Review comment on WES-2026-8

General comments:

The paper "Impact of atmospheric stability and turbulence on wind turbine wake characteristics: a nacelle lidar study" provides a detailed analysis of the influence of atmospheric conditions on near wakes compared to turbine operational parameters. It emphasises the importance of considering atmospheric conditions for near wake models. The nacelle lidar dataset covers an extensive period from November 2023 until June 2024. The paper is generally well-written and the topic is relevant to the scientific community. To improve the manuscript further, I would suggest minor revisions.

Specific comments:

L20 ff: Please add citations for the statements you are making, e.g. on the topics: increased load on downwind turbines due to the wake, wake models for layout optimization, low complexity of models, real world conditions are more complex -> see near wake region and atmospheric inflow conditions.

L. 146 Can you please specify how you calculated the intersection with the ground, because if you assume that the nacelle lidar is located at 92m, then the intersection of the beam with the ground would be around 755m not 700m.

L. 147: Why does it say data until December 2024 in this line, when the abstract states that the data analysed is available until June 2024.

L. 150: Could you please specify this further? Because in L. 287 you say that the wake of OPUS1 is influenced by the wake of OPUS2 at 4D downwind.

L. 186: Can you explain why you introduced VD when you did not use it and only used v_h/U in figure 4. VD is then reintroduced in L. 267 and used in figure 7.

Fig. 4: This figure is very interesting and displays a lot. It would be helpful to add more references of the figure to the text and maybe also some more explanation as e.g. why you used this day during nighttime.

L. 213ff: This section describes wake characteristics but changes metric a few times between meters and diameter. I would suggest being more consistent with that and when e.g. talking about the wake length to always refer to the diameter. (also L. 275)

Fig. 5: To stay consistent, I would suggest to also add "D" to the y axis values, as e.g. in figure 4.

L. 237: Could you please rephrase the sentence describing the new criterion? The term velocity ratio should also be introduced and defined.

L.244: Could you please add the meaning of the abbreviation RHI?

Sec. 4: I would suggest to add 1-2 sentences about the location of the measurements you used for the meteorological properties or refer to the section where you described that.

L. 272: It is a little confusing that you say the location of OPUS 2 is at around 4D when in figure 7 the captions says that the line is at 4.3D.

L. 278: Can you explain how μ was calculated? What wind data did you use?

Fig. 7: How is the velocity deficit calculated? Did you compare the minimum in the wake to the wind speed measured by the nacelle lidar outside of the wake or to the tower measurements? What uncertainties do you induce by comparing different altitudes (nacelle at 92m; sonic at 85m) and different measurement principles (lidar and sonic). And how is the influence of the beams being outside of the rotor plane above 4D downwind of the wind turbine?
I would also suggest emphasizing in figure 7a, b, c that the data is not consistent, because the lidar has a resolution of 20m.

L. 283 How do you define the "wake deficit", is it the same as VD or is it v_H/U, please specify?

L. 300: Can you please specify if wake centre deflection, wake deflection, lateral deflection and wake tilt are the same and maybe only use one of the words throughout the paper to make it easier to follow.

Fig. 9: Could you please add another value to the y-axis, that would make it easier to estimate the magnitude of VD?

L. 320: Please further elaborate this. It would help if you would describe the cause more straight forward. "Under stable conditions VD is stronger (also described in literature). The negative correlation between VD and TI and e supports that lower TI and e values are associated with stable conditions. "

Sec. 4/5: It would help to define a clear wording when talking about correlation e.g. 0: zero; 0.01-0.3: weak; 0.3-0.7: moderate; 0.7-1: strong. Because strong is used interchangeably for different values and not consistently.

L. 334: While here you are saying that there is no significant correlation between μ and Ri and the potential temperature gradient, in L. 319 you say that VD is stronger during stable conditions. While the values for Ri and the potential temperature gradient are very similar for VD (at 1D: 0.17; 0.33) and μ (at 1D: -0.18; -0.34) the wording implies there is a significant difference. I would suggest adjusting one of the sentences to be consistent in the wording.

Sec. 4.3: Something that was missing from my point of view was the explanation on why some correlations differed so greatly between the filtered and the unfiltered dataset: Why is the correlation between Ri and ΔA for the unfiltered dataset as negative but for the filtered dataset weakly positive? Why is the correlation between ΔU and VD negative

in the unfiltered and positive in the filtered dataset? What is your explanation to why the slope is independent from filtering?

L. 373 I would suggest to already add a sentence here, even though there is a sentence in L. 464, describing that the first datapoint of the lidar is 100m and therefore the mean needs to be considered with caution because it cuts off everything below 100m.

L. 434: Please specify, if you mean the velocity deficit or if you are referring to v_h/U which is e.g. displayed in figure 5 to illustrate the transition in the near wake region.

L. 450: The manuscript emphasizes the potential use of this data for modelling and application, is there an opportunity to make this dataset available?

L. 507: Even though the dataset covers multiple seasons, how well represented are they in the filtered data? For the analysis you only use 1375 out of 20098 10min periods, is there a pattern, e.g. mostly nighttime, exception in certain month/seasons?

Technical comments:

L. 109: "wind measuring sensor" could be more precisely replaced with sonic anemometer

L. 128ff: Please be consistent with introducing and referring to parameters with their abbreviations. In this paragraph sometimes there is no abbreviation, sometimes there is only the abbreviation and sometimes there is both.

L. 158: There is a space missing "wind speed.Additionally"

L. 335: the minus in front of 0.18 is missing