Synchronised WindScanner Field Measurements of the Induction Zone Between Two Closely Spaced Wind Turbines

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Reviewer 1

The authors present measurements of the induction zone of a wind turbine. The measurements were acquired by two Doppler lidars, that scanned in a synchronous mode a horizontal plane, at the hub height of a 3.5 MW wind turbine, that extended up to 0.8 D upstream. The conduction of field campaigns using multiple Doppler lidar is a challenging task. The authors provide a thorough description of the installation and the alignment of the wind lidars, as well as an investigation of the magnitude of the errors in the measuring setup. Using the acquired wind measurements, the spatial characteristics of the induction zone are examined during four periods when the inflow of the wind turbine examined was characterised by free flow, fully waked or partially waked conditions. The overall manuscript is well written and well structured. However, I think that there are some parts that should be clarified and explained better, before the article is ready for publication. I am highlighting these points in the list below.

We thank the reviewer for their critical assessment of our work. In the following we address their concerns point by point. We hope these changes will positively benefit the manuscript. Comments to the reviewer points are made in blue while modifications to the manuscript are shown in red.

Based on the comments of Reviewers 1, 2 we have summarised the major changes in the revised manuscript below:

- Expanded the site characterisation section with the topography and the presence of flow blockage (treeline) in between the two turbines and the discussion section to include the effects of the topography and treeline as a possible explanation for our measured flow features and a discussion on how to decouple the terrain effects from the rotor aerodynamic effects.
- Updated the LES results section with an analysis of the statistical uncertainty of the measurements.
- Expanded the LES Results section to include the rationale behind our assumption of the vertical velocity on the Standard Uncertainty Propagation (SUP) and its impact on the width of error bars. The discussion section has been updated to include the significance of our results based on the SUP assumptions and suggestions for future measurements with a third synchronised lidar.
- The conclusion section has been reworked substantially to include a description of our measurements, measured flow features, a summary of the uncertainty analysis and summary

of challenges in conducting field measurements for obtaining validation data for numerical models.

Main Comments

Comment 1: The authors present four data sets of induction zone measurements, which correspond to cases where the inflow of a utility-scale wind turbine is characterised by free, waked and partially waked conditions. There is a thorough description of the observed spatial features of the flow, but there is a lack of scientific conclusions. The interaction of the induction zone with the wake flow is something to be expected. My recommendation to the authors is to present what do we learn from the results of this study. In the last sentence of the Conclusions sections they write "A preliminary evaluation of the engineering models of the induction zone indicates that the models do not completely capture the complex flow behaviour and turbine interactions". I could not find a discussion of this topic in the manuscript for the case of the waked inflow. Figure 12 shows the induction zone for a free flow, but when the authors write "turbine interactions" I understand they did an evaluation of engineering models of the induction zone in waked conditions.

Reply:

The objective of the work was first to use the unique fast scanning capabilities of the WindScanner systems and quantify the measurement errors due to the limitations of our experimental setup. We were able to calculate the magnitude of errors using a virtual lidar in LES approach and successfully characterised the induction zone for multiple inflow scenarios. The main conclusion that we could draw from our study is the complexity of setting up synchronised lidar measurements, difficulty in quantifying the effects that are seen in field measurements due to limitations in our measurement setup (no third lidar), the additional considerations needed to characterise the induction zone behaviour based on error analysis and therefore how one should interpret the observed flow fields. We believe this to be an important contribution as field data is usually considered as "ground truth" and is demanded as a reference for qualitative and quantitative validations of numerical flow development models. Therefore, we have strived to provide a thorough description of the site, lidars, measurements and their associated uncertainties for comprehensive traceability of our work in addition to making the lidar dataset available on request for future studies and validation attempts.
Indeed the observed spatial flow features such as induction - wake interaction and the induction zone behaviour during wake steering are expected phenomena. However, the experiments are, to the author's knowledge, the first high-resolution measurements of the induction zone - wake interaction for utility-scale turbines.
On the topic of turbine interactions, we had evaluated the coupled FLORIS induction zone model for the waked and partially waked cases but had not presented them in the paper due to the inability of the model to capture the flow field behaviour that was expected after the poor model performance in the undisturbed induction zone. We will therefore remove our statement in the conclusions and expand the discussion section that an analysis was performed but not shown in the paper.

We have updated the conclusion section based on our answer above.

Comment 2: Figure 1 presents a photograph of the landscape where the measurement campaign took place. There we can see that the topography of the area between the two wind turbines is not homogeneous. There is a tree fence which is located in the vicinity of the WT1 wind turbine and extending towards the meteorological mast, and furthermore a crop field between the wind turbines WT1 and WT2. The authors don't describe in detail the physical characteristics of these features and don't discuss what if there is, or not, an impact of these features on the flow. I think that this is important, since it can partially explain the features of flow presented in Figs 6, 10, 13 and 15. For example, in Fig. 10 (c) the induction zone is seen to be symmetric right in-front of the rotor plane, but it gets asymmetric as the upwind distance increases. Why should this happen? Have the authors acquired any measurements or have they performed an LES study of the flow (where the variations of the terrain features were taken into consideration) while both wind turbines were not operating?

