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# *Interactive comment on* "Cluster wakes impact on a far distant offshore wind farm's power" *by* Jörge Schneemann et al.

**Anonymous Referee #1**

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Reviewers Comments

Title: Cluster wakes impact on a far distant offshore wind farm's power Authors: Jörge Schneemann, Andreas Rott, Martin Dörenkämper, Gerald Steindfeld, Martin Kühn DOI: 10.5194/wes-2019-39

General comments

The authors did an excellent job in combining lidar data with SAR data. This approach is novel and the observations resulting from this approach make an important contribution to the ongoing research in the field of mesoscale wakes of offshore wind farm clusters and their impact on the power production. Additionally, the manuscript is clearly written and structured. However, I have one minor comment (see below) that can be



considered as minor, but at the same time this comment is substantial in nature and must be done.

The authors present two case studies whereby one is characterized by stable conditions and the second one by weakly unstable and stable conditions. However, the stability criterion they applied to define the stratification of the atmosphere can be misleading as they only consider the atmosphere below 24.6 m MSL to obtain the stability of the atmosphere. In contrast, the aircraft observations of Platis et al. (2018) and analytical models (e.g. Emeis (2009)) reveal that the atmosphere at rotor height and above are of major importance when defining the stability of the atmosphere when considering wakes of wind farms. The state of the atmosphere above the rotor drives the vertical turbulent momentum flux, that in turn drives the recovery of the wind deficit. Consequently, using a bulk Richardson number using measurements at sea surface and at 24.6 m are independent of the atmosphere above 24.6 m. This point is totally missing in the discussion, especially in the paragraph P20L8-19. The authors claim that they observed a wake with a length of 55 km in weakly unstable conditions, but they don't mention the possibility that an inversion above 24.6 m could have hindered vertical momentum transport from above. For example, in the case study presented in Siedersleben et al. (2018), an inversion was present above hub height but the atmosphere was weakly unstable stratified below hub height in the morning hours. Therefore, the authors should at least mention the possibility of stable conditions above hub height, otherwise the results presented in this study are misleading. Depending on the motivation of the authors they could also check nearby soundings taken at the shore upwind to get an idea of the atmosphere above rotor height.

Other minor comments

P4L16: Why z-score, what advantage does this method have. Please comment on that!

P5L12: Figures 2 and 1 should be Figures 1 and 2

P6L1: . . . with different settings . . . Why not are you not mentioning the names of the settings? -> ... with two different setting A and B as listed in Table 2.

P8L12: How did you derive the virtual temperature at the sea surface? Did you interpolate the pressure measurements take at 24.6 m to the sea surface and what humidity did you use?

P9L21-P10L5: Does the RMSE in wind speed and wind direction correspond to a quality flag smaller or equal than 2?

Figure Comments

Fig. 5: Letters indicating the orientation of the cross section would be very helpful.

Fig. 6, Fig. 7 and Fig. 9, Fig. 10: Again, letters indicating the orientation of the cross section would be helpful. Additionally, the cross section as indicated in Fig. 6b) seems to be longer than shown in Fig. 6d).

Fig. 5b) and Fig. 6b): What is the meaning of the rotor like looking icons?

Fig. 8): . . . in the averaged lidar interval are marked by ?red? horizontal dotted lines?

Fig. 9): Why is the power output only shown for the front row turbines? Are the other turbines producing less than rated power and the z-score is, hence, omitted for these turbines?

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