In this document, the reviewer comments are in black, the authors responses are in red.

The authors thank Dr. Nygaard for his detailed review and useful suggestions to improve the quality of our work.

Summary: this paper presents single lidar measurements of multiple wind turbine wakes. A wind field reconstruction model is used to derive wake characteristics from the line of sight wind speeds. The presented model is an extension to multiple wakes of a previous model developed by the same research group to characterize a single wake. Results are presented for the decay of the velocity deficit and the increase of the wake width with downstream distance. The wake centerline is found to shift orientation with height, indicating stretching of the wake profile in veered flow. The results are interesting and add to the developing picture of wakes in wind farms.

Thank you for finding our results interesting!

The method is sound, but could have been better explained, especially with regards to the vertical dimension and the influence of multiple elevation tilts (see specific comments below). The authors find some differences between the outer and the inner turbines in a row, but no explanation for these differences is offered.

The paper is definitely worthy of publication, but I suggest the authors consider the questions and suggestions below to improve the presentation of the methodology and the results.

Questions and suggestions:

In the model of Eq. (4) there is no reference to the vertical dimension or the elevation angle. Is it supposed to be applied at a fixed elevation angle? It is unclear how the vertical structure of the wakes is considered. At a fixed elevation angle the laser beam will probe at increasing height with increasing range. Were multiple elevation tilts combined in figures 7 and 8 to account for this?

Yes, equations (3) and (4) are applied at each fixed elevation angle. Then, to compute results in Figure 7 and 8, all the six elevation angles used in the campaign are combined to produce the final results.

To make this clear, we will modify the sentence that introduces our models as follows: “At a fixed elevation angle, the line-of-sight velocity u_LOS can be related to …”.

Moreover, to make clear that Figures 7 and 8 use data from all the elevation angles, we rephrased the sentences below as follows:

- “Figure 7 shows velocity deficit versus downwind distance from the turbines, calculated

from the 276 PPI scans (at all the elevation angles) performed during the whole night (stable conditions) of 26 August 2013."

- [Caption of Figure 7] "Continuous lines represent the median values calculated from the PPI scans performed at all the considered elevation angles during the night (stable conditions) of 26 (panel a) and 23 (panel b) August 2013".
- [Caption of Figure 8] "Wake width vs downwind distance from the turbines, for the wakes of the four turbines in the studied row, from PPI scans performed at all the six considered elevation angles."

Consider adding a reference to Wang and Barthelmie - Journal of Physics: Conference Series 625 (2015) 012017 - Wind turbine wake detection with a single Doppler wind lidar. This has a similar wake wind field reconstruction method.

We will add a reference to this paper in the introduction.

P1, L23: The reduction in power for turbines in wake can exceed the 40% mentioned as the upper limit. As is well known, it is very sensitive to wind direction, being largest when the wind is aligned with rows of turbines. Also the observed maximum reduction depends on the size of the wind direction sector over which the data are averaged. But even for a 30 degree sector Nygaard 2014 found reductions of up to 60% for certain conditions. The total wake loss considering all wind directions and wind speeds is typically less than 20%. My point is that the 40% mentioned in the text is a meaningless number without further context. It only applies for certain wind directions and for averaging over a sector of a certain size. I invite you to make the context of this number clearer or to consider, if a specific value is needed.

Thank you for pointing out that we should have been more careful with this. Since, as you explained, giving a precise number for this energy reduction is not possible, we will eliminate this sentence from the manuscript, and include the references to Barthelmie et al. 2010 and Nygaard 2014 in the previous sentence, along with other papers which deal with energy reductions in wakes.

P2, L11: the appropriate reference for the Jensen model is N. O. Jensen, A note on wind generator interaction, Risø-M-2411 (1983). The reference you have to Jensen 1984 was new to me, so thank you for pointing it out. However, on a quick browse through that paper I did not see any mentioning of the Jensen wake model. The Jensen 1983 report is often cited together with the 1986 paper by Katic, Højstrup and Jensen, which introduces the method of wake superposition employed in the Park model in WAsP.

Thank you for the useful suggestion. We will modify the reference, which will include Jensen 1983 and Katic et al. 1986.