Reply: We acknowledge that we did not discuss the site layout in detail. The induction zone of onshore wind turbines can be incorrectly estimated using observations if the effects of nonuniform terrain on the flow are not carefully considered. This non-uniformity could be a result of the changing elevation upstream of the turbine or as pointed out, through the presence of a high tree line. The impact of terrain on the induction zone have been studied through simulations [12] and experiments [9]. For instance, Mikkelsen et al., [9] in their dual-WindScanner lidar measurements at the DTU Riso site measured a vertical velocity of 1 m/s in the induction zone which was attributed to the sloping terrain upstream of the turbine.

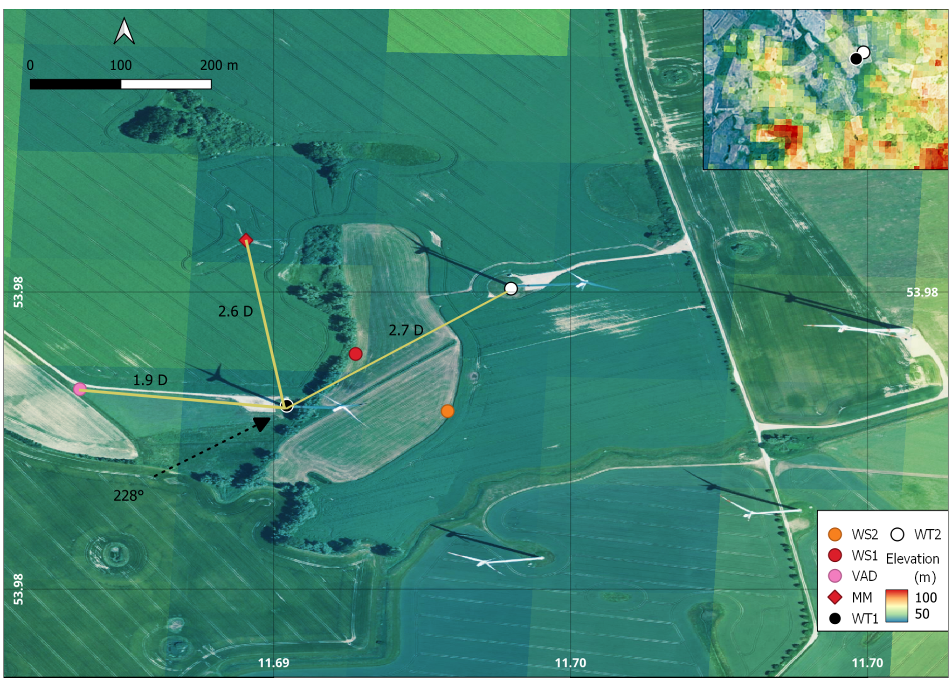

Figure 1: The wind park and measurement layout at Kirch Mulsow with elevation contours. A zoomed out image of the site is shown in the top right corner illustrating the hills present upstream of the wind park. Here WT1, and WT2 refer to the upstream and downstream turbines, MM and VAD are met mast and the VAD lidar while WS1, WS2 refer to the WindScanners.

We provide a more detailed description of the site, as illustrated in Fig. 1. The elevation data was obtained with a resolution of 200m maintained by the German Ministry of Cartography and Geodesy [2]. While the elevations at the locations of the two turbines WT1, WT2 are approximately 52 m, abrupt elevation changes are seen upstream notably the presence of a small hill with an elevation of 105 m 22 D upstream of WT1 along the predominant wind direction of 228°, creating a slope of 1.09° towards the two turbines. The village of Garvensdorf with its farmhouses was approximately 1200 m upstream of WT1.

We also note the presence of tree lines and small clumps of forested terrain. As rightly pointed out, a treeline exists between WT1 and WT2 extending towards the met mast with a height of approximately 15 m-20 m estimated from pictures made during installation while other tree lines and clumps of forested area are present at various upstream positions along the 228° sector. Further analysis of the measurements from the same site was done by Hulsman et al. [8] who showed a non-negligible effect of the tall tree line by comparing the met mast and the VAD lidar data at 100 m elevation. However, the comparison is not directly transferable in our sector of interest due to the orientation of the VAD lidar and the met mast but provides enough evidence of the perturbation of the flow by the same treeline. This is consistent with literature where treelines acting as windbreaks have been shown to perturb the vertical flow profile high above the treeline [5, 14].

Therefore, terrain effects, in particular the treeline could have potentially influenced the induction zone flow. Furthermore, these could have also impacted the WindScanner measurements, in particular, the assumption of $w = 0$ m/s for the dual-Doppler reconstruction which could have influenced the measurement results shown in this paper.

However, we could not quantify the magnitude of these terrain effects on the flow and the lidar measurements. We could not acquire synchronised measurements when both the turbines were non-operational, which would have provided insights into the flow behaviour due to terrain. We acknowledge that a high-resolution LES study with a terrain map could have been used to isolate the flow behaviour due to terrain and the turbine influence. However, the LES runs in our study were intended to study the lidar measurement accuracy and therefore were initialised with a roughness length as a proxy for the terrain complexity.

