES for the simulation of wind turbine wakes.

The article is well written and organized. Furthermore, the topic is of sufficient scientific



relevance for the wind energy community. However, it is imperative that some important issued are addressed before this referee can recommend it for publication. My major objections are related to the lack of a grid convergence study. This makes nearly impossible to compare and interpret the results with any confidence. Consequently the conclusions of this work lack of a solid foundation.

In the following, I describe both major and minor issues that should be adressed.

### Major comments:

1. My major concern is the lack of a grid convergence study. How do you know that the mesh resolution is enough for LES? How do you know that it could not be coarser? What about the same questions for the URANS simulations? In the way you present your results, this referee can not have any confidence that your interpretation of the results is correct.

2. Because of the lack of a grid convergence study, I wonder if LES with the RANS grid would provide similar results to $k - \omega - SST - SAS$. Perhaps it produces similar results at a lower computational cost, since it needs to resolve less equations. Please clarify first the issue with the grid uncertainty, and then you will be in the position to discuss the benefits and drawbacks of the different turbulence modelling strategies. I suggest the use of Roache's Grid Convergence Index (GCI).

3. You use the Gaussian smearing factor $\epsilon = 2.5$, but you do not say why. In the literature, it is usually recommended to choose $\epsilon = 2.0$ (see e.g. PhD thesis by Troldborg from DTU). For lower $\epsilon$, the simulation usually tends to be unstable, and for larger $\epsilon$ it tends to be inaccurate. In fact, the correct tuning and setting of this parameter always requires a sensitivity analysis. Please include it.

4. Additional details on the actuator line model are required: do you use a tip correction model? If yes, which and why? What about root corrections? Which is the number of elements per blade? How is the spacing between the elements? Do you use dynamic-stall corrections? Which is the ratio between the average chord length and the cell size?
5. The $k - \omega - SST - SAS$ are partly more accurate than the LES - Smagorinsky results (e.g. line 343). How can this be? Please explain this unexpected behaviour.
6. line 384: For LES the tip chord length it 1.8 times the cell size. For RANS it is then 3.6 times the cell size? According to the recommendations from the literature, the average chord length should be around the same size as the cell size! And you are talking about the tip region, so for the rest of the blade this issue is even more pronounced! Please elaborate on this issue.
7. line 396: I am not sure about the wake behaviour you describe here. In case the rotor and tower interact in the way here proposed, why does it just move slightly upward? Actually the part of the tower shadow comprising one rotor lenght should rotate in the same way that the rotor wake does. And the same effect should be also visible in the case without yaw misalignment. How do you distinguish the tower wake from the rotor wake after they meet?
8. A discussion of the experimental uncertainty is required.

**Minor comments**

1. The nomenclature that you use for the different turbulence modelling approaches is quite misleading. What you are comparing is LES with a Smagorinsky subgrid-



scale model, URANS with $k - \omega - SST - SAS$, and URANS with $k - \omega - SST$. Please adapt the whole text accordingly.

2. The scale adaptive model SAS is an URANS model and is based on k-Omega-SST. This must be explicitly described in the paper.
3. Please explain all the parameters of all the equations.
4. Page 4, line 34: The available blade conforming approaches do NOT include LES because of its huge computational cost. The article that the authors are citing is based on DES not LES.
5. All the turbulence models that you use are readily available in OpenFOAM. Please state this information, so that other researchers can reproduce your work.
6. page 7, lines 196-198: You mention the general results of your work several times before and after this point. In this section you are supposed just the numerical mesh. Please delete this sentence and try to avoid describing the results before coming to the results section.
7. From the beginning of the article, it was clear which turbulence models you use for RANS, however it was unclear which sub-grid scale model you use for LES. I found that information in section 3.2.2 when you describe the boundary conditions, but up to that point I was wondering the whole time about it. Please state this information more clearly at a earlier point.
8. If I understand it right, you do NOT use the *dynamic* Smagorinsky model for LES. I wonder why.
9. Line 238: you use the van Leer differencing scheme and you claim that it is strictly bounded. However, I believe that the implementation in OpenFOAM is unbounded. Please clarify this.

10. Line 256: it is unclear why you assume $F_{SAS} = 2$
11. Section 4: why do not you use the same controller for the experiments and the simulations?
12. Line 358: you claim that SAS may lead to a faster wake recovery. Why “may”? Does it or does it not?
13. Fig. 5: Are the results time-dependant or averaged?
14. line 424: what about the thrust?
15. line 484: I think that you do not mean “shed” but “trailed”. Please correct or comment on this.