Answer to reviewers

Dear Sir/Madam,

We thank the reviewers for the feedback on our manuscript. Below is our answer to the reviewers.

Reviewer 2

Reviewer's summary:

This is a very well prepared manuscript, a very relevant subject, and a step forward in experimental investigation of the spatial structure of turbulence over the sea. The authors demonstrate a good knowledge of the field and they have devised a fascinating experiment that deserves wide acclaim.

There are some main issues with the paper.

It is a pity that the authors spend energy to present the size of the larger rotors of today (up to 164 m in diameter) but that the separations studied are not anywhere near those. In their example study on coherences the lateral separation is only 21 m. It is so small because they cannot use the data form the third lidar (lidarS) because it is misaligned by 7 degrees which they ingeniously estimate. This bring me to the more series issue which is related to the first. The authors' main result is probably figure 16 where it is shown that the coherence decay coefficient C_x decreases with distance from the coast until it reached a minimum between 1 and 1.5 km. The variation is very strong starting at a value of 10 and reaching a value of 2 at the minimum corresponding to larger coherence at this distance.

After the minimum the decay coefficient increases significantly again. So between 1 and 1.5 km the coherence is strongest.

However, the issue with the direction of lidarS raises the suspicion that lidarW and lidarN are neither well aligned. If lidarW and lidarN are misaligned (converging) with just 0.5 degrees, then the lateral separation distance d_y at 1000m would suddenly only be 10 m, not 20 m. The corresponding decay coefficient C_y would be a factor of two too large. I am not convinced at all that lidarN and lidarW do not have this or even larger misalignment. An additional observation casts doubt on the results. Let's focus on the middle plot of figure 17. The coherence here is from a (nominal) separation of 21 m, so it is almost in the inertial subrange since the hight above sea level is more than 100 m. Here the coherence is given by theory (Kristensen and Jensen , 1979), compare with Mann (1994) figure 8 top left which shows the squared coherence. At $k_1 * d_y = 0.4$ the squared coherence is 0.5 corresponding to a co-coherence of 0.7. In figure 17 (middle) this happens at a frequency $f = 0.1$ Hz. That corresponds to $k_1 = 2pif/U = 0.045$ or $k_1 * d_y = 0.94$. The theory says (dotted line in the Mann figure) that the squared coherence should be around 0.1, so the co-coherence should be approximately 0.3. A good working hypothesis would be that the beams converge and that they reach a minimum distance at around 1200 m. This could explain figure 16.

In order for this paper to be published the authors have to account for the pointing uncertainty and its possible consequences. I hope they are able to do that because it is apart from this aspect a very nice paper.

General comments

A summary of the procedure to correct possible azimuth and elevation angle offsets is presented in section 4.3. A new section (section 3.4) describes the sensitivity of the pointing accuracy on the identified decay coefficient and shows that there exist some sectors that cannot be used to study the co-coherence because the separation distance is close to or smaller than the measurement uncertainty. Unfortunately, we cannot include a more detailed assessment because this would be beyond the scope of the manuscript, which aims to present the measurement campaign.

