Response to Reviewer 2

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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.

Thank you very much for your thoughtful and constructive feedback on our manuscript. Below, we address your specific and technical comments and indicate the corresponding changes we have made to the manuscript. We appreciate that you took the time to read our manuscript and for sharing your expertise, which has contributed to improving our study.

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.

  Thank you for this suggestion. We have added relevant references, the manuscript is adapted as follows: *"The wake of a wind turbine is a region characterized by reduced wind speed and increased turbulence that forms behind the rotor as the wind turbine extracts energy from the flow (Porté-Agel et al., 2019). Wind turbine wakes impact the efficiency of wind farms by not only reducing energy production due to lower wind speeds in the farm (Barthelmie et al., 2009; El-Asha et al., 2017) but also by increasing the loads on downwind turbines, which can lead to fatigue and a reduced lifespan (Thomsen and Sørensen, 1999; Kim et al., 2015). To mitigate these effects, research has focused on understanding the wake dynamics and integrating the knowledge into wake models which serve as useful tools for layout optimization, wind resource assessment and wind farm control (Archer et al., 2018; Fleming et al., 2014; Pedersen and Larsen, 2020). To maintain computational efficiency, these models are often of low order complexity (Amiri et al., 2024). However, this leads to a simplification of complex real-world characteristics, including the near wake region and the variability of atmospheric inflow conditions (Abkar and Porté-Agel, 2015; Meyers et al., 2022)."*

- 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.

You are right. Our initial estimate was indeed rough as it was based on the location of high CNR values in the lidar scans. Using the nacelle height of 92 m and 7° elevation angle, the ground intersection occurs at 92 m/ arcsin 7°=751 m, as now stated in the revised manuscript in Section 2.2. Nevertheless, in the lidar scans disturbances may appear already at a closer distance due to the width of the lidar pulse (FWHM of 50 m at 200 *ns* pulse width). Hard targets (like the ground) at the pulse edges can cause stronger backscatter than aerosol in the center, explaining the observed interference starting at around 700 m.

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

  Thank you for mentioning this point. We appreciate the opportunity to clarify this. While the lidar measurements have been conducted continuously from November 2024 and are still ongoing, our study is limited to data up to June 2024 due to consistency of available inflow mast data (required for veer, shear and stability quantities) and reliable operation of the lidar, which was subject to some failures in later periods and was replaced in 2025. Due to the responses to a comment of Reviewer 1, the number of cases has changed slightly from the original manuscript. We have included more details in the revised manuscript (Sect. 2.2) on the number of 10-minute periods retained after each filtering step: full dataset: 19804, wind direction sector: 6171, normal turbine operation: 4964, and availability of inflow data: 1396.

- 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.

  Thank you for pointing this out. We have rephrased the passage to state that OPUS1 is not operating in the wake of OPUS2, but indeed there will be wake interaction with OPUS2 wake starting at 4D downstream of OPUS1 for the selected wind direction sector, as follows: *"Additionally, it guarantees that OPUS1 is not operating within the wake of OPUS2. While OPUS2 is located downstream of OPUS1 under these conditions, wake interactions begin at 4.3 D, which is not relevant to this study as it focuses on characterizing the wake of OPUS1 up to 4 D downstream, as measured by the nacelle lidar."*

- 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.

  You are correct. We introduced VD but didn't refer to it until Section 4 as we were more focused on showing the velocity ratio (v_h/U). We have moved its definition to Section 4 in the revised manuscript to avoid confusion (see also later comment on Fig. 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.

