Answer to reviewers

Dear Sir/Madam,

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

Reviewer 1

Reviewer's summary:

This article gives an overview of a unique measurement campaign that uses cutting-edge instrumentation with the intent of answering difficult and important questions for wind engineering. The dataset seems promising and the authors seem to have put a lot of thought into instrument choice, placement, analysis, etc. I have specific comments on the attached PDF. An overview of key comments is below:

Technical – I would like to request a little more on (i) coastal internal boundary layers, including previous work/measurements where appropriate; (ii) the validity of assuming v_r ==wind speed; (iii) acknowledgment of other coherence models in addition to davenport; (iv) can these data potentially be used to improve the way we model coherence rather than rely on axisting model for coherence for their very analysis?; (v) if/how/when can any of these data be shared with other groups for collaborative research; (vi) a discussion on where we need to go in instrumentation development so you could get more and better data next time (your dataset is great in comparison to past efforts but obviously we have a long way to go); (vii) how do you anticipate modeling studies being able to complement this dataset?; (viii) at the end, please provide some big-picture take-aways of how far this dataset can take us – for technology development in the context of wind plants, offshore turbine design and operation, coastal infrastructure, etc; (ix) why wasn't the lidar data validated against the sonics at the beginning, just at the end?

Other minor – while the article is well written and relatively easy to follow, it could benefit substantially from editing by a professional communications specialist (several sentences are a bit strange or have small mistakes); clean up on nomenclature, variable definition, figure improvements, etc are noted in the pdf.

Responses to the questions

Q 1.1 *I would like to request a little more on (i) coastal internal boundary layers, including previous work/measurements where appropriate*

Reply: In the present context, coastal internal boundary layers occurs for easterly wind, i.e. when the wind is blowing from the land toward the sea. Although interesting, such wind conditions are out of the scope of the present study, which focuses exclusively on wind from the sea. Including a discussion on coastal internal boundary layers was, therefore, not judged relevant to the present study and has been omitted. Also, it should be noted that easterly wind are rarely observed near the measurement site, as shown by the wind roses in the manuscript.

Q 1.2 *the validity of assuming* v_r *==wind speed*

Reply: This is a good question. Although the present manuscript did not present direct validation against in-situ measurements or numerical models, assuming $v_r \approx \overline{u}$ is supported by the co-coherence estimates of the selected time series. The negative co-coherence fluctuations were modelled using a streamwise distance d_x, calculated based on the assumption that the along-beam component was parallel to the mean wind direction, i.e. a zero yaw angle. If a large yaw was observed, a poor agreement between the modelled and estimated co-coherence would have been observed. We wrote in the manuscript that samples with a "misalignement error" up to 20° is accepted because for a real wind turbine, similar misalignment error are expected. A systematic assessment of the assumption $v_r \approx \overline{u}$ should be conducted for the entire dataset using the new Norwegian hindcast archive (NORA3) (Solbrekke et al., 2021), which have been publicly available since may 2021 from the THREDDS Data Server of The Norwegian Meteorological Institute. In section 4.1, we have added the following sentence:

"The misalignment error between the scanning beams and the wind direction above the sea can be assessed systematically using the Norwegian hindcast archive NORA3 (Solbrekke et al., 2021), which has been openly available since 2021 with a spatial resolution 2.5 km and a temporal resolution of 1 h."

In section 4.3, we have added the following content:

"Between 13:30 and 14:20, the windcube V1 recorded a mean wind direction of 231° at 100 above the ground, such that the mean wind direction above the wind profiler increased by only 6° in 50 min. During the same period, the NORA3 hindcast provided a wind direction of 237° at 100 m above the sea surface, 3 km west of the lighthouse. The small difference supports the idea that, for the case at hand, the mean wind direction did not significantly change as the flow moved toward the coast.

For hourly wind records in 2019 and 2020 with $\overline{u} \geq 5\,\mathrm{m\,s^{-1}}$ at 10 m above ground near LidarN, the interquartile range of the wind direction difference between the NORA3 hindcast and the data collected on the mast operated by the Norwegian Meteorological Institute was only 12°. Therefore, it was concluded that during the COTUR campaign, the NORA3 hindcast could provide a reliable estimate of the hourly mean wind direction, especially under strong wind conditions where the error was significantly reduced."