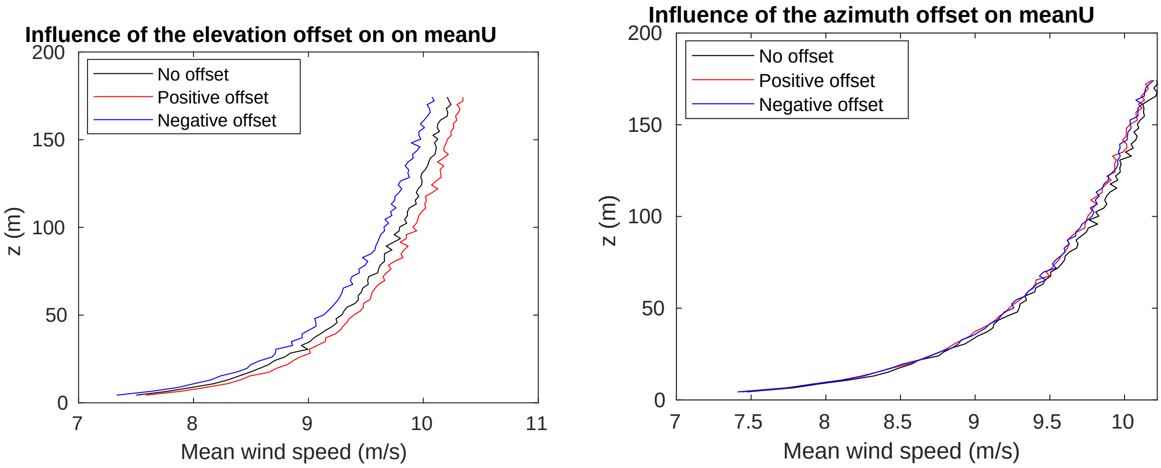

Figure 1: Influence of an elevation offset of 1° (left) and an azimuth offset of 6° (right) of a noisy idealized logarithmic mean wind speed profile.

In section 4.3, we have reassessed the assumption that the beams are parallel by considering that there exists an "offset" for the azimuth and/or elevation angles. Here, the term offset refers to a relative deviation from a reference elevation and azimuth angle. In the case study, the reference values are taken from LidarW. The distance between the scanning beams is small enough so that the mean flow is considered uniform in the horizontal plane between the scanning beams. Therefore, by comparing the slant mean wind speed profiles from the three lidars, it is possible to estimate the offsets for the elevation angle (fig. 1). For the azimuth angle, the identification based on the mean wind speed profile is more challenging, as shown by fig. 1, especially since the sign of the azimuth cannot be identified by this method. It requires an additional comparison of the correlation coefficients between the scanning beams.

The identification of the offsets relies on a two-step process: Firstly, the azimuth offsets are estimated using the correlation coefficient between adjacent velocity records for a given range gate. The maximal correlation indicates the range gate where the beams are intersecting, as shown in Fig 15 of the revised manuscript. Once the azimuth offsets are estimated, the elevation offsets are calculated by minimizing the root mean square error (RMSE) between the reference mean wind speed profile from LidarW and one of the other Lidars corrected for the azimuth offset. Preliminary tests with noisy idealized profiles indicate that the elevation offset can be estimated within +/-0.1° with this method.

For the case study, the relative azimuth offsets were $-0.4°$ and $6.3°$ for LidarN and LidarS. In the original manuscript, the value of $7°$ was mentioned for LidarS but a more detailed assessment revealed it was actually around $6.3°$. The relative elevation offset were $-1.4°$ for LidarN and $-0.4°$ for LidarS. In fig. 2, which uses arbitrary azimuth offsets for LidarN, the median value of the decay coefficient C_y ranges from 8 to 11. In the present case, errors on the lateral distance at scanning distances larger than 1 km have a limited influence on the decay coefficient C_y because the selection of the range gates with the same altitude leads to large along-wind distances, which reduces the values of the co-coherence. It should be noted that in Cheynet et al. (2017, Fig. 11), it was found that studying the along-wind co-coherence with a separation distance of several hundreds of meters was still possible with a pulsed wind Lidar instrument. Further work is, however, required to know how reliable is the method to identify the azimuth and elevation offsets. As a result, Figures 17, 18 and 19 have also been redrawn.

On the coherence of turbulence at large cross-wind separations

The estimation of the lateral co-coherence at a separation distance of 100 m or higher, which is relevant to offshore wind turbine design, has major shortcomings. At such distances, only a few data points