We have updated the test site characterisation section, and also updated the corresponding results discussion section with the potential impact of the site orthography on the measured flow features.

Comment 3: Following the comment above, in the abstract and the conclusions it is stated that the measurements presented in this article reveal a horizontal asymmetry of the induction zone which the authors claim that it is due to the vertical shear. This statement is based on the observed characteristics of one case (Case 1), with a shear exponent equal to 0.21. On which basis the authors support that this shear exponent is strong enough in order to induce a horizontal asymmetry? And how can they decouple the observed horizontal asymmetry from potential spatial variations of the horizontal flow due the heterogeneity of the terrain?

Reply: Ideally, we would have liked to show at least two undisturbed inflow cases, with a range of shear (or at the least strong and weak), with similar inflow and turbine operational conditions and measured with the same instrumentation. This was impossible in the limited amount of time we could perform measurements. This is the limitation of the study as the outcomes were based on small datasets which is just a small portion of the operational states of the wind turbine.

Bastankah et al, [1], in their wind tunnel experiments noted a "slight asymmetry with respect to rotor axis" with a shear exponent of 0.17 and attributed the reason to the sheared inflow. Our argumentation followed this work and the stronger shear exponent (0.21) that was present during the measurements. However, as discussed earlier, we had discounted the effect of terrain on the flow and the measurements. Performing multiple LES runs with a flat terrain and the map of the terrain with increasing values of vertical shear can potentially decouple the effects of the terrain and the shear on the flow and identify the driving factors behind the asymmetry and provide a strong explanation for our measurements.

We have expanded the discussion of our results mentioned above to include the potential impact of the terrain characterisation on our measurements.

Comment 4: I think that the error values that are used in Sect 3.1, concerning the line-of-sight (0.1 %) and the pointing accuracy (0.1 deg) are rather low. Regarding the line-of-sight the authors use as a reference the work of Pedersen and Courtney 2021 to support the choice of the 0.1 % value. First, the value reported in that study concerns a cw Doppler lidar, but not the Doppler lidars used here. And second, I guess that the line-of-sight error is dependent also on the probe length. Regarding the pointing accuracy, this will be dependent on the scanning speed. I think that the authors should address these points in the "Discussion" section.

Reply: We acknowledge the points made by the referee regarding the line-of-sight and pointing errors. For continuous-wave lidars, the error will be dependent on the probe length which scales quadratically with the focus distance. The work of Pedersen and Courtney [10] suggests a 0.1% error, but as pointed out was with a different lidar system in a highly controlled environment. van Dooren et al ([16]), used the same WindScanner lidars in a wind tunnel study and quantified the error against a hot wire anemometer with a mean average error metric less than 2 %, dependant on the turbulence intensity of the inflow in their set up. The probe length in this study was in the order of 13 cm and 13.9 cm for the two WindScanners respectively. For our field measurements, where probe lengths were in the order of 6.75 m to 27.75 m, an increase in the error will be expected. One way to estimate this error would have been to set up a field measurement, focus the WindScanners with similar focus distances next to a sonic anemometer to obtain representative probe lengths, project the sonic measurements into the line-of-sight and compare the accuracy of the two systems. However, such a campaign was not executed before this study was conducted.

Indeed, the pointing accuracy will be dependent on scanning speed. We had chosen a sinusoidal motion pattern as this could be achieved by continuously rotating the two prisms in one direction only. Sinusoidal scanning therefore reduced the acceleration of the scan head reducing induced vibrations and eliminating the scan reset time. The two prisms in the WindScanner are controlled through a programmable multi-axis motion controller (PMAC), whose input is the angular position of the prisms calculated beforehand for a particular scan trajectory. Depending on the scanning trajectory, speed and the limitations of the system, it might be the case that the prism might not reach its intended angular angular position before performing a movement to the next measurement location. The WindScanner data stream captures the actual angular position along with the commanded motion positions. Figure 2 illustrates this difference ($\Delta\theta_{M1}, \Delta\theta_{M2}$) for the two steering motors for three consecutive horizontal plane scans from the presented measurement setup. For both motors, the values of $\Delta\theta_{M1}, \Delta\theta_{M2}$ did not exceed 0.1°, except for 16 points (0.12%) in each full measurement. These points were located on the very edges of the measurement plane and could not be fully eliminated. As more than 99 % of the measurement points showed an average pointing error of less than 0.1°, we report this number.

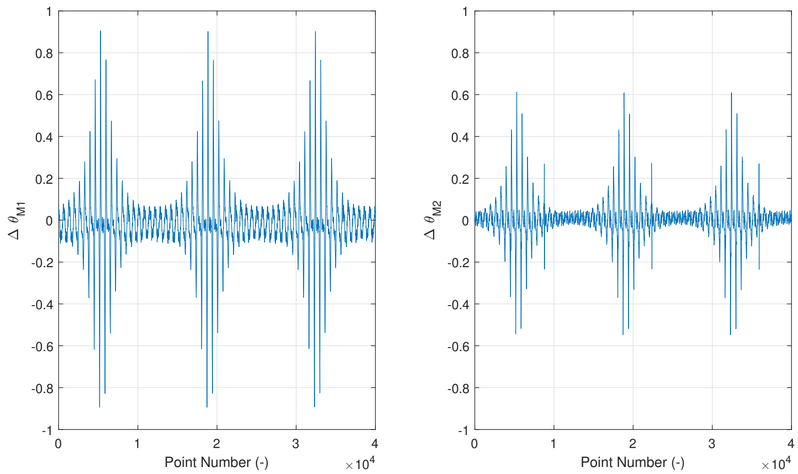

Figure 2: Angular difference between the commanded and actual motor positions for the two steering motors M1 and M2.