Q 1.3 *acknowledgment of other coherence models in addition to davenport;*

Reply: We have added the following sentences to section 3.3, after the introduction of the Davenport model:

"Note that there exist alternative coherence models based on the spectral tensor of homogeneous turbulence (e.g. Kristensen et al., 1989; Mann, 1994), but these cannot easily assessed using long-range scanning lidar instruments measuring the along-wind component only. Therefore, these models are not discussed herein. "

There exists mainly two types of coherence models: those based on a spectral tensor of turbulence (Kristensen et al., 1989; Mann, 1994) and those based on an empirically fitted exponential decay, such as the Davenport model. The description of the coherence of turbulence with a spectral tensor requires an estimation of the one-point velocity spectra and cross-spectra for the different velocity components. Therefore, it cannot be achieved with the scanning lidar instruments deployed during the COTUR campaign. Empirical coherence models are usually derived from the Davenport model and have been discussed in the past (e.g. Jakobsen, 1997; Cheynet et al., 2017; Cheynet, 2018; Cheynet et al., 2016a, 2018). Since the manuscript does not focus on proposing new coherence models nor discussing which one is best, we feel that a more detailed review of coherence models is not relevant for this particular paper.

Q 1.4 *(iv) Can these data potentially be used to improve the way we model coherence rather than rely on existing model for coherence for their very analysis?*

Reply: The measurements conducted during the COTUR campaign allows the estimation of the coherence at heights above the sea surface not reached before. Therefore, it could be possible to propose new coherence models for wind turbine design. However, little is known about the abilities of long-range Doppler wind lidar to measure reliably and accurately the coherence of turbulence. To improve the way we model the coherence, using traditional arrays of masts or tall masts may still be a wiser choice at the moment. The situation may change once there are a sufficient number of studies demonstrating the capabilities of long-range scanning lidars to estimate the coherence.

Q 1.5 *(v) If/how/when can any of these data be shared with other groups for collaborative research;*

Reply: According to the collective agreement between the institutions involved in the measurement campaign, the data can be accessed immediately in collaboration with at least one of the participating institutions. The data set will be made available for "stand alone" usage after a quarantine period of 3 years, earliest from May 2023.

Q 1.6 *A discussion on where we need to go in instrumentation development so you could get more and better data next time (your dataset is great in comparison to past efforts but obviously we have a long way to go);*

Reply: In the conclusion, we have added a point on the need to improve the pointing accuracy of Lidar instruments to study the coherence of turbulence at scanning distance of several kilometers, which reads as:

"The decay coefficients used to model the co-coherence displayed a dependence on the scanning distance, which is partly attributed to the limited pointing accuracy of the long-range WindScanner system. As pointed out by Vasiljević et al. (2016), achieving an averaged pointing error as low as 0.01° may be achievable in a near future and could become necessary to study the lateral co-coherence of turbulence at scanning distance beyond 2 km. "

When it comes to studying turbulence with scanning lidar instruments, a significant limiting factor is currently the software, as highlighted in the conclusions. Future lidar based studies of turbulence will also benefit from the ongoing work on reducing the measurement volumes. The hardware limits us to LOS measurements with $f = 0.5$ Hz mainly due to computational processing speed. If the measurement resolution and the measurement frequency is to be increased, we would need more computational power (better processor, more processor threads, etc.).

To increase the signal to noise ratio, larger mirrors can be used to collect the backscattered light, as done with the upgraded instrumentation of the short-range WindScanner system. However, the paper focus on scanning strategies relying on the combination of commercially available lidar instruments and the central role of lidar softwares to ensure their synchronization. Although interesting, discussing the machinery and mechanical elements of these lidars seems out of the scope of the manuscript.

Q 1.7 *how do you anticipate modeling studies being able to complement this dataset?*

Reply: The new high-resolution Norwegian hindcast archive (NORA3) has been available publicly since May 2021 and can be used to assess the mean wind speed and mean wind direction at a distance of 3 km away from the Norwegian coast. Such data are available with a hourly temporal resolution, a 2.5 km horizontal spatial resolution, and multiple heights above the surface. Such models could be used to identify situations where the scanning beams are not well aligned with the wind direction. We have modified Fig. 15 to include the mean wind speed profile from the NORA3 hindcast and provide a more complete comparison with the different instruments. The present data can also be used for validations of the numerical flow simulations in the micrometeorological context.