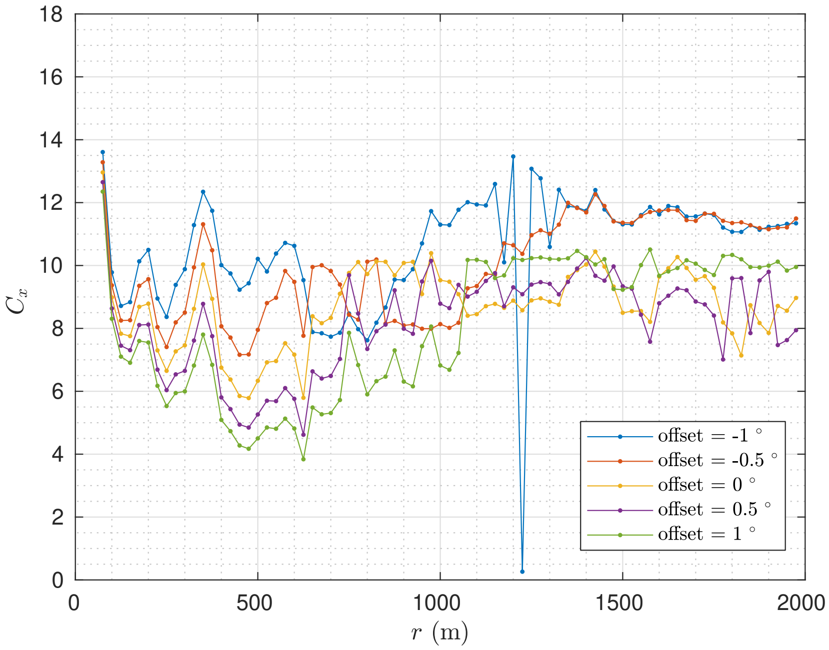

Figure 2: Sensitivity of the lateral decay coefficient on a positive or negative offset introduced in the azimuth of LidarN for the case study on 25-10-2019 from 13:35 to 14:25.

will be different from zero, such that any attempt to fit an empirical coherence model will be associated with large uncertainties. In fact, it can be shown using synthetic turbulence generation that for a single separation distance above $\sim 10^2$ m, the coherence cannot be estimated reliably. For that reason, it was decided not to install the scanning lidars more than 100 m from each other.

To establish new coherence models that are valuable at both short separation distances (< 10 m) and large separation distances (ca. 100 m or higher), the short-range WindScanner is a more promising candidate than the long-range WindScanner system, as shown in Cheynet et al. (2016). The short-range WindScanner system allows the study of the coherence in a large number of locations in space, which reduces considerably the uncertainty associated with the large separation distances. However, such instrumentation has its own limitations, in particular, a maximum scanning distance of 150 m - 200 m.

Specific comments

***Q 2.1** p.1 l. 11 "undocumented" -> "hitherto undocumented" (but this conclusion is anyway dubious. See main issues above).*

Reply: We have used the following shorter formulation "The preliminary results document the variation of the lateral coherence with the distance from the coast".

***Q 2.2** p.2 l.30 Mann (1994) was actually a study of lateral coherence in the marine atmospheric boundary layer.*

Reply: Yes that is technically correct even though the site did not correspond to "open sea conditions". We have reformulated the paragraph as

"Linear arrays of met-masts have been used since the 1970s to study the lateral coherence above land (e.g. Pielke and Panofsky, 1970; Ropelewski et al., 1973; Perry et al., 1978; Peng et al., 2018). In the Marine Atmospheric Boundary Layer (MABL), much less information is available. In coastal sites, the lateral coherence has been studied using masts mounted on an islet (Mann, 1994) or an island (Andersen and Løvseth, 2006). However, many offshore sites are free of them and the installation cost of masts can become prohibitive if the structure must be anchored to the seabed. "

Q 2.3 *p.5 l.100: "Bosh Rexroth" -> "Bosch Rexroth"*

Reply: This typo is now corrected.

Q 2.4 *p.12 eq.1: You could clarify that x is in the direction of the mean wind vector (if it is).*

Reply: Yes x is in the direction of the mean wind vector. This has been clarified in the revised manuscript.

Q 2.5 *p.13 eq.4: The Davenport model has no theoretical foundation. This ought to be mentioned.*

Reply: We agree with the reviewer. We have added the following sentence: "Although the Davenport model has no theoretical foundation, it is widely used for its simplicity, especially for engineering applications. "

Q 2.6 *p 13 l 266: "C_y is ab" -> "C_y is an"*

Reply: This typo is now corrected.

