

Review of “Observations of wind farm wake recovery at an operating wind farm” by Krishnamurthy, R., Newsom, R., Kaul, C., Letizia, S., Pekour, M., Hamilton, N., Chand, D., Flynn, D. M., Bodini, N., and Moriarty, P.

The revised manuscript analyses the vertical profiles of the vertical momentum flux and vertical wind speed within a wake induced by a large wind farm in the US Great Plains. In their paper, the authors distinguish between several meteorological parameters, including atmospheric stability, boundary layer height, presence of LLJ events and extreme veer and shear occurrences. Further, the authors provide an exemplary extreme case with a very high downward flux in the wake, possibly induced by the presence of a gravity wave. The results show a clear dependence of vertical momentum flux and wind speed deficit on the prevailing atmospheric stability regime, as well as on the presence of extreme events, such as LLJs and in one particular case a gravity wave. Further, observations suggest, that the wind farm’s effects are present throughout the entire atmospheric boundary layer, even far above the rotor plane. Thus, the manuscript addresses internationally relevant questions of importance for the scientific community within the scope of the journal.

From my point of view, the language used in the presented manuscript is very nice and the writing style is easy to follow. The chosen title is concise and represents the content of the paper quite well. The authors provide a very thorough and informative literature overview and separate their work from previous research.

Within the introduction of the paper, the objective statement is formulated very vague. Instead, I would suggest that the analysis of the wake properties is directly included (cf. comment #7).

The structure of the revised manuscript is now easier to grasp and follows a clear storyline. Sections that do not directly add value to the main objective of the paper are now moved to the appendix and provide valuable additional information on the measurements and post-processing.

Considering this and the comments presented in the following, I would recommend the manuscript for a minor revision.

General comments:

1. The authors jump between abbreviations and the written version of LLJ and low-level jet. (e.g. L. 10, 14, 16, 24, 278, etc.)
2. Sometimes, adding a “the” would lead to increased readability, e.g. L.236: “[The] larger the vertical momentum flux, [the] faster the wake [...]” or L. 241-242: “the impact of conventional updrafts or downdrafts on [the] propagation of wakes”
3. In Fig. 3 error bars are given for the median profiles, while they are missing in Fig. 5 and following. Is there any specific reasoning behind this?

Specific comments:

4. L. 28: You only mention mesoscale simulations here, but e.g. in Schneemann et al. 2020 the authors observed them with scanning offshore lidars.
5. L. 53: The formulation “ABL is lower than 300m” suggests that this is always the case. However, as per my knowledge, even in stable conditions boundary layer height can exceed 300m (e.g. Peng et al. 2023).

6. L. 209: Here, the authors claim that “sufficient” data is available. It would be helpful to know, how much (e.g. in hours or No. of measurements) that is.
7. L. 229: The authors claim, that larger momentum flux deficits for near surface areas are observed for unstable and neutral conditions. However, I would argue that during stable conditions, based on the provided figures, the momentum flux deficit is larger than for neutral conditions and also compared to unstable conditions. At larger heights it then seems as if momentum flux deficits are larger during unstable conditions. Maybe a clearer picture containing directly the difference in fluxes or some other information supporting the presented claim could be provided. Also, the large error bars make it hard to really make such a distinguished claim.
8. L. 271: The authors only mention a “set threshold”. Could this be specified? This would then also make the next sentence, specifying three different thresholds, which were all combined in the end no longer necessary. Maybe just specify the “weakest” threshold.
9. L. 280: The authors claim that it is “evident that [the] higher the Z_{LLJ} , [...] [the] higher the hub-height wind speed”. However, Fig. 4c shows that a maximum LLJ height is observed for hub height wind speeds of 13 m/s with a slight drop-off thereafter.

Technical Corrections:

10. L. 140: Here, the unit GWh is written out, which is not necessary and is also not done for other units, e.g., meters or Megawatts.
11. L. 142: Here, it sounds like with “the millions of U.S. homes” all homes in the entire state/country are meant. As this is not the case, I would leave out the “the”.
12. L. 156: It should be “correct” instead of “correcting”
13. L. 503: I think here it should be “impact on wake recovery”, not “of”
14. L. 511: “Gravity waves enhance” instead of “enhances”

Literature

Peng, S., Yang, Q., Shupe, M. D., Xi, X., Han, B., Chen, D., Dahlke, S., and Liu, C.: The characteristics of atmospheric boundary layer height over the Arctic Ocean during MOSAiC, *Atmos. Chem. Phys.*, 23, 8683–8703, <https://doi.org/10.5194/acp-23-8683-2023>, 2023.