We have updated the discussion section with the explanation:
We note that the error in $v_{\rm los}$ of 0.1 % might be low compared to our measurements. The work of [10] suggested a 0.1% error in a highly controlled environment. [16], used the same WindScanner lidars in a wind tunnel study and quantified the error against a hot wire anemometer with a mean average error metric less than 2 % in their set-up. However, the probe lengths in this study were of the order of 13 cm. For our field measurements, where probe lengths were in the order of 6.75 m - 27.75 m, an increase in the error is expected. Further measurements are required, for example, by focusing the lidars against a sonic anemometer with representative probe lengths into the lidar line-of-sight to obtain a representative $e_{\rm vlos}$.

Comment 5: Section 3 presents the results of the virtual Windscanner evaluation using LES. According to the LES the largest values of the "w" component are equal to +/- 0.8 m/s (by taking into consideration the contours of Fig.6 and the inflow free wind speed at the hub height (7.7 m/s). However, for the estimation of the propagated uncertainty a constant vertical component is assumed, with a varying magnitude for each of the cases. Specifically, in Sect. 3.2.1 "w" is equal to 0.2 m/s while in Sects. 3.2.2 and 3.2.3 "w" is equal to 1.0 m/s. Can the authors comment on the selection of those values?

Reply: We acknowledge that the reasoning behind the choice of w velocities for the propagated uncertainty calculations was not detailed in the paper. In the LES analysis, the simulated data provided an accurate value of the local w component variation inside the scanning area. This local w velocity was subsequently used to calculate the propagated uncertainties providing a methodology to investigate the lidar error inside the LES. However, in the free field, we did not have any measurements of the local w component. Therefore, an assumption of a constant w was required for the Standard Uncertainty Propagation (SUP) methodology. For the full wake and partial wake cases, a value of $w = 1$ m/s was assumed, similar to the wind tunnel experiments of van Dooren et al [15]. For the undisturbed inflow

case, the vertical velocity would be only dominated by the temperature flux between the ground and the air, and therefore a conservative value of $w = 0.2$ m/s was chosen based on the weakly stable conditions during the measurements.
The assumption of constant w velocities in the measurement plane for SUP calculations will have a significant impact on our results. Due to this assumption, the errors would be overestimated at locations where the local w component would be low, for instance, the most upstream part of the scan where the aerodynamic influence of the rotor on the flow would not be felt by the wind field. To illustrate this we plot the variation of e_u and e_v in the scanning area for the LES case with an assumption of $w = 1$ m/s in Figure 3.

