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Interactive comment

Interactive comment on "Power curve and wake analyses of the Vestas multi-rotor demonstrator" *by* Maarten Paul van der Laan et al.

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The manuscript deals with the comparison of power curve and wake data from measurements and various simulations for a multirotor-demonstrator. The authors identify a significant performance gain and the potential for a substantial denser spacing of such an architecture as compared to an equivalent single-rotor turbine. While some supporting evidence is given, I am not yet convinced of these conclusions. Nevertheless, in my view, the obtained data are of interest. Hence, I recommend to accept the manuscript for publication with minor revisions.

My two main concerns are:

1. Simulations and measurements don't seem to agree well enough and, for the far

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wake, sufficient measurement data are missing (as also stated by the authors).

2. The neutral atmospheric stability conditions chosen for the simulations are not fully representative for what a turbine would see in the field and, hence, real performance gains and wake recoveries may be substantially different.

The following suggestions may help in addressing the above concerns:

- What is the estimate for measurement errors? How does this estimate change with wind speed, shear and turbulence intensity?

- Regarding figures 10 & 11, it may be useful to add lines or additional graphs for measurements filtered for similar stability conditions as in the simulations, i.e. for roughly neutral conditions or at least the same shear. If sufficient data were to remain after filtering the measurements, comparisons may be improved and the conclusions of the authors strengthened.

- I liked the concept of the equivalent single rotor used in section 4.4, but would have also liked to see a figure with power curves of this equivalent rotor compared to the (simplified) multi-rotor simulations. It may then make sense to introduce the equivalent rotor at the end of section 3 and add power curve figures after Fig. 11.

- With the concept of the equivalent single rotor, the choice of a suitable simulation tool from the authors' arsenal and systematically varying shear and turbulence intensity of the inflow, it may be possible to gain sufficient insights to explain observed power gains and accelerated wake recovery (and the differences to the measurements).

If the authors feel that the above recommendations cannot be obtained in a timely manner, they may choose to limit the manuscript to neutral stability conditions by adjusting the title and toning down the abstract and conclusions accordingly.

Additional suggestions to improve the manuscript:

- Some more proofreading may be in order, e.g. last sentence in the introduction, p. 9

line 8, captions of figures 10 & 11, p. 17 lines 11-13, p. 18 line 7, references Ghaisas et al. and Meyer et al.

- In the abstract and conclusions, providing +/-0.2% (p. 1 line4-5) may be misleading as the true measurement errors are likely substantially higher (as is stated on p. 14 lines 14-15).

- Regarding the first paragraph of the introduction, are there also some disadvantages of a multi-rotor architecture?

- Regarding figure 7, maybe plot (also) in log-log scale? Moreover, plotting spectra of longitudinal velocity fluctuations or turbulent kinetic energy of measurements and the fitted Mann model may be helpful to appreciate the quality of the inflow data.

- On p. 10 line 14 shouldn't it be C_mu=0.9 or sqrt(C_mu)=0.03?

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