Review of the manuscript Observations of wind farm wake recovery at an operating wind farm authored by Raghavendra Krishnamurthy, Rob K. Newsom , Colleen M. Kaul , Stefano Letizia , Mikhail Pekour , Nicholas Hamilton, Duli Chand , Donna Flynn , Nicola Bodini , Patrick Moriarty

General comments

The manuscript is about the analysis of an interesting observation data set for vertical momentum fluxes upstream and downstream of wind farms. The study gives insight into the dynamics of wakes depending on the background atmospheric conditions. The study is of high relevance for the validation and improvement of numerical models required to optimize the design and operation of wind farms. The manuscript is well written and understandable. We recommend publication with minor revisions. The identified deficits are mainly related to the theoretical background, which should be explained a little bit more carefully, as well as the spectrum of citations, which could be a little bit broader in some places.

We thank the reviewers for their thorough and thoughtful assessment of the article. In the updated manuscript, we have addressed most of the reviewers concerns and provided justification or clarification for others. Our point-by-point responses may be found below in blue font.

Specific Comments

Page 2, Line 49: Maybe one should better say "Todays wind turbines operate …"

We have updated the manuscript to reflect this statement.

Page 2, Line 49: Maybe one could add that in some cases the boundary layer is not even that thick.

Thanks for the comment. We agree and have updated the manuscript and stated: "*and in offshore or stable atmospheric conditions the ABL is lower than 300 m (Shaw et al., 2022)*.

Page 2, Line 60: "As wakes grow …" Please be more specific. Do you mean growth in the lateral vertical extend?

We mean laterally and have made this clear in the updated manuscript.

Page 2: Please add some brief info about satellite radar wake measurements for offshore wind farms, e.g. B. Djath, J. Schulz-Stellenfleth, and B. Canadillas, "Impact of atmospheric stability on X-band and C-band Synthetic Aperture Radar imagery of offshore windpark wakes," *Journal of sustainable and renewable Energy*, vol. 10, no. 4, 2018, doi: 10.1063/1.5020437. in the intro paragraph mentioning different observation systems. The above publication also points out the importance of a better understanding of vertical momentum fluxes for the interpretation of SAR observations.

Please also mention airborne campaigns, e.g. A. Lampert *et al.*, "In situ airborne measurements of atmospheric and sea surface parameters related to offshore wind parks in the German Bight," *Earth System Science Data*, vol. 12, no. 2, pp. 935–946, 2020, doi: 10.5194/essd-12-935-2020. which also provided info about vertical momentum fluxes.

Thank you for alerting us to these two references. We have now added the above references to the updated manuscript.

Page 3, Line 88: "… change in surface roughness .."

I think this statement is based on a simplified view of the real processes, which is perfectly fine, but this should be stated somehow. Please cite P. Taylor, "On wind and shear stress profiles above a change in surface roughness," *Quarterly Journal of the Royal Meteorological Society*, vol. 95, no. 403, pp. 77–91, 1969. in this context too.

We agree and this reference has been added to the updated manuscript.

Page 3, Line 91: " …growth with downstream distance …" But it

will not grow forever (?)

We agree it will not grow forever and will be capped by the inversion height or the atmospheric boundary layer depth. We mention that in subsequent sentences below. Therefore, for the sake of not complicating the sentence structure we have left this statement as it is.

Page 4, Line 96: "During stable …" Did

you mean "unstable" ?

Perhaps there has been a misunderstanding, we do mean during stable conditions the internal boundary layer height grows to the atmospheric boundary layer height within a short distance, since the atmospheric boundary layer height is shallower, and the wakes are longer.

Page 4, Line 103: Please explain the meaning of the function F_1 more carefully (Buckingham Pi theorem, I guess)

Yes, F is an unknown function and this has been mentioned in the updated manuscript.

Page 4: I was a little bit confused, because the roughness length z0 of the surface without wind farms and the stability seems to be irrelevant in this discussion (?), see e.g.

> S. Emeis, "A simple analytical wind park model considering atmospheric stability," *Wind Energy*, vol. 13, no. 5, pp. 459–469, 2010, doi: 10.1002/we.367|.

Please explain this part a little bit more carefully.