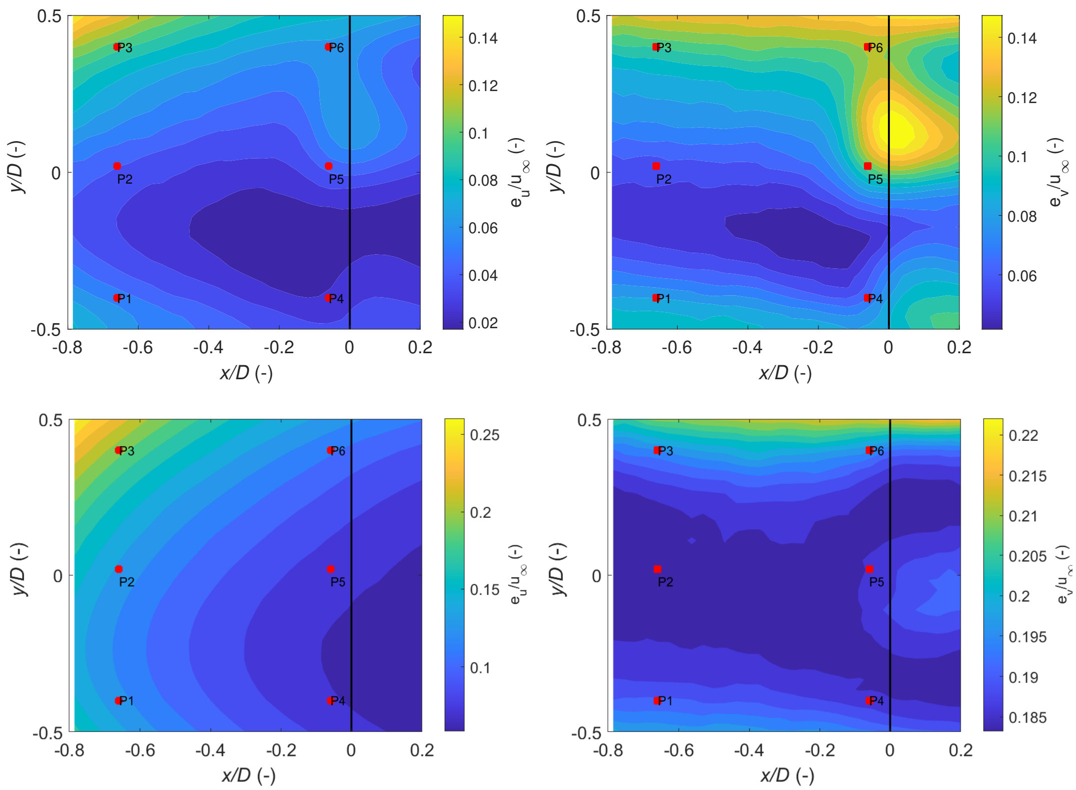

Figure 3: Variation of e_u and e_v in the scanning area using the local w component from the LES (first row) and the assumption of $w = 1$ m/s (second row).

The comparison of e_u and e_v for the two methods indicates a couple of things. Firstly, assuming constant w velocities on the scanning area masks the velocity reconstruction error that is dependent on the flow dynamics, especially close to the rotor. Secondly, the magnitude of e_u and e_v for the constant w velocity case is substantially larger than the errors estimated using the local w velocities. The consequence of this is that the presented error bars in Figures 11, 14, 16 and 17 of the original manuscript are conservative and as pointed out by Reviewer 2, leads to difficulties in the interpretation and significance of the results. A possible workaround would have been to use a third synchronised lidar that would eliminate the assumption of vanishing vertical velocity, however, a third system was not available for measurements. We acknowledge this unavoidable limitation of our measurement setup and have made the following changes in the manuscript.

We have expanded the LES section with the results presented in Figure 3 for the constant w velocity assumption and extended the discussion section, highlighting the assumption and its impact on the significance and interpretation of the measurements.

Specific Comments

Comment 1 — Lines 10 – 12: The authors write: "The measurements revealed more evidence of horizontal asymmetry of the induction zone owing to vertical wind shear under undisturbed inflow conditions". Why do the authors write "revealed more evidence" here, since it is the first time where they mention the horizontal asymmetry of the induction zone?

Reply: We acknowledge that the phrasing of this sentence was not clear in the abstract. Based on the comments of both reviewers, we have rephrased this sentence to the following:
The measurements revealed the existence of a horizontal asymmetry in the induction zone possibly due to a combination of the rotor and terrain effects under undisturbed inflow.

Comment 2 — Lines 14 – 15: The authors write: "We observed that the downstream turbine induction zone during wake steering depended on the direction of the wake steering. . . ". Isn't this an obvious statement?

Reply: Yes, this is an obvious statement. At the end of the sentence, we mentioned that the "lateral movement of the deflected wake could be measured". To our knowledge, this was the first time, the lateral velocities in the deflected near wake were measured in field experiments. Therefore, we have modified this sentence to the following:
We observed the downstream turbine induction zone during wake steering while the lateral movement of the deflected wake could be measured for the first time in the free field.

Comment 3 — Lines 105 – 106: What was the purpose of equipping the mast with a gas analyser?

Reply: The Irgason device is an eddy covariance system which combines both an infrared gas analyzer (IRGA) and a 3D sonic (SON) anemometer combined into a single sensor. For characterising the atmospheric stability, the Obhukov length was used. Therefore, flux measurements were carried out at the met mast at three heights, 2 m, 6 m and 60 m. Additional temperature and relative humidity probes were installed to calculate the mean air density for the moisture correction of sensible and latent heat fluxes. A more detailed description of the method for estimating stability from the Irgason is detailed in Bromm et al., [4]. We have added the following to the text:
More details on the derivation of the Obhukhov length from the Irgason are detailed in [4].

Comment 4 — Line 110: What was the spatial resolution of the inflow lidar?

Reply: The pulse length of the inflow lidar was 25 m. This information has been updated in the revised manuscript.
The inflow lidar was performing VAD scans with a elevation angle of 75° and with range gates set from 50 m to 840 m with a spacing of 5 m and a pulse length of 25 m.

Comment 5 — Line 146, Eq.5: Please describe what is denoted by "x".

Reply: The term **x** denotes the spatial position vector of the measurement point in space. We have updated the text to the following:
The measured line-of-sight velocities of a cw lidar at the position $\mathbf{x} = (x, y, z)$, $v_{\text{los}}(\mathbf{x})$ can be mathematically expressed as the convolution of the wind vector $\mathbf{u}(\mathbf{x})$ projected along the laser beam direction and the volume averaging function:

Comment 6 — Line 151: The effective radius is an important parameter for the operation of a cw Doppler lidar since it determines the spatial resolution of the lidar. How is the value stated here (56 mm) determined?

Reply: We did not conduct any experiments to calculate the effective radius. The effective radius of the WindScanners were reported by DTU Wind Energy who developed the WindScanner systems.

Comment 7 — Line 169: What is meant with the term "greedy controller"?

Reply: The term "Greedy Controller" is used here to describe the control strategy when both the turbines were operated to extract maximum power, i.e., no wake steering was performed when this controller was active. We have rephrased this sentence to the following:
In this sector, active wake steering was performed by toggling between two unique wake steering controllers and one greedy controller where no wake steering is performed, each operational for 35 minutes.