P2, L28: it is important to include more references on capturing multiple wakes in wind farms. At present only two are listed. But since this is the main focus of the paper it is crucial to establish the existing state of this area of study. “Among others” is insufficient. Here are some suggestions: Hirth et al., Wind Energy 18, 529 (2015) - Coupling Doppler radar-derived wind maps with operational turbine data to document wind farm complex flows, Hirth et al., Wind Energy (2015) - Dual-Doppler measurements of a wind ramp event at an Oklahoma wind plant, Kumer et al., Energy Procedia 80 245 (2015) - Characterisation of single wind turbine wakes with static and scanning WINTWEX-W LiDAR data (already cited elsewhere in the paper), Wang and Barthelmie paper mentioned above, Van Dooren et al., Remote Sens. 2016, 8, 809 - A Methodology for the Reconstruction of 2D Horizontal Wind Fields of Wind Turbine Wakes Based on Dual-Doppler Lidar Measurements

Thank you for pointing out that we should improve the references here. We will substantially improve this paragraph with the references you suggest: “The interactions between multiple wakes must be captured in studies of large wind farms, as done by Clive et al. (2011); Hirth et al. (2015a, b); Kumer et al. (2015); Wang and Barthelmie (2015); Aubrun et al. (2016); van Dooren et al. (2016).”.

P3, L12: a photo or photo collage would greatly assist the understanding of the setup of the field campaign, the description of the surroundings and possibly the interpretation of the results.

The following pictures showing the instruments at the site of the campaign (and its land use) will be included in the Supplementary Material.

Figure 1: scanning lidar co-located with WC-3, looking to the southwest (photo courtesy Paul Quelet)

Figure 2: WC-1, looking towards SE (photo courtesy Lundquist)

Figure 3: WC-3 looking to SE (photo courtesy Lundquist)

Figure 4: WC-2 looking to SE (photo courtesy Paul Quelet)

P3, Fig.1: the figure is good, but should only include the relevant information. Were all the surface flux stations used in determining the atmospheric stability? Were all profiling lidars used in the analysis? Leave out the details not connected with this paper.

Although we agree that the figure includes some instruments that were not directly used in our work, we still think that it is important to give a complete overview of the set-up of the instruments at the campaign. However, we will improve our description in the manuscript to make clear which instruments we used for our research.

Regarding the different surface flux stations, we will include the following sentence in Section 2.1.2 to clearly states which station we used in our work: "We used measurements from the surface flux station ISU_3 to assess atmospheric stability conditions, with the calculation of Obukhov length".

Regarding the different vertical profiling lidars, see the comment below.

Sec 2.1: the text should specify clearly how the profiling lidars were used in the analysis. Section 2.1 has a brief mention of a comparison between the scanning lidar and WC-3. How was this done? What did the results show? WC-3 was used to determine veer on page 14, but please introduce this in the description of the observational dataset. Was WC-2 used for anything in the present study?

Thank you for noticing that we should be more clear regarding how we used data from the different instruments deployed at the site. Regarding the comparison between WC-3 data and scanning lidar data, the manuscript already includes the reference to the Vanderwende's et al. paper where this comparison is described in detail. Our paragraph includes the following sentence about the results: "Vanderwende et al. (2015) demonstrated good agreement between the co-located scanning and WC-3 profiling lidar measurements at the altitudes where measurements overlapped."

Regarding how we used data from the different profiling lidar, we will add this sentence in the description of the observational dataset: "At CWEX-13, southerly wind conditions dominated the campaign. So, we used data from the WC-1 profiling lidar to measure upwind conditions for the studied row of turbines, and calculate the ambient wind veer." We did not use data from WC-2 and WC-3 in the retrievals of wake characteristics.

P4, L15: the period at the beginning of the line belongs at the end of the previous line Sec.2.1.1: I miss a detailed description of the scan patterns. What was the sector size for the PPI scans? What was the time per scan? This is hinted at on P14, but it belongs in this section. What was the order of the RHI and PPI scans? Am I right that the RHI scans are not used in the present analysis? If that is correct, then please state that. Was the pointing accuracy of the scanning lidar checked using hard target returns (eg from the wind turbines)? This information is important to interpret the results and should be included. Alternatively, if the authors have made this information available elsewhere, a reference could suffice.