Q 2.7 *p.13 l 277: In my opinion using "dependence" is better English than "dependency".*

Reply: We agree with the reviewer. We have replaced "dependency" with "dependence"

Q 2.8 *p.13 l.285: The phase delay has been studied by Chougule, Mann, Kelly, Sun, Lenschow and Patton (`https://doi.org/10.1080/14685248.2012.711524`) both theoretically, numerically and experimentally for vertical separations which are the most relevant. Those phases most likely do have consequences on load on wind turbines although nobody has studies this in detail.*

Reply: The manuscript deals with horizontal separations so the phase delay due to vertical separations is of limited relevancy in the present case. The paper by Chougule et al. (2012) offers an interesting discussion on the phase differences due to the vertical wind shear. However, contrary to their claim, similar studies were conducted in the 1970s and 1980s, see e.g. Bowen et al. (1983), whose results were included in the standard ESDU 85020 (ESDU, 2001). Although the finding in Chougule et al. (2012) and Bowen et al. (1983) are consistent, the latter study suggests that the phase difference is significant for lateral velocity component only, which also observed from sonic anemometers records on the Norwegian coast in 2017 (unpublished).

Q 2.9 *p.14 eq.6+7: C_x is defined from the longitudinal coherence, i.e. with purely longitudinal separation. Is there any justification of the combination of C_x and C_y for an arbitrary separation in the horizontal plane, or this just a convenient interpolation formula?*

Reply: This choice is based on empirical observations from wind tunnels as far as we know. The combination C_x and C_y as

$$d = \sqrt{(C_x d_x)^2 + (C_y d_x)^2} \tag{1}$$

is also used for synthetic turbulence generation. Therefore, it is natural to use the same approach when characterizing C_x and C_y. Figure 3 is an example for a combination of lateral and vertical separations

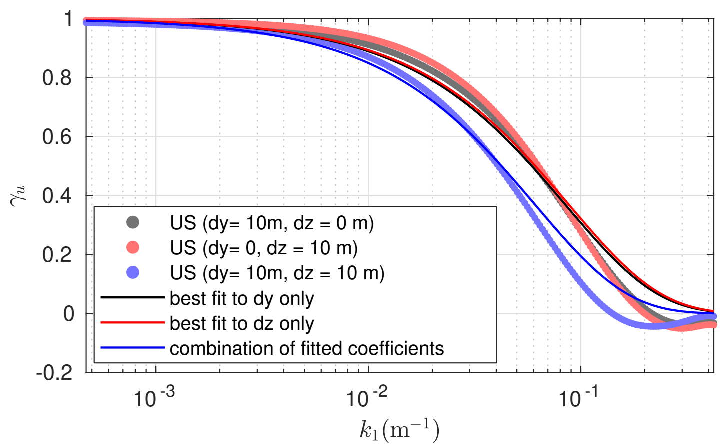

Figure 3: Co-coherence of the along-wind component computed for lateral separations d_y and vertical separations d_z usig the uniform shear (US) model (Mann, 1994) based on the Great Belt experiments (scatter plot). The red and black lines are the least-square fit of the Davenport model to the computed co-coherences for the pure lateral and vertical separations, respectively. The blue solid line is computed based on the combination of the fitted decay coefficients.

but indicates that the formulation as in eq. (1) it is appropriate to approximate the co-coherence at an arbitrary separation in a plane. to Jasna: Wasn't it a talk from Allan Larsen at BBAAVIII in Boston in 2016? Do we have additional arguments?