Comment 8 — Line 172 – 173: How were the group of horizontal planes averaged? Where the measurements grouped based on their position in a grid?

Reply: We have described the post-processing of the measurement data in the beginning of Section 3, Results but have moved the text to Section 2.2.1.
For visualisation, the longitudinal and lateral velocities are interpolated using a cubic interpolation scheme onto a uniform grid with a spacing of 10 m. We rotated all measurements in the global reference frame into the main wind direction at the met mast hub height.

Comment 9 — Line 175, Table 2: The authors present in Table 2 a list of different types of errors along with a qualitative characterisation of their impact. I think that this qualitative characterisation rather than general, it depends a lot on the measuring configuration and features of the measured flow. Therefore, I suggest that the authors should either discuss why the impact of these errors are general or mention that this characterisation concerns the specific measuring campaign. For example, the assumption of zero vertical component is probably not high over offshore areas. Furthermore, maybe this table is more part of the "Results" than of the "Methods".

Reply: Agreed that the impact and magnitude of the errors from different sources are highly site-specific. However, we prefer to keep the table at the beginning as it would allow the reader to have an overview of the different errors before reading through the section. To make our table specific for our particular setup, we have modified the following text to:
For this particular measurement setup, the various lidar errors, their impact and their analysis methodology are tabulated in Table 2.

Comment 10 — Line 186: Why is the maximum distance of the cw lidars used equal to 300 m?

Reply: Continuous-wave systems have an inherent maximum range (on the order of a few hundred meters), beyond which it is impossible to focus the beam, because of diffraction [6]. Furthermore, the maximum range of the cw lidars has been specified as 300 m as the measurement volume extends beyond 30 m after this range. This definition of maximum range follows the reasoning of the smaller 3" WindScanners first developed by DTU, whose maximum range was 150 m after which the probe volume extended beyond 30 m.

Comment 11 — Line 189: "This effect is most severe for measurements at the wake edges, . . . " Please add that this statement concerns this study.

Reply: We agree with this statement. The text has been amended to:
This effect concerns our study as is most severe for measurements at the wake edges, as the measurement volume extends from inside the wake to the freestream, and for measurements very close to the downstream turbine WT2, as the measurement volume would extend partially into the turbine wake.

Comment 12 — Lines 202 – 203: What is meant with the term "effective intersection diameter"?

Reply: Giyanani et al [7] define effective intersection diameter for a synchronised WindScanner system as the diameter of a sphere circumscribing the location of the laser beams at the focal point. The effective diameter therefore describes the sphere within which the laser beams are expected to intersect at a particular measurement point.

Comment 13 — Lines 213 – 218: I think that the authors should refer here to already published articles that have investigated the impact of the measuring position to measuring errors in a dual lidar measuring configuration, such as:
Peña A, Mann J. Turbulence Measurements with Dual-Doppler Scanning Lidars. Remote Sensing. 2019; 11(20):2444. https://doi.org/10.3390/rs11202444 Please note that I am neither the author nor the co-author of the above publication.

Reply: We have added relevant references to the paper of Pena and Mann [11] along with the work of Stawiarski et al [13] and van Dooren et al [15] to this section.

Comment 14 — Lines 231 – 234: Eqns. 7 and 8 assume that the uncertainty terms are uncorrelated. Please add this assumption.

Reply: Agreed. We have added the assumption of small errors and the zero correlation between errors for SUP to the text.

Comment 15 — Line 244. What are the unfavourable conditions that the authors refer to? Please elaborate. And what was the impact of the lower availability on the spatial distribution of the measurements? Were they certain areas with systematically lower data availability values than others?

Reply: The majority of the unfavourable conditions were due to periods of rain which impacted our measurements. The spatial availability distribution was dominated by the presence of the wind turbine nacelle and the spinning blades that systematically reduced data availabilities close to and behind the rotor. We have modified the text to the following:

We noticed that many measurements were also affected by unfavourable conditions such as rainfall and lower availability of aerosols to backscatter the laser beam. For operational safety reasons, the WindScanners were operated only with on-site personnel supervision. The measurements were further influenced by the presence of the wind turbine nacelle and the rotating blades that would systematically reduce data availability in the scan region.

Comment 16 — Line 261. What was the agreement between the power law model and the measurements?

Reply: Figure 4 illustrates the averaged vertical wind speed distribution along with the power law fit for the full wake case. In general, a good fit is observed between the measurements and the fit with minor discrepancies noted at the lower and upper parts of the rotor due to the nature of the terrain. Also note that for case 2,3,4, the VAD lidar measurements are influenced by the induction zone of WT1 due to the small separation of 1.9 D.