  The example in Figure 4 is now explained in more detail in the text in Section 3 after the wake detection algorithm was introduced. This example shows a quite long wake where the different development stages of a wake are visible and also the usage of the different Gaussian functions is highlighted. We choose this wind direction as the wakes of OPUS1 and OPUS2 do not overlap which allows to interpret solely the wake of OPUS1 far downstream. The manuscript in Sect. 3 now states *"An example of a processed horizontal PPI scan is shown in Fig. 4b. A night-time case with stable conditions is selected where the wake's velocity deficit is strongly pronounced and it is persisting far downstream, even beyond 10 D. Lateral cross-sections (Fig. 4c-f) demonstrate the evolution from resembling a double Gaussian with* *asymmetric amplitudes, over a double Gaussian towards a single Gaussian in the far wake. Complementary, Fig. 4a indicates the best fitting Gaussian function. The wake centerline moves slightly away from the turbine hub towards $y < 0$ downstream. Also, in Fig. 4b the filtering of the scans excluded spurious measurements around $(x, y) = (3D, 2D)$, where the three masts and OPUS2 are located."*

- 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)

  Thank you for this helpful comment. We agree that consistency is important. In L. 213ff, we choose to keep using meters for lidar related specification of the wake detection algorithm, since the lidar data is on a 10x10m grid. This applies also for example for the first measurement distance later on which is given in meters as well. Otherwise, we refer to rotor diameters with variables concerning the turbine wake including e.g. wake width.

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

  We appreciate this thoughtful suggestion. We have updated the figure to extend from - 0.5 D to 0.5 D on the y-axis, thus we changed the parameters slightly. As it is an analytical example, the message remains the same, though readability and interpretability of this figure are enhanced and better align with the dimensions of a turbine wake.

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

  Thank you for pointing this out. We agree that the phrasing was unnecessary complicated. We changed the description in the updated manuscript and introduced velocity ratio accordingly as follows: *"Therefore, we introduce an additional criterion for determining the near wake length: the transition occurs where the difference between the wake center and the minimum velocity ratio is less than 5%."*

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

  Thank you for catching this. We have now introduced the full term, range height indicator.

- 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.

  You are correct. We haven't introduced these measurements in the results section very well. We have now added additional information when introducing what kind of data is used in the analysis and are referring to Sec. 2.1 in the revised manuscript. We have adapted the text as follows: *"The analysis includes wake data of OPUS1 derived from 10 minute averaged lidar scans, measured as described in Sect. 2.2, as well as turbine operational parameters of OPUS1 and meteorological parameters, provided by the inflow mast located west of OPUS1, the latter two are described in Sect. 2.1."*

- 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.

  Thank you for mentioning this ambiguity. We adapted the text to use 4.3D as well as this is the more precise location.

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

  Thank you for mentioning the lack of clarity. μ represents the wake center in cross-wise direction at hub height. We introduced it in the updated manuscript in Sec. 3.2 as it is a derived quantity from the Gaussian fits. We have renamed the variables in Eq. 5-7 from b, b_1 and b_2 to μ, μ_1 and μ_2 respectively to keep the naming convention clearer. We did not use other wind data then the nacelle lidar data to calculate the wake center.

- 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.

  Unfortunately, it seems we did not make the calculation of the velocity deficit clear enough. We used the wind speed of the inflow measurement mast at 85m U and the minimum wind speed in the wake U_{min} as basis, thus VD is calculated through $1 - U_{min}/U$. In the revised manuscript we adjusted the explanation in the beginning of Section 4.1 as follows *"Fig. 7 shows the mean values and standard deviations of the velocity deficits VD, which is defined as $1 - U_{min}/U$ with U_{min} the minimum velocity in the wake region (wake center $\pm\sigma_w/2$),..."*. We give more details in the revised manuscript on the implications using data from the sonic at 85m in Sect 2.1. In detail, we state *"Although the height difference of 7 m between the sonic at 85 m and hub height introduces minor discrepancies, the observed wind shear values (mean: 0.029 s^{-1}, max: 0.086 /s) suggest that the differences in wind speed are small, on the order of 0.203 ms^{-1} on average and up to 0.602 ms^{-1} at maximum shear. Similar considerations apply to wind direction, where the mean difference is 0.371° and the maximum difference is 3.465° based on observed veer between 33 m and 149 m."*.
  Lidar and sonic anemometer measurements differ due to the lidar's larger measurement volume, lower temporal resolution and assumptions about wind homogeneity when the data is processed, which can lead to underestimations in velocities especially when the wind field is inhomogeneous with e.g. steep gradients.
  Beyond 4 D downstream, we do not use the +-7 ° elevation scans for vertical information of the wake. This is indeed due to the fact that the beams will be measuring outside of the rotor area (Fig. 2b). Actually, we use the vertical information up to 2 D downstream in our analysis and beyond that we solely analyze the 0 ° elevation scan. Additionally, we mention the resolution of the lidar data in the figure caption of Fig. 7abc in the revised manuscript.