Q 1.8 *At the end, please provide some big-picture take-aways of how far this dataset can take us – for* *technology development in the context of wind plants, offshore turbine design and operation, coastal infrastructure, etc;*

Reply: We have added the following lines to the conclusion:

" Remote sensing measurement of atmospheric flow above the ocean from sensors located on the seaside can be valuable to the design of the next generation of wind turbines. However, these are also deployed at increasing distances from the coast. Therefore, a detailed study of the influence of the coastline on the measured wind turbulent characteristics is required to know whether these can be directly applied to model far offshore wind conditions."

The measurement campaign demonstrates a novel measurement methodology for remote sensing of the offshore wind conditions including the along-wind turbulence. It is both the methodology and the data set that are relevant to design of wind sensitive structures such as wind turbines, long-span bridges and coastal structures. Specific implications of the observed wind conditions for different types of structures is out of the scope of the present paper.

The lessons learned from the measurement campaign using the WindScanner software and lidar-Planner softwares and their further development are already addressed in the manuscript. For technology development with respect to wind plants and turbine design and operation, this dataset needs to be analyzed in more detail to provide a specific answer to the reviewers question. Generally, the purpose of the measurement campaign was to estimate the coherence of turbulence by using remote sensing devices. This quantity is needed to estimate the extreme wind loading and fatigue loading on offshore wind turbine. The current manuscript intends to provide an overview of the measurement campaign and the presentation of a more detailed data analysis and its scientific results is beyond the scope of this manuscript and the AMT journal. We envision that results of a more thorough data analysis that gives a precise answer to the application of remote sensing for coherence and turbulence estimation will be documented in further studies.

Q 1.9 *Why wasn't the lidar data validated against the sonics at the beginning, just at the end?*

Reply: The resources required for the sonic anemometer measurements were made available towards the end of the measurement campaign. In particular, the telescopic masts, owned by the University of Stavanger, were not available for field deployment in 2019. Also, the sonic anemometer mast were positioned on a private property and we only received permission from the owner to place equipment on his property in 2020.

Specific comments

Q 1.10 *Line 77: I can't quite understand what is meant – do you mean the coastline effects might at times be noticed up to several hundred meters off shore? what is the closest to the coast that the lidar can see?*

Reply: We have reformulated the sentence as "The influence of the coastline on low-frequency velocity fluctuations, i.e. a time-scale from one to ten minutes, may be noticeable up to several hundred meters away from the shore (Emeis et al., 1995). Therefore, the use of long-range scanning Doppler wind lidar instruments is justified to study the flow conditions up to 2 km away from the seaside."

The aim of the campaign is to study offshore wind conditions from the coast. But we are not truly measuring offshore wind if low-frequency turbulent fluctuations are modulated by the presence of the coastline, which slows down the flow at a mesoscale level. So the sentence indicates that measuring the wind from the ocean several hundreds of meters away from the seaside will not guarantee that the flow characteristics will be truly representative of offshore conditions.

Q 1.11 *Line 77: the escarpment in Obrestad or the one in Bolund?*

Reply: We were referring to the escarpment of both locations. We have reformulated the sentence as “Results from the Bolund hill experiments suggest that the escarpment at Obrestad lighthouse might affect the local flow characteristics up to 50 m above the instruments.”

Q 1.12 *there are ways to reword some of the sentences that make them easier to follow. Here i would say "the wind direction distribution during the experimental campaign (march 2019 - march 2020) is consistent with the climatological records (1990-2020)"*

Reply: The suggestion of the reviewer is accepted

Q 1.13 *how much more? are climatological changes being seen in the winds? perhaps you could just say "this includes winds in the 180-270 sector, which are favorable for the COTUR experiment and happened x% more often during the measurement period than suggested by the 30-year reference mean"*

Reply: We have reformulated the sentence as “This includes winds in the 180°-270° sector, which are favourable for the COTUR experiment. These flow conditions happened 20% of the time between March 2019 and march 2020 against 15% for the 30-year reference median value.”

