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Interactive comment on "Evaluation of the wind farm parameterization in the Weather Research and Forecasting model (version 3.8.1) with meteorological and turbine power data" *by* Joseph C. Y. Lee and Julie K. Lundquist

Anonymous Referee #1

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This manuscript tries to evaluate the wind farm parametrization in WRF by comparing WRF power results to SCADA power data from a given onshore wind farm in Iowa and from additional lidar observations from the CWEX-13 experiment. Main result of the study is said to be the fact that the simulated ambient wind speed is the most important parameter for the quality of the simulation of the wind farm yield. The subject is important and is expected to have both scientific and economic impact.

Thus, the manuscript deserves publication. But unfortunately – at least to my estimation – there are several issues which should be addressed before a publication could

be recommended.

Major review points

(1) The study presented in this manuscript does not differentiate between deviations due to the simulation of the magnitude of the ambient wind speed and those due to inabilities of the wind farm parametrization (WFP). The WFP is not introduced in any detail and none of the found deviations is attributed to any feature of the WFP.

(2) Mesoscale models such as WRF are known to underrepresent the nocturnal low-level jet phenomenon. This has been analysed and explained by Sandu et al. (2013). Deviations in simulated power due to deviations in simulating the ramp effects at the onset of low-level jets (LLJ) have to be attributed to WRF itself and not to WFP. With regard to this known feature it seems a bit unlucky to choose a LLJ episode for this WFP evaluation.

(3) The lower right frame of Fig. 8 shows the dependence of the bias of the simulated power output from atmospheric stability. The authors interpret this figure as showing no significant dependence. My impression is, if the very few data points beyond the stability of 0.6 are skipped, that there is a significant influence of atmospheric stability (leading to a negative bias for more stable situations).

(4) The discussion section (Section 4) makes reference to several results which have not been shown in the preceding results section (Section 3). Therefore, the reader cannot prove these conclusions.

(5) The main conclusion that simulations with WFP are better than those without it is quite trivial.

(6) The study does not present any points which would allow for an enhancement of the simulation tool (either WRF or WFP or both). (Please refer to comment (1) above as well)

(7) sigma_u and sigma_w cannot be derived from a single monostatic remote sensing

device (be it a sodar or a lidar) operating in Doppler beam swinging mode. Thus, the variables TKE and TI (equations (1) and (2)) are very unsure and cannot be used for a reliable evaluation study. This problem can be easily seen from the massive scatter of the TKE values in the third frame of Fig. 7.

Minor review points

(8) The analytic wind farm model in Emeis (2010) is not based on an exaggerated surface roughness. This model uses the farm-averaged thrust coefficient of the wind turbines to extract momentum at hub height. In doing so the model considers a modified surface stress due to the wind farm as well. Please update the paragraph (lines 31 to 37).

(9) The explaining text accompanying the figures in the results section (Section 3) is sometimes quite short.

(10) The chosen colour scale of several figures (especially Figs. 3 and 5) should be improved. It is extremely difficult to see the small differences which are said to be important.

References

Emeis, S., 2010: A simple analytical wind park model considering atmospheric stability. Wind Energy, 13, 459-469.

Sandu, I., A. Beljaars, P. Bechtold, T. Mauritsen, G. Balsamo, 2013: Why is it so difficult to represent stably stratified conditions in numerical weather prediction (NWP) models? J. Adv. Model. Earth Syst., 5, 117-133.

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