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Interactive comment

Interactive comment on "How realistic are the wakes of scaled wind turbine models?" *by* Chengyu Wang et al.

Anonymous Referee #1

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General comments The subject dealt with by the authors is of great interest. Indeed, few studies today attempt to highlight the validity of the results obtained in wind tunnels, which are so important in the development processes of wind turbines. The paper is very well written and very well structured. The analytical approach is very good and well explained. A real effort of pedagogy is to be emphasized on the description and explanation of the different scaling. The method used for comparison (experiment – simulation) can be debated because it is not common to validate experimental results from numerical simulation. From this point of view, although the numerical simulation is detailed in previous articles, it seems to me that its description should be substantiated here. We can also note a welcome frankness on the limitations of certain hypotheses or results obtained. It seems to me that after the answer to some questions raised

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Discussion paper

below, it is quite likely to be published.

Questions on context and methodology - The authors seek to demonstrate good replicability on a reduced scale of a wind turbine wake. The results presented tend to show good reproducibility of the main wake parameters. Nevertheless, a discussion around the minimum size of the model would have been interesting. According to the authors, it is possible to validate the different parameters of similarity regardless of the size of the model? What parameters are most likely to be sizeable?

- The authors present their results for a horizontal axis wind turbine (may be specified in the title of the article ?). Do they think that the method used and the similarity parameters are the same for a vertical axis wind turbine? Are there any other problems during wind tunnel tests for a VAWT?

Scientific comments - As the authors quite rightly point out, the Reynolds numbers on the blade are very different between the reduced scale and the full scale. However, it seems to me that the paper does not provide enough detail on this point. The authors say that the profiles work on very different regimes (line 155-165). This may be true, but the ranges of Re involved must be given. Indeed, one can obtain an independence of aerodynamic coefficients above a certain number of Re (which depends on the airfoil). In this case, the flow regimes and topology (slope, Czmax) are not necessarily different?

- According to the authors, the difference between regimes comes only from the fact that the boundary layer is turbulent or not? If so, why not use carborundum (or others) to trig the boundary layer on the blade? - Concerning the Re, the authors attribute the difference of Power Coefficient to the different values of the Re. I think it would be interesting to ensure this point that the authors show the corresponding polar (numerical and experimental).

- A strong assumption is that the validation of small-scale numerical simulation provides confidence in the results obtained at scale 1. It seems to me that this assertion needs to be substantiated. Indeed, the numerical method is presented very quickly and I have difficulty understanding how to be sure that the flow is correctly reproduced on such a large scale by LES-type methods. Quantitative information on cell size, calculation volumes and other simulation parameters should be provided. I think that's important, because simulation is here taken as a reference

- This need for precision on numerical simulation is all the more important as this simulation is used to detail the blockage and upstream turbulence effects. It is indeed known that the generation of turbulence by numerical simulations (whatever the method used) is still a topic of research in the community today.

- Line 112: The authors propose a Strouhal number based on the vortex shedding rotor. Can they explain which physical phenomena precisely corresponds to this Strouhal number?

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