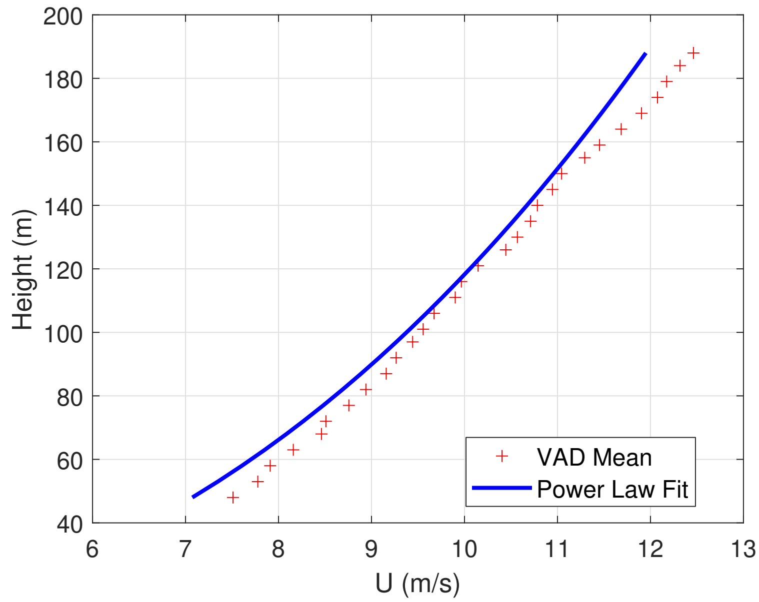

Figure 4: Averaged measurements of the VAD lidar during the occurrence of the full wake case illustrated along with the power law fit.

Furthermore, no flow events such as low level jets which could have impacted our measurements were detected during the investigated measurement periods.

Comment 17 — Line 306, Figure 6 (top row, right), why does the distribution of the "w" component is tilted in respect to the wind turbine rotor?

Reply: The tilted / shifted distribution of the w component could be attributed to the 20 m hub height difference between the upstream and downstream turbines, similar to the field campaign operating in highly veered wind flow that is displacing the wake.

Comment 18 — Line 310: The authors state that there is "an excellent agreement between the LES and the virtual WindScanner..." I think that this is subjective statement. The author should explain why they think that there is a good agreement between the two. Especially when in the next sentence it is stated that there are deviations between the virtual lidars and the LES.

Reply: We agree that the comparison between the LES and virtual WindScanner profiles is subjective. Here, we wanted to describe that the spatial plots of the 2D velocity reconstructions between LES and WindScanner simulations are similar in a qualitative way, showing that the WindScanner could capture the dominant flow structures with the reference LES wind field such as the flow expansion at the rotor tips. Therefore, we have rephrased the text to:
A good qualitative agreement between the LES and the virtual WindScanner resolved u, v profiles are noted at most parts of the scanning area. The simulations reveal that the WindScanners can capture the spatial features in the flow such as the wake rotation and flow expansion at the rotor tips.

Comment 19 — Lines 316 – 335. The authors state that Fig 7. shows that error in "u" is at larger the WT2 rotor plane. However, from the contour plot this is not obvious. For example, the error in P2 is similar to the one at P5 and higher than the P6. Furthermore, the maximum error in the "v" component when $y/D >0$ looks that is higher than 14%.

Reply: The u component error was stated to be large at the rotor plane at locations where high local w velocities were present. This can be seen in the seen in Fig.06 top right where the spatial variation of the vertical velocity is shown. The error in P2 and P5 are similar (4.2 % and 4.6 %) due to the similar w velocities while at P6, the u component error is 6.7 % owing to the larger local w component present here.
For comparative purposes, we presented the colour axes for both e_u and e_v up to 0.2. We have modified the colour limits of this plot in the revised manuscript where it is clear that the maximum value of e_v at $y/D > 0$ is 15%.

Comment 20 — Figure 8. It is very difficult from the colour of the bars to identify the contribution of each error. Can you please choose better colours?

Reply: We have updated the figure and assigned a separate colour for the eight error terms for easier visualisation.

Comment 21 — Line Figure 12 presents the velocity deceleration of the longitudinal wind speed along centre of the rotor. The figure presents that the model based on the 1D Vortex sheet theory fails to reproduce the observed wind characteristics. I am wondering to which extent this is observed due to the model or due to a non-optimum selection of the free wind speed and of the induction zone factor. Have the authors tried to estimate the free wind speed and the induction zone from the Windscanner measurements?

Reply: The authors agree with the reviewer that an improper selection of induction factor will have a substantial influence on the modelled flow deceleration. We had indeed investigated estimating u_∞ and the axial induction factor from the measurements following the work of Borracino et al [3] with the 1-D vortex sheet theory. A negligible difference was seen between the measured and modelled rotor axis velocity deceleration and hence was not reported in the paper.
The most likely reason for the discrepancy is a bug in the coupling between the induction zone models and the wake models in FLORIS. This reasoning is supported by the good agreement between the standalone induction zone models that are used in the coupling and the field measurements.