***Q 2.10** p.15 l. 310-323: You argue that when determining C_x the vertical separation due to slightly slanted beams (or horizontal separation due to beams slightly off mean wind direction) can be ignored. I am not so sure. Given that C_x is small (Taylor's hypothesis is not very wrong) then a lot of the apparent value of C_x could come from these vertical or horizontal separation.*

Reply: In the revised manuscript, a new method to select the range gates for the coherence is used. It is based on the lowest vertical separation distance instead of the lowest along-wind separation. This guarantees that the selected range gates are in the horizontal plane. Once the range gates are selected, the coefficients C_x and C_y are simultaneously estimated (let's call it method A). If a single Lidar is used, it is possible to study C_x as a function of the scanning distance (method B), but the influence of the elevation angle on the value of C_x will be larger than in method A. Both method A and method B have their advantages and drawbacks, but a more in-depth discussion is beyond the scope of the present study.

***Q 2.11** p.16 l. 338: Yes, that is certainly a good point.*

Reply: This is also a controversial point because such an overestimation was not predicted nor clearly observed in previous studies. The root-coherence estimates they present are bin-averaged. If bin averaging is applied separately on the real and imaginary parts of the coherence, and if they were afterwards combined to get the root-coherence, this can lead to an overestimated root-coherence at low frequencies. The strange shape of the root-coherence estimates they obtained indicates that the overestimated root-coherence they obtain may be due to the bin-averaging instead of the probe volume.

***Q 2.12** p.16 sec.3.4: Many placed you write R_{ib} where it should have been Ri_b.*

Reply: We agree with the reviewer. We have corrected this typographical mistake.

***Q 2.13** p.16 l.358: "Obukhov length"->"inverse Obukhov length"?*

Reply: We agree that “non-dimensional Obukhov length” sounds strange here. We have replaced it with “dimensionless stability parameter” as defined in the glossary of the American Meteorological Society.

Q 2.14 *p.19 fig.11: It looks like, as you mention, that the masts are in some complex flow generated by the hilly terrain. Has it been completely ruled out that the sonic on the West mast is not mounted horizontally?*

Reply: We have added the following sentence in section 2.2.4: “The masts were equipped with a spirit level to ensure that the anemometers were mounted horizontally. ”

The masts were equipped with spirit levels to ensure that the instruments were not affected by tilt angles. Figure 2 in the manuscript shows that the portion of the terrain where the masts are instrumented is sloppy and the slope becomes increasingly negative toward the west. Even though the anemometers cannot be perfectly levelled, the 1-m digital terrain model suggests that the tilt angle was largely governed by the terrain slope.

Q 2.15 *p.21 fig. 13: Just an idea: The vertical stripes are obviously not of atmospheric origin and should be removed. A procedure for that would be sec. 2.6 in Lange et al (2017)* `https://iopscience.iop.org/article/10.1088/1748-9326/aa81db/meta`

Reply: We agree that the vertical strips are not physical. The suggested solution gives encouraging results for one of the three lidars, as shown by fig. 4 but it also introduces some horizontal stripes and a non-linear trend in the velocity records. Therefore, we have adopted an alternative approach where (1) the spatial average of the wind speed is subtracted from the 2D flow field, (2) the spatial average is smoothed in the time-domain using a sliding window of 10 seconds (3) the time smoothed spatial average is added to the 2D flow field (bottom panel of fig. 4). This led to improved results, without bias and with a filtering level that can be tuned to the user’s need. We have added the following paragraph to the manuscript:

“Vertical stripes inFig. 13, possibly related to electromagnetic noise (Lange et al., 2017) were filtered out using the following procedure: first, the spatially averaged along-beam wind speed was subtracted from the 2D flow field and smoothed in the time-domain using a moving mean function with a 10-second window. The time-smoothed spatially averaged wind speed was then added to the flow field. This method provided satisfying results with minimal distortion of the data.”

Q 2.16 *p.21 fig.14 caption: “a which the” -> “at which the”.*

Reply: This is now corrected

Q 2.17 *p.22 l.446: I can see that sampling volume affects turbulence intensity, but not that the sample rate should do.*

Reply: In the present case, the sampling volume filter out the velocity fluctuations at frequencies above 0.20 Hz, so it is true that a sampling frequency of 1 Hz or 10 Hz would not change the estimated turbulence intensity. We have removed, in this sentence, the part mentioning the "limited sampling frequency used 1 (Hz)".