Thank you for pointing out that we did not provide this piece of information in the proper location and that we should provide a more detailed description of the scan patterns. We will add the following sentence: "each PPI scan lasted approximately 100 seconds, spanning an azimuth range of 50deg with a speed of 0.5deg/s, while a RHI had a duration of about 30 seconds." We will also add the following table with more details about the scans performed during the field campaign:

Table 2. *Description of the 30-min cycle of scanning lidar scans in CWEX-13 field campaign. The characteristic fixed angle refers to the elevation angle for PPI and VAD scans, the azimuth angle for RHI scans.*

number of scans	type of scan	characteristic fixed angle	duration of each scan	cumulative time
2	VAD	75°, 60°	132 s	0:00 - 4:24
6	PPI	2.8°, 2.5°, 2.2°, 2.1°, 1.8°, 1.5°	104 s	4:24 - 14:48
3	RHI	160°, 170°, 180°	32 s	14:48 - 16:24
6	PPI	2.8°, 2.5°, 2.2°, 2.1°, 1.8°, 1.5°	104 s	16:24 - 26:48
6	RHI	160°, 170°, 180°, 180°, 170°, 160°	32 s	26:48 - 30:00

We did only use PPI scans to detect wakes, and this is stated at the beginning of section 3: "The line-of-sight velocity measured by the WINCDUBE 200S scanning lidar (Figure 3) during the horizontal (PPI) scans can be analyzed to determine wake characteristics and how they evolve in space as the wakes propagate."

P5, Fig. 2: how large was the change in elevation between the lidar location and the turbines? State what is was, then argue why it can be neglected. Otherwise, make an assessment of the uncertainty or bias it introduces into the results.

The change in elevation between the turbines and the scanning lidar was 7m. This will be included in the caption of the figure. However, we DID take into account this difference, as already stated in the caption itself.

P6, L3: the Monin-Obukhov length has dimensions of Length. I assume the listed limits should be in meter.

Thank you for noticing this. We will correct the manuscript accordingly.

P6, L6: insert a "The" at the beginning of the line. Same on line 23 after Figure 3.

We will add "the" in both the proposed sentences.

P6, L12: define carrier-to-noise ratio. Explain the filter on CNR (<-27dB) and why this is necessary. Why was this threshold chosen? How sensitive are the results to this value?

We will add more details regarding the CNR threshold: "First, a threshold is imposed to the carrier-to-noise ratio (CNR), which represents the strength of the backscattered signal compared to background noise (values closer to 0 dB indicate a stronger signal relative to the noise): all measurements with carrier-to-noise ratio CNR < -27 dB are discarded from further analysis [Vanderwende et al. 2015]. Measurements with a lower CNR often had unrealistically high (> 15

m/s) values of radial velocity; and this threshold value is comparable with choices in other studies [Cariou et al. 2011, Bastine et al. 2015, Debnath et al. 2016]."

P6, L13: do data refer to the line of sight wind speeds?

Yes. To make this clear, we will change the sentence in "…line-of-sight velocity data…".

P6, L13: define \mu and define MAD in an equation. Is the standard deviation equal to MAD (it is cryptically stated to be evaluated according to MAD)? How sure are you that the outliers removed are not physical? Do you know the source of the outliers you exclude?

We will improve the description of the MAD method as follows: "in each scan, line-of-sight velocity data which are not included in the interval $(\mu - 3\hat{\sigma}, \mu + 3\hat{\sigma})$, where μ is the average of the data, are removed from the analysis. The standard deviation $\hat{\sigma}$ is evaluated according to the median absolute deviation (MAD), assuming normally distributed data: $\hat{\sigma} = 1.4826\ \mathrm{MAD}$, where $\mathrm{MAD} = \mathrm{median}(|u_{LOS,i} - \mathrm{median}(u_{LOS})|)$".

The threshold was chosen as in Aitken et al. 2014, and, under the assumption of normally distributed data, 99.7% of data are included in the selected interval. Thus, the discarded values can be considered to be due to extreme events such as hard strikes.

P6, L23: I suggest inserting "the assumed" in front of uniform ambient wind speed.