Note that this larger number was not found to be representative of a climatological change because no trend was found in the data.

Q 1.14 *Line 132-133: higher than what?*

Reply: The sentence has been reformulated as “The profiles of the atmospheric temperature and humidity were retrieved up to 10 km with non-uniform vertical spacing. In the first 1200 m above the surface, the vertical measurement resolution ranged from 25 m to 40 m whereas above 1200 m, it ranged from 50 m to 300 m.”

Q 1.15 *Line 223: why 20? do you recommend this value for future applications to lidar measurements?*

Reply: As for most outliers detection techniques, such a parameter is manually tuned based on the number of points that are discarded. The study does not focus on presenting a new CNR filtering technique for Doppler wind lidar data. Therefore, it would not be wise to recommend a specific value for the Mahalanobis distance based on the specific lidar configuration used during the COTUR campaign.

Q 1.16 *Line 224: how was this picked?*

Reply: The values recommended for CNR filtering with the WindCube 100S are generally empirically determined. For scanning lidar instruments, the lower CNR threshold value is usually between -27 dB and -28 dB. In the present case, we attempt to rescue data points associated with a CNR under -28 dB. The value of -35 dB is chosen to fix a lower limit of the data rescuing algorithm since it is clear data recorded with a CNR of -35 dB or lower are mainly noise. For this reason, we do not need to justify this choice of a new threshold value.

Q 1.17 *Line 227: i always find it tricky to pick this value– did you test different window sizes? is 200 s* *related to some physical reason– ie a length scale you hope to capture? won't 200 s wash out too much of the turbulence?*

Reply: The outlier detection algorithm used is not a filtering algorithm aiming to remove non-linear trend. In fact, non-linear trends were not removed in the present study as they often reflect low-frequency turbulent fluctuations, which are valuable for wind load modelling on offshore structures. Selecting an appropriate set of parameters for outlier detection algorithms relies on trial and errors. Fortunately, there is several possible good choices for the window length. We have tested window lengths from 60 s to 600 s. Since the sampling frequency is 1 Hz, 200 s corresponds to 200 data points and slightly more than 3 min. A window length between 180 s and 300 s gives satisfying results for terrain with low roughness. The outlier detection algorithm with a moving median filter needs to use enough data point such that the random error in the median estimate is small. This avoids turbulent fluctuations being detected as outliers. Conversely, too many data points will result in outliers that are not detected properly.

Q 1.18 *Line 235-236: This paragraph gets a bit confusing as it progresses and needs to be revised. 20% for mean/median, 50% for std dev, and how does all of this relate to coherence?*

Reply: The values of 20% and 50% are related to the stationarity tests. The coherence relies on the estimation of the two-point cross-spectral densities and one-point power-spectral densities of a random process. These are not defined is the random process is not stationary. The paragraph one lines 230-237 simply introduce the parametric stationarity test applied and the thresholds values selected.

Q 1.19 *in other words 65% of the data collected were useful for coherence analysis?*

Reply: This is not what we meant. Less than 65% of the data collected were used for the study of coherence because non-stationary fluctuations are not the only reason for a lower data availability. For example, if the lidar beams and the mean wind direction locally measured differed by more than 20°, the lidar data were dismissed. Therefore, the value of 35% of data that did not pass the stationary test indicates simply that the stationary test is not too conservative.

Q 1.20 *Line 246-247: a little more explanation here will be helpful. explain how magnitude and phase relate to these physical quantities you are trying to explain. if I understand correctly a phase difference seen in ρ_u means that a certain eddy reaches one point before it reaches another point so it suggests an elongated (ie sheared?) structure? anyway i think these two sentences where the math is linked to the physical world warrant their own paragraph.*

Reply: We agree with the reviewer that these paragraphs were quite obscure. We have reformulated the first half of section 3.3 to better introduce the concept of root-coherence, co-coherence and quad-coherence.