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

  Thank you for bringing this misleading phrasing to our attention. We changed it to velocity deficit to clarify that it is about the quantity VD.

- 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.

  We appreciate your concerns and agree that the terminology used in the manuscript was unclear. Indeed wake center deflection, wake deflection and lateral deflection mean the same, though wake tilt refers to the vertical slope of the wake centers at different heights. To improve clarity, we have applied the following consistent terminology. "Lateral wake center deflection" is now used to describe the variable μ in the revised text in Sect. 4.1 and generally instances referring to wake center deflection, wake deflection and lateral deflection in the whole script have been updated to this term except for a few occurrences to not lengthen the text unnecessarily and keep it concise, e.g. when in the same paragraph the whole term was used already. Additionally, we have replaced "wake tilt" with "vertical wake skewness" throughout the manuscript, as this more accurately describes the vertical slope of the wake center at different heights.

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

  If we were to include further details on the axis, this plot would reveal information about the specific turbine control strategy. Therefore, we unfortunately cannot provide these due to confidentiality agreements. The figure in this manuscript is supposed to show the filtered regions qualitatively.

- 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."

  Thank you for this suggestion. We have expanded the discussion on the connection between stable ABL, less turbulence and stronger velocity deficits of the wake, as follows: *"Correlations are calculated for meteorological parameters with wake characteristics for this filtered dataset and shown in Fig. 10. The velocity deficit is more pronounced under stable conditions, as indicated by weak positive correlations to Ri and $d\theta/dz$ at 1 D. As shown e.g. in a LES study by Abkar and Porté-Agel (2015), stable stratification suppresses turbulent mixing, leading to stronger, more persistent wakes. In stable ABLs turbulence, here represented by TI and e, is usually lower. Consequently, negative correlations of VD with turbulent characteristics TI and e support that wake velocity deficits are stronger under 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.

  We appreciate this helpful suggestion. We have followed it and used consistent terminology for correlation strengths throughout the manuscript.

- 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.

  Thank you for catching this. Due to the responses to a comment of Reviewer 1, the correlation values have changed slightly, so this sentence had to be revised. Nevertheless, as mentioned in the previous comment, we have now adopted the suggested wording throughout the whole manuscript.

- 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?

Thank you for your helpful input on this chapter. We agree that the differences between the unfiltered and filtered datasets were not explained well enough. Therefore, we have included a paragraph in Section 4.3 to provide further information on why the correlations differ. The discrepancies arise from different mechanisms, as follows:

- Correlation between Ri and ΔA: In the unfiltered dataset, negative correlations emerge due to cases of negative lateral asymmetry under strongly stable conditions, i.e. high $d\Theta/dz$, Ri and $\Delta\delta$. These cases occur at low wind speeds and low turbulence. They skew the distribution of ΔA against stability parameters, such as Ri. In the filtered data, these data points are removed, thus revealing the underlying positive correlation with, for example, Ri.
- Correlation between ΔU and VD: In the unfiltered data, low VD above $U_{\max}$ corresponds to high ΔU, pushing the correlation towards negative values. Filtering the data reveals the relationship between increasing ΔU with more stable conditions, leading to positive correlations of ΔU with VD.
- Correlations of slope independent of filtering: Correlations with the vertical wake slope remain consistent across the datasets because turbine operation appears to impact the vertical skewness only minimally. However, veer and stability parameters dominate the extent of the vertical slope, since their effect persists independently of the turbine operating state.