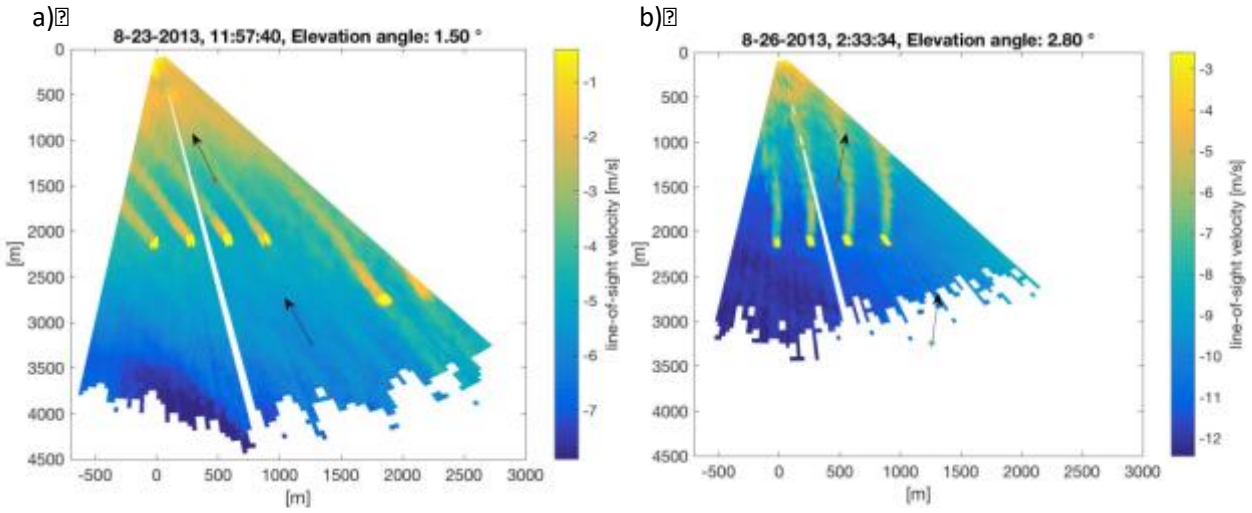
Thank you for the suggestion. We will add "the assumed" in the sentence.

P7, L1: there is a "the" missing in front of ambient flow speed.

We will add "the" in the sentence.

P7: it would be very useful to have an overview image or map showing the turbine locations and the scanned sector.

The following figure, showing the location of the scanning lidar, of the four turbines, and the line-of-sight velocity measurements during two PPI scans (one for each of the studied days), will be added to the manuscript at the end of the "Lidar measurements" section. The Figure will be introduced in the paragraph as follows: "Figure 3 shows examples of maps of line-of-sight velocity measured by the scanning lidar during two PPI scans performed at night on the selected days. The wind turbine wakes can clearly be detected in terms of reduced wind speed downwind of the four wind turbines."



The caption of the Figure will be: "Figure 3. Color maps of line-of-sight velocity measured by the scanning lidar during two PPI scans performed at 11:57 UTC (6:57 am LDT) on 23 August 2013 (panel a) and at 02:33 UTC (9:33 pm LDT) on 26 August 2013 (panel b). The scanning lidar is located in the origin of the coordinate system. The two arrows show wind direction as measured by the profiling lidars WC-1 and WC-2 at 80 m AGL."

P7, Fig.3: to define the wind direction there should be an indication of the north (or south) direction in the figure. Is south upwards in the figure?

North is upwards in the Figure. To make this clear, we will modify the Figure adding a North direction arrow, and rephrase a sentence as follows: "…both alpha and phi are >0 for clockwise rotations from North."

P9, L19: MAD acronym was already defined. When the method was applied again, did you use the same bounds as on page 6?

We will eliminate "median absolute deviation" from the sentence since we already defined MAD previously. Yes, we used again 3 standard deviations as threshold. The sentence will be rephrased as: "the MAD method is applied again to discard wake characteristics which do not lie within three standard deviations of the mean".

P9, L20: "mean characteristic" – is this for the entire database or for a single scan?

Thank you for noticing that we didn't specify this clearly. The sentence will be rephrased as: "the mean characteristic at each range gate for each whole night".