We agree with the reviewer, but these estimates are based on a single column model (Calaf et al., 2010, Stevens, 2016). One of the authors, Krishnamurthy et al., 2022, has developed an IBL relationship as a

function of atmospheric stability for canonical boundary layers but they are currently not formulated to account for the wind turbine dynamics. This is something that the authors plan to work as a part of future research.

Page 5, Line 140: Please use a different notation for "v", e.g. v_\perp, here. Is it so obvious that <w>=0, e.g. in cases with convective cells?

Since this relates to sonic data post-processing, the 2-axis rotation ensures that the <w> = 0. We would recommend the reviewer to refer Wilczak et al., 2001 for additional information and techniques.

Page 5: I think a figure explaining the geometry would be helpful.

Since this entire section was moved to the Appendix, after reorganization and in the interest of reducing the size of the manuscript, we have referred to the article (Sathe et al., 2015), which provides a geometry used in this manuscript. We have provided a sample image for the scan pattern for the reviewer's benefit.

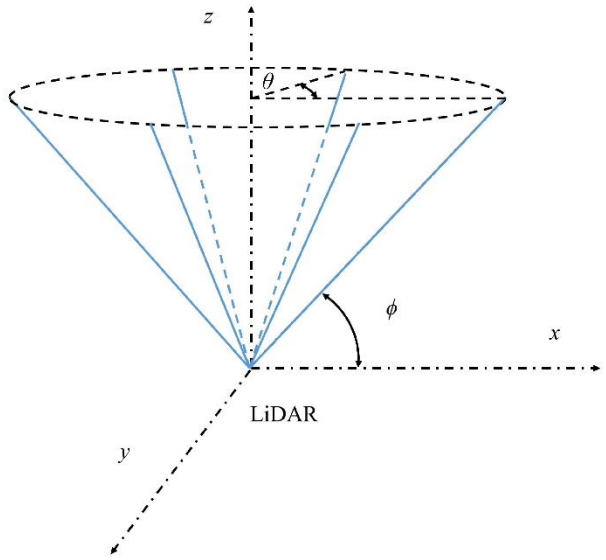

Figure. Schematic diagrams of Velocity Azimuth Display scan. LiDAR is placed at the origin of the Cartesian coordinate system.

Figure 1a: The R^2 value is hard to believe. I think the reason is that there are so many points on top of each other. Please use a density plot, i.e. 2D histogram. Please indicate in the caption that different axis scaling is used in a) and b).

Thank you for the comment. We have mentioned the x-axis scaling difference. Yes, the R2 value is high due to small distribution of observations of momentum flux.

Page 7, eq. 7: Maybe I missed it somehow, but how did you measure the vertical heat flux?

The kinematic heat flux is an estimate from the sonic anemometer. We mention this in the updated

manuscript.

Page 12, Line 274: "… median streamwise momentum …"
I did not fully understand which upstream/downstream distances the curves in Figure 4 correspond to.

Thank you for the comment. We have now removed this figure, as its repetitive and agree that it did not add much to the manuscript. For other figures, we have provided clear labeling, so hope there is no confusion in the updated manuscript.

Page 14, Line 303: "… in Figure 4b …" Did you mean Figure 3 ?

Thanks for the typo, this has been fixed.

Page 14, Line 324. "Larger …" please correct sentence.

I think it would be good to learn more about the wind speed profiles upstream to see where we see the largest vertical gradients and where mixing can increase vertical momentum fluxes most effectively.

We agree and have mentioned in our previous statement to "in stable atmospheric conditions, due to large (positive) wind shear, the momentum flux must be negative to create downwind turbulence." The authors have shown many instances where this statement is true in the manuscript.

Page 22: In the context of the discussion about good definitions of wake length one should also mention that it is sometimes not trivial to distinguish wakes from variations in the background wind field, e.g.

> B. Djath and J. Schulz-Stellenfleth, "Wind speed deficits downstream offshore wind parks - A new automised estimation technique based on satellite synthetic aperture radar data," *Meteorologische Zeitschrift*, vol. 28, no. 6, pp. 499–515, 2019, doi: 10.1127/metz/2019/0992.

Thank you for alerting us to this very interesting paper. We have added it to our references in the updated manuscript.

Page 30, line 550: " … upwind surface roughness (z_(0,hi)) …"
I'm confused, because I thought z_(0,hi) is the "…roughness due to the presence of a windfarm …" (page 4, line 119)
The reviewer is correct, this typo has been corrected in the updated manuscript.