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WIND ENERGY SCIENCE DISCUSSIONS

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

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The work presented in the manuscript combines SAR surface wind speed retrievals with scanning lidar measurements and SCADA data to characterise wakes from wind farm clusters and their impact on a wind farm in the German Bight. This is adding important new measurements and observations to a growing literature on cluster wakes.

The manuscript is well structured and the presentation of the results is clear and easy to follow. I definitely recommend that the paper is published. Only minor adjustments are necessary.

P. 2, line 9-10: this makes it sound like optimization of wind farm layouts to reduce wake effects is new. Of course, the industry has been doing this for many years. I suggest

slightly rephrasing to avoid this misinterpretation.

P.2, line 15: I suggest adding a reference to the work by Volker on the Explicit Wake Parametrisation (EWP), for example Geosci. Model Dev., 8, 3715–3731, 2015

P.3, line 13-14: maybe add that the wake and free flow regions are defined manually

P.4, line 16: clarify if the curtailment filter is based on a specific SCADA signal or derived from a combination of signals. This is of general interest to readers working with SCADA data. A reference to another paper describing similar filtering would also be sufficient

P.6, table 1: are the hub heights with reference to mean sea level?

P.6, line just above table 2: insert "the" between "in" and "beam"

P.7: how does the finite acquisition time combined with the scan rate "smooth" the lidar measurements? Is this similar to a spatial averaging? Please comment in the text

P.8, line 5: please add a description (and a reference if relevant) for the interpolation onto a regular grid. Also, it is not clear if the exclusion of grid points with less than 10 single scan contribution applies after interpolation or time averaging or both. Please clarify this

Eq. (3): in Stull (1988) the bulk Richardson number is defined in terms of the virtual potential temperature. The authors use the potential temperature. Please specify why this approximation is appropriate and does not introduce bias in the classification of stability. Furthermore, I have seen other papers where the temperature in the denominator of the bulk Richardson number is not the surface temperature, but the air temperature at the measurement height. This cannot change unstable to stable conditions, but it can shift the stability parameter closer or further from neutral conditions. Finally, I wonder if the lidar measurements used in equation 3 include measurements in the cluster wakes. If this is the case, the wind speed in the denominator of equation 3 will be too small, thus biasing the stability parameter away from neutral conditions

P. 9, line 13: which z is used to calculate the stability parameter. Is it z_TP? Please specify

P.12, figure 5: are the non-operating/curtailed turbines the one marked with the symbols that are not circles? This is not clear from the caption. The same symbol is used in figures 9-11, but is not referred to in the captions. Were most turbines standing still or curtailed in the second flow case?

p. 15, figure 8: I worry about the classification of weakly unstable conditions at the time of the 11 October 2018 SAR image. The stability is assessed entirely on a model. What is the uncertainty of this? Later in the same day, when meteorological measurements are available a large bias is seen between the mesoscale temperature and the measured temperature. This bias (if it also existed in the morning) could maybe change the stability classification from weakly stable to weakly unstable.

p. 16, line 30: can you comment on the expected AEP impact of the OSS platforms? I would expect it to be small, since the platforms are fairly low compared with the turbines and have a smaller cross-sectional area.

p. 17, line 3: at rated power OR above rated speed (as opposed to at rated speed)

p. 17, line 9: if the average wind speed is smaller the wind speed deficit should be larger not smaller due to the increase of the thrust coefficient at lower wind speeds. Or am I missing something?

p. 18, line 7: when saying that SAR mostly supports the lidar wake measurements, can you be more specific?

p. 21, line 14: I suggest this phrasing: Wind turbines are sensitive to the wind conditions over a wide range of heights defined by the swept rotor area.

p. 22, line 9: is the wind speed deficit region truly decreasing in width, or is it the region of a certain colour in the heat map that is shrinking? The increase of wake width is typically coupled with the decrease of the peak deficit.

p. 23, line 26: "then" should be "than"

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