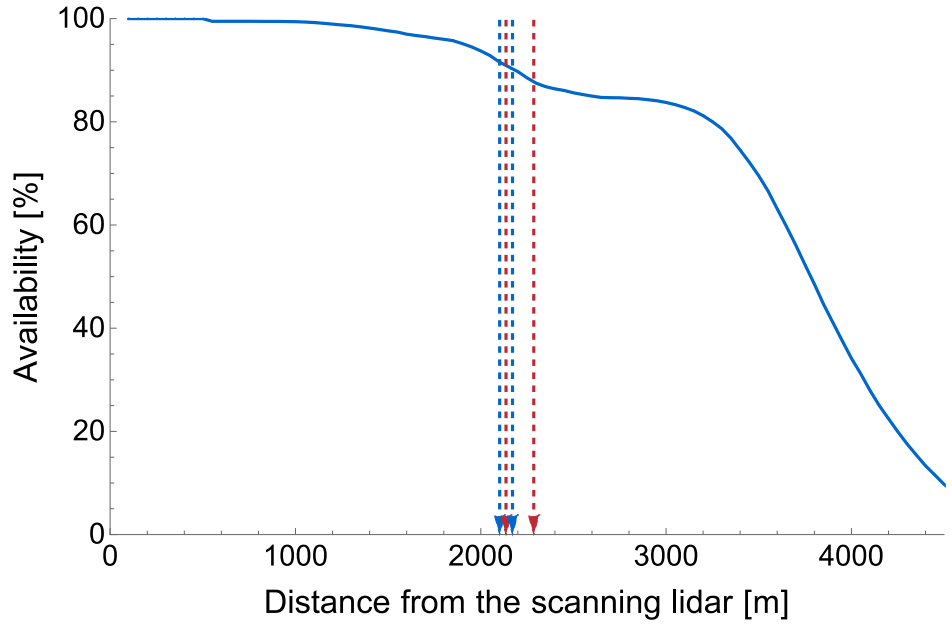
P9 L21: define the Pearson correlation coefficient and the mean squared error.

We will rephrase the sentence as follows:

with Pearson correlation coefficient ($corr(u_{LOS}, \hat{u}_{LOS}; g) = cov(u_{LOS}, \hat{u}_{LOS}; g) / \sqrt{cov(u_{LOS}, u_{LOS}; g) cov(\hat{u}_{LOS}, \hat{u}_{LOS}; g)}$, where g represents the data weights) larger than 0.9 and mean squared error ($MSE = \frac{1}{\sum_{i=1}^{n} g_i} \sum_{i=1}^{n} g_i (\hat{u}_{LOS,i} - u_{LOS,i})^2$) lower than 0.5 are included in the final analysis of the results.

P10, Fig. 5: what do the corresponding plots of data availability look like? These could be important to include to understand if the some of the decrease in Figure 5 is driven by the measurements and not by wake characteristics. I would also like to know the number of scans included in the stable and unstable curve. Were there no neutral conditions?

Since we considered southwesterly (26 August 2013) and southeasterly (23 August 2013) flows, as the wakes go further from the turbines, they actually get closer to the scanning lidar (see scheme in Figure 1), so we are not concerned about data availability at long downwind distances (i.e. close to the lidar). However, the following plot showing data (line-of-sight velocity) availability for 23 and 26 August will be included in the Supplementary Material. The vertical dashed lines show the positions of the turbines. As can be seen, the data availability for all the range gates considered in Figure 5 (i.e. to the left of the dashed lines) is nearly 100%, and so our results are not affected by this possible issue.



Regarding neutral conditions, we will add the following sentence to the caption of the Figure: "(neutral conditions were detected only for very short periods, and are not included here)". So, nearly all the PPI scans performed during the selected days are included in Fig. 5.