Q 2.18 *p.22 p.453 “range-dependant” -> “range-dependent”.*

Reply: This is now corrected

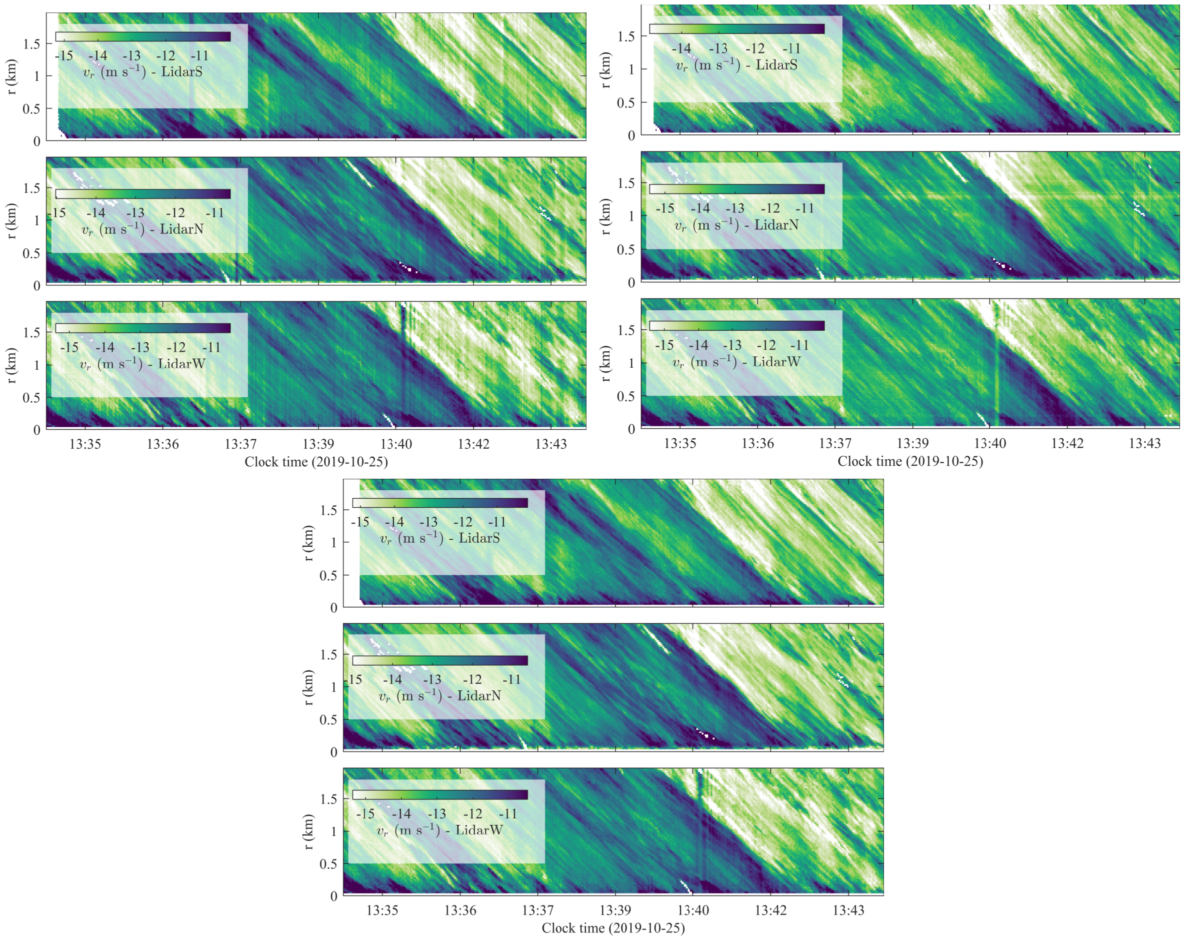

Figure 4: Top left: Without filtering out the vertical stripes. Top right: After filtering-out the vertical stripes using the method by Lange et al. (2019). Bottom: Vertical stripes filtered using the time-smoothed spatial average.

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