- 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.

  Thank you for this valuable suggestion. We have added a sentence after introducing the histogram stating *"However, the distribution is truncated at a minimum near wake length of 110 m, since the lidar measurements begin at 100 m."*

- 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.

  Thank you for pointing out that we have to clarify what we mean here. We changed the phrasing in the revised manuscript to *"The asymmetry of the double Gaussian velocity ratio ..."* to be consistent as we deduced the asymmetry from the Gaussian fits.

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

  We appreciate your interest in making the dataset available. While we do not plan to publish the complete dataset of this study, we are planning targeted benchmarks using data from the WiValdi wind farm in the near future.

- 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?

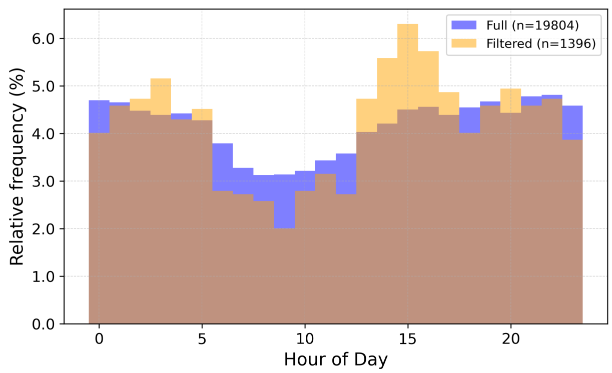

Figure 1: Hour of day distribution for full (19804) and filtered (1396) dataset

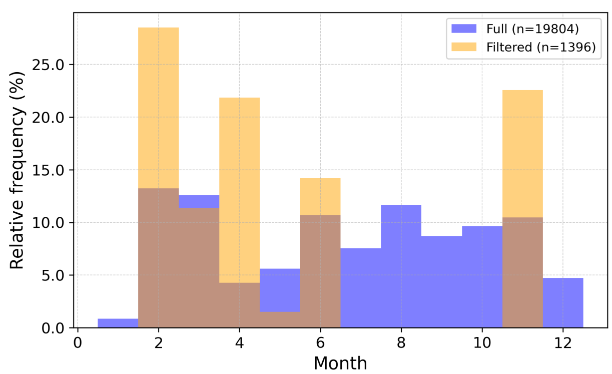

Figure 2: Month distribution for full (19804) and filtered (1396) dataset

Thank you for raising this point. The filtering ensures that only wake measurements with available inflow measurements, including various heights, are retained. The distribution throughout the hours of the day is comparable for the 19804 and 1396 10 minute periods (slightly adapted number of cases see comment to L. 147), see Fig. 1. There is a slight higher representation for cases from 13-15:00 in the filtered dataset compared to the full dataset and lower representation during 5-12:00. However, we consider these deviations to be small enough to treat the subset as representative. Due to the availability of inflow measurements, the dataset ends in June 2024, so we indeed lack data for some summer and autumn months. Additionally, due to turbine maintenance or operation not being in normal power mode, data is unavailable for December 2023 and January and May 2024, see Fig. 2. Nevertheless, the analyzed dataset covers a broad range of atmospheric conditions as shown by the histograms in Fig. 6.

Technical comments

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

  Thank you for this suggestion. We have revised the manuscript to specify that the masts are equipped with sonic and cup anemometers and wind vanes.

- 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.

We have reviewed this passage and ensured that all abbreviations are introduced at the first occurrence. Later mentions use either the long form or the abbreviation.

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

  Thank you for catching this detail. We have corrected this.

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

  We appreciate you pointing out this sign mistake. We have changed this.

References

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