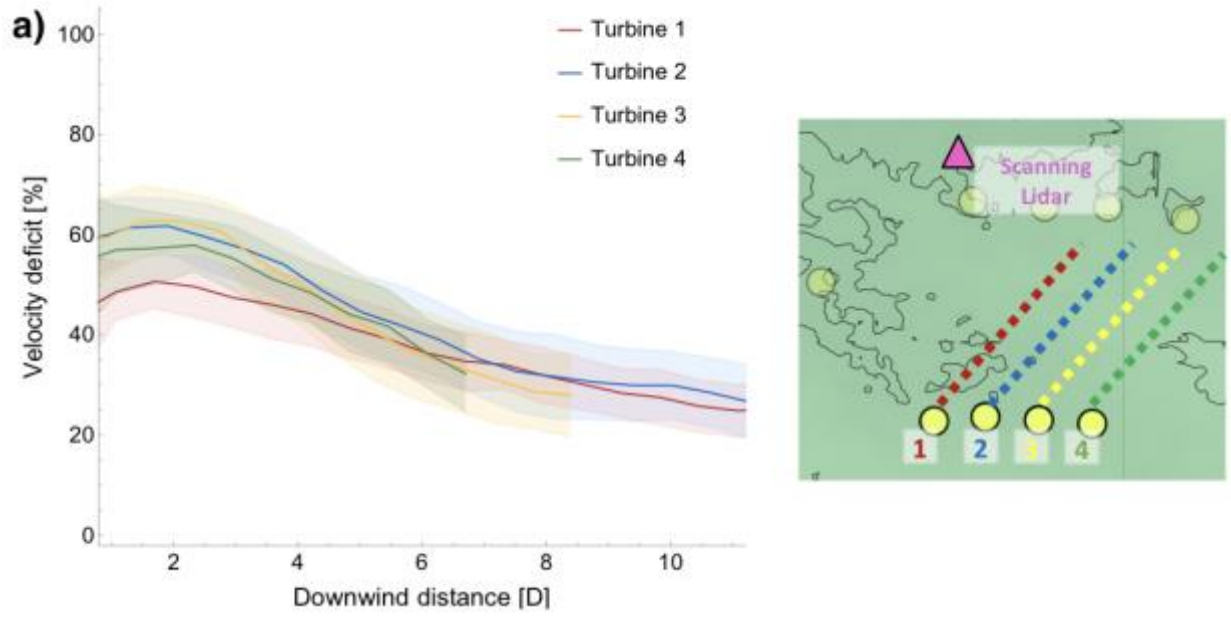
P11, Fig. 6: are these plots along the wake centerlines? What was the wind direction and how was

it oriented with respect to the turbine row? This can be deduced from figures 8 and 9, but you might as well make it clear here.

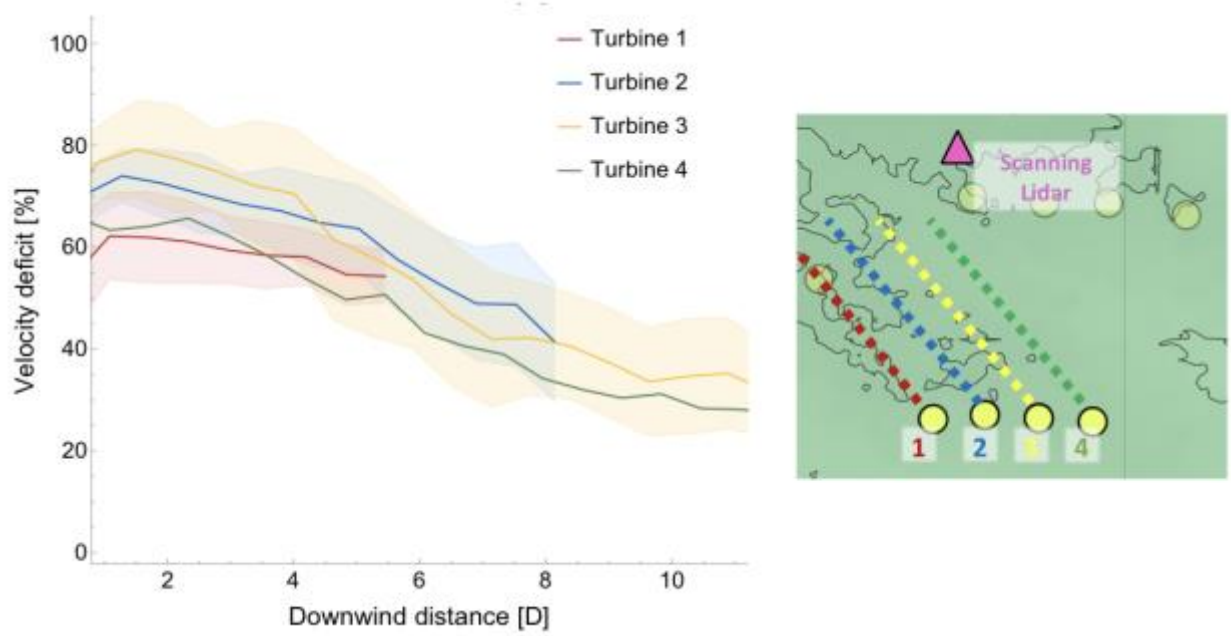
Considering how we modeled each wake (equation 4) and how we defined the velocity deficit (equation 5) in the wake, the velocity deficit is by definition calculated at the location of the center of the wake – thus the wake centerline. Regarding the wind direction, it is stated in Section 2.1.1 that 26 August shows predominant southwesterly wind. However, as a reminder, the caption of the figure will include: "… 26 August 2013, southwesterly wind, …".

