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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Blind test comparison of the performance and wake flow between two in-line wind turbines exposed to different atmospheric inflow conditions" *by* Jan Bartl and Lars Sætran

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We thank the referee for his/her critical and appropriate comments. We were asked to answer all referee comments, while a revised manuscript should not be prepared at this stage yet. In the following, we will therefore engage with all the comments and propose improvements for the final manuscript.

Major comment RC1-1:
(. . .) On the other hand, the inherent weakness of these benchmarks is the difficulty

to organize the comparison process in a synthetic way, with clearly stated outcomes based on metrics, and without losing the reader in details or technical digressions. Consequently, I suggest to the authors to provide a table in §2.3, summarizing the properties of the different numerical tools (CFD methods, wind turbine model, mesh properties, blockage, etc)

The authors' reply to RC1-1:
We agree that a table summarizing the different CFD related specifications (method, turbine model, meshing resolution, blockage, turbulence model, . . .) of the different participants will improve the clarity. A table will therefore be included in 2.3 in the revised version of the paper.

Major comment RC1-2:
And in order to make the discussions on results more straightforward, I suggest to use Metrics (as correlation coefficient (R), fractional bias (FB), normalized mean square error (NMSE), geometric mean (MG), geometric variance (GV), fraction with a factor of 2 (FAC 2)) to provide a synthetic comparison

The authors' reply to RC1-2:
It is indeed a good idea to use statistical measures for the comparison of the results rather than only a qualitative description. The correlation of the modelled mean velocity data as well as the TKE data with the experimental data will be analysed using the metrics geometric mean (MG), geometric variance (GM), fractional bias (FB) and normalized mean square error (NMSE). We consider including a separate table at the end of each result section (3.1, 3.2, and 3.3) supported by some comments explaining correlation values.

Major comment RC1-3:
The inflow conditions are not representative of atmospheric flows. Indeed, the ratio between the integral length scale and the rotor diameter is not at all realistic. Conse-

quently, do not mention “atmospheric” flows but “turbulent flows” in the title and in the body of text.

The authors’ reply to RC1-3:
Your comment, that the inflow conditions are not representative of atmospheric flows, is correct. The grid-generated turbulent flows were created to highlight effects of different turbulence levels and spatial non-uniformity in the inflow. The title will be changed according to your recommendation.

Minor comment RC1-4:
- Remove Figs 1 and 3. Use Fig. 4(a) to describe the general set-up

The authors’ reply to RC1-4:
We agree that there is a redundancy in Figures 1,3 and 4. Figure 1 is purely descriptive and will be removed. Figure 3, however, includes important information about the setup’s geometry. A coordinate system (which was forgotten in this version) will be included.

Minor comment RC1-5:
- §2.2.3 change “measurement uncertainties” with “Statistical and measurement uncertainties”

The authors’ reply to RC1-5:
The suggested formulation is more precise and will be changed accordingly.

Minor comment RC1-6:
- §2.4.2 : A single hot wire does not measure one velocity component but the velocity magnitude in the normal plane to the wire. Please be more precise when you explain the computations of k and the used approximations.

The authors’ reply to RC1-6:

The description is indeed not completely precise as the hotwire experiences cooling from all three flow directions (partly due to the unavoidable cooling of the supporting prongs). According to Jørgensen [*Jørgensen, F.E.: “Directional Sensitivity of Wire and Fibre-Film Probes.” DISA Information No. 11. 1971*] , the effective velocity measured by the single hot-wire is composed as follows: $U_{eff}^2 = U_x^2 + kU_y^2 + hU_z^2$.

Therein, the yaw factor k has typical values of k=[0.1, 0.5] while the pitch factor h is typically ranging between h=[0.1, 0.2]. Furthermore, the calculation will be individually explained for the hot wire measurements and LDV measurements.

Minor comment RC1-7:
- How the TKE is computed for LDV measurements (at least, 2 velocity components are measured simultaneously)? How does it compare with TKE assessed from hot wire measurement?

The authors’ reply to RC1-7:
That is indeed not sufficiently explained. Also for the LDV, measurements only the streamwise flow component is used to estimate the TKE. A second crosswise flow component is available, which was calculated and compared to the streamwise turbulent fluctuation, but not included in the data as shown in the paper. However, a reference to a previous Blind test experiment at the same facility is given (Figure 9 in Krogstad et al. 2014), in which the authors show that the TKE calculated from the isotropic normal stress approximation compares well to the TKE calculated from all three (measured) flow components. A similar comparison has been done for the present tests, but due to similar results it has been referred to the previous publication.

Minor comment RC1-8:
- §3.3: please show the velocity and turbulent profile for the inflow of test case C. Is it a linear shear?

The authors' reply to RC1-8:
The shear flow is described in more detail in chapter 2.1.3. It is not a linear shear. The shear flow profile can be approximated by the power law coefficient alpha=0.11 as described in chapter 2.1.3. For the mean velocity and turbulence intensity profiles it is referred to the invitational document by Sætran and Bartl (2015). As the shear flow case is a central topic of discussion, however, these profiles can, of course, also be included in the main paper.

Minor comment RC1-9:
- P18, line 10 (not shown in this report). . .why do not use in this manuscript the equivalent wind speed as reference to compute the power coefficients if it gives better results? Please modify it

The authors' reply to RC1-9:
This sentence might have been misunderstood and can be clarified. The sentence shall explain that the power curve measured for test case C (shear) is matching the power curve for test case B (uniform) almost exactly if the rotor equivalent wind speed was used. This is somehow just a trivial statement as the rotor equivalent wind speed concept is defined to an equivalent rotor power with a rotor average wind speed. For a linear shear the rotor equivalent wind speed would be identical to the hub height reference wind speed, but that is not exactly the case here. For this blind test case, it was specifically defined a priori to use the reference wind speed at hub height, which is slightly different to the rotor equivalent wind speed. Where to define the reference wind speed is just a matter of definition. All modellers indeed used the hub height reference wind speed correctly and actually achieved a good match with the experimental results. All modellers predicted a lower power for test case C than in test case B, which was also measured. The reason for that is the fact that the reference wind speed at hub height is for this case not identical with the rotor equivalent wind speed.

Minor comment RC1-10:
- §3.3 : the incoming flow is not isotropic anymore. How k is computed in the present case?

The authors' reply to RC1-10:
The turbulent kinetic energy k for shear inflow is computed in the same way as in the previous cases for uniform inflow. It is correct that the incoming flow and thus also the turbulent kinetic energy are not completely isotropic in this case. For this reason, the 2 components wake data from LDV measurements will be further analysed for isotropy also for this case. If significant deviations from the isotropic approximations appear, these will be quantified and shown in a graph.

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