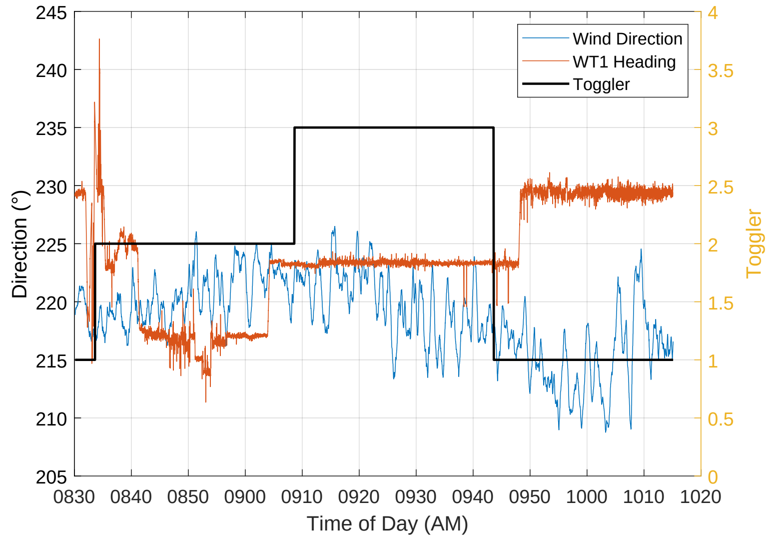

Figure 5: Wind direction from the met mast and WT1 heading on 25.02.2021. The period from 09:45AM to 10:15AM UTC was when the 12.8° yaw offset case was recorded.

Comment 22 — Line 450. Figure 15 presents the longitudinal component of the wind measured for two different yaw misalignment angles. I can understand the spatial distribution of the "u" component, where the trace of the wake propagates mainly at the region where $y/D < 0$. One the other hand when the yaw misalignment of the WT1 is equal to 12.8 degrees then wake tract is in the region where $y/D > 0$. However the direction of the propagation of the wake seems strange. I would expect that the wake should move upwards, but from the measurements it looks that it is moving downwards. Can the authors comment on this?

Reply: For the 12.8° yaw offset case, the spatial distribution of the longitudinal component seems to indicate that the wake was deflected towards $y/D \geq 0$, but closer to the rotor deflects downwards. This could be possibly because of how the measurements were chosen for analysis. The WindScanner was performing measurements when the wind direction was aligned with the two turbines. The mean wind direction for the 12.8° case was 217° already creating an offset as both the turbines are not completely aligned, as in the other three cases. Furthermore, the standard deviation of wind direction at approximately 10°, was the highest amongst all the investigated cases. We show the wind direction at the metmast and the WT1 turbine heading in Figure 5. Here the toggler value of 1 indicates when the measurements were recorded for analysis. During the time of measurements, the wind direction ranged from 208° to 223°. This comparatively large variability in wind direction during measurements could be the most possible explanation influencing the spatial wind field distribution over the individual scans, and therefore the averaged results. We have added the following text to the modified manuscript:

It is noted that for the positive offset case, the spatial distribution of the u component seems to move near the rotor axis instead of deflecting towards $y/D > 0$. This could be potentially attributed to the 10° misalignment between the wind direction and the turbine orientation direction in addition to the large variability of the wind direction from 208° to 223° which was the highest of all investigated cases.

Minor Comments

Comment 1 — Line 5. Please change the "The measurements were conducted with..." with "The measurements were acquired by..."

Reply: We have changed this in the revised manuscript.

Comment 2 — Lines 96 – 97: Please add in this sentence that WT1 is the upstream wind turbine and WT2 is the downstream.

Reply: We have changed this in the revised manuscript.
The upstream and downstream turbines are abbreviated as WT1 and WT2 respectively..

Comment 3 — Line 103: Please replace the verb "outfit" with the verb "equip"

Reply: We have changed this in the revised manuscript.

Comment 4 — Lines 104-105: Please add the names of the Theis products.

Reply: We have added the following to the text:
Inflow conditions were measured by a met mast placed 2.6 D north of WT1, equipped with two anemometers,Thies First Class Wind Transmitter anemometer of type 4.3352.00.400 from at the lower tip of 54 m and close to the WT1 hub height of 116m. A wind vane of type Thies First Class Wind Direction Transmitter of type 4.3151.00.212 is also installed at 112 m.

Comment 5 — Line 140: What is meant with the "Without generalisation ..."? Please clarify.

Reply: We have used the terminology without generalisation to indicate that our assumption of a vanishing vertical component might not be the only way to reconstruct two-dimensional wind fields from dual lidar measurements.

Comment 6 — Line 167: Remove the dot after "Table"

Reply: Removed.

Comment 7 — Line 168: Table 1. Label: Is it Fig. 2(b) or Fig. 2(a)?

Reply: The reference is for Fig. 2(a). We have made the change in the revised manuscript.

Comment 8 — Line 183: Figure 4. Replace "The doted lines..." with "The dashed lines..."

Reply: Replaced.

Comment 9 — Line 187: Please delete the second "averaging"

Reply: The repetition has been deleted.

Comment 10 — Line 252, Table 3. Please add a description of each column in the label of the table.

Reply: We have added the following to the caption:
Summary of the measurement cases. Each case is characterised by its freestream wind speed u_∞, turbulence intensity (TI), mean wind direction (θ_{wdir}), stability parameter (z/L), stability, wind veer (γ), vertical wind shear (α_{shear}) and the yaw offset of the turbines (γ_{WT}).

Comment 11 — Line 317. Replace Figure 7 with Fig. 7

Reply: Replaced.

Comment 12 — Line 325. Replace Figure 7 with Fig. 7

Reply: Replaced.

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