P11, Fig. 7: the authors should attempt to explain the differences they see. While the two outer turbines both have smaller deficits than the inner turbines, the difference in deficit between the two outer turbines is larger than the difference between the inner turbine and the outer turbine with the highest deficit. It would be useful to label the curves with the turbines they belong to. Could there be a relation with the vertical structure of the wakes and sampling the wakes at different heights. As stated above it is important to know how the different elevation tilts were combined (or not) in making this figure. Is the width of the shaded bands one or two standard deviations? I suggest making the same plot for the 23 August data.

We will change the Figure as follows to highlight the behavior of the single turbines:



And we will include the following plot from 23 August:



The paragraph which introduces this new version of the Figure will be changed accordingly: "Figure 7 shows velocity deficit versus downwind distance from the turbines, calculated from the 276 (242) PPI scans, at all the elevation angles, performed during the whole night - stable conditions - of 26 (23) August 2013."

As can be seen, for the 23 August 2013 the difference between outer (turbines 1 and 4) and inner (turbines 2 and 3) wakes is more consistent for all the studied turbines, and the phenomenon noticed by the reviewer does not appear. Therefore, we cannot infer general results beyond what we stated in the paragraph: "wakes from outer turbines (number 1 and number 4), have lower velocity deficits than the wakes from inner turbines (number 2 and number 3), for relatively small downwind distances, with a difference up to 15%".

Regarding the use of different elevation angles, the caption of the Figure will include: "Velocity deficit vs downwind distance, for the four wakes of the studied row of turbines. Continuous lines represent the median values calculated from the PPI scans performed at all the considered elevation angles during the night (stable conditions) of 26 (panel a) and 23 (panel b) August 2013;".

Regarding the shaded bands, we will rephrase the description in the caption as "shaded areas show $\pm$ one standard deviation of the data" (and the same was added for the caption of Figure 8).

P 11, L 4: replace low with small

We will replace the adjective. Thank you for suggesting!

P12, L 12: the passive voice makes it hard to understand the sentence. Consider rephrasing it. I think you mean widest, when you write largest.

We are not sure which passive voice you refer to. In any case, we will slightly rephrase the sentence as: "…the scanning lidar systematically identifies as the widest the wake which, at a given downwind distance, is the most perpendicular to the laser beam…".

P16, Fig. 11: I am not sure I agree with the conclusion of a larger angular difference for the outer turbines based on the linear fits. The linear fits are very poor. Indeed, the data could be seen as describing an oscillation, where the inner and outer turbines follow each other.

We agree that the quality of the linear fits is rather poor, however we think they are useful to show the different behavior between wakes from inner and outer turbines. To make this clear and to include your suggestion about oscillations, we will rephrase the description of these results as follows: "as suggested by the linear regression fits, wakes from outer turbines often present a larger angular difference in wake centerlines compared to wakes from inner turbines, though with variability for different veer values that motivates further study".

P20, L16: &amp should be &.

Thank you for catching this mistake!

P21, L14: Spera, D should be Neustadter, H. E. and Spera, D. and the page number is 240 not 241.

Thank you for noticing. We will correct the reference accordingly.