Q 1.21 *Line 272-273: not sure what is meant here. that the prediction was wrong and site-specific constants can be better for harsher sites? are you suggesting we are under-designing our structures for harsh conditions?*

Reply: We have reformulated the sentence to avoid any further misunderstanding as "Improved decay coefficient estimates could lead either to substantially reduced construction costs or more robust design."

There is a lack of detailed full-scale analysis of wind turbine response to turbulent load associated with the monitored incoming wind field. Therefore, one cannot conclude whether the coherence models used in standards and codes lead to an over- or under-design of wind turbines.

Q 1.22 *Line 281: this paragraph appears unlinked to the previous; it needs an introductory sentence that links them. e.g. "connecting lateral and vertical coherence is challenging in field measurements".*

Reply: We agree with the reviewer. The paragraph was modified within the large reformulation of section 3.3.

Q 1.23 *Line 295: this needs more words or some rewording; e.g. the coherence loss as eddies decay along their propagation direction?*

Reply: We have reformulated the sentence as "Taylor's hypothesis can be relaxed using an additional decay coefficient $C_x \neq 0$, reflecting the time-varying characteristics of eddies as they are advected in the along-wind direction."

Note that the term "eddy decay" has a specific and different meaning in fluid mechanic so it cannot be used here.

Q 1.24 *Line 301: along which direction? axial? & along which directions? lateral and vertical?*

Reply: Gust are three dimensional so the limited size we referred to is is for every direction. For the lateral coherence, the spatial dimension of eddies along the lateral direction is of interest. We have reformulated the sentence as

"For a given turbulence length scale in the lateral direction L_y, the Davenport model is usually valid if $d_y/L_y \ll 1$, which is no longer the case at large crosswind separations (Irwin, 1979; Kristensen and Jensen, 1979). To account for the limited size of the eddies in the lateral direction, additional decay coefficients could be introduced, but, in the present case, these were found small enough to be neglected."

Q 1.25 *Line 312: what is the full-scale estimate?*

Reply: The term "full-scale estimate" refers to natural wind, which contrasts with wind tunnel test or simulated data. We have removed the term "full-scale" in the revised manuscript.

Q 1.26 *Line 320: closest three-dimensionally? or two-dimensionally as the sketch shows (ie dx and dy only are considered)?*

Reply: In the revised manuscript, we use the lowest vertical separation so that the selected range gates are in the same horizontal plane. The newly selected range gates are usually associated with $d_z < 1$ m. Therefore, the influence of d_z on the estimated decay coefficients is minimized.

Q 1.27 *Line 324: needs rewording eg the co-coherence estimation is highly uncertain if...? otherwise sounds like you are estimating the uncertainty too but i don't think that's what you mean?*

Reply: The term "uncertainties" is indeed chosen to reflect the statistical uncertainties associated with the estimation of the co-coherence. Note that the paper does not focus on studying these statistical uncertainties as it is a topic of its own.

Q 1.28 *Line 325: where did these numbers come from? are you expecting a certain eddy size in this offshore footprint that is being scanned? or is this related to the range gate size?*

Reply: These numbers comes from the experience of the authors with the study of the coherence both from measurements of natural wind and synthetic turbulence generation. Using simulated correlated wind histories, it can be shown that using increasingly large crosswind distances leads to poorer co-coherence estimated because the number of data points significantly different from zero becomes small.

Q 1.29 *Line 325: "what is record duration"*

Reply: Record duration means averaging time, which is fixed by the instrument configuration. For example, a record duration of 50 min means that the instruments measure the wind for 50 min. We have replaced the term "record duration" with "averaging time" on line 325.

Q 1.30 *i thought you were already measuring the flow in three positions here with the three scanning lidars? this sentence is confusing me*

Reply: We have reformulated the sentence as "Another alternative is to increase the spatial resolution by simultaneously measuring the flow in a large number of locations."

Q 1.31 *Line 328: as opposed to one that is not appropriate? confusing*

Reply: Yes this is what we mean. An inappropriate PSD estimate would be the periodogram method with a rectangular window, for example.

Q 1.32 *so when you said "appropriate" you meant that the segment length (window size?) and overlap amount need to be tested to make sure they are not introducing uncertainty, and here you found that they are not? so the welch method does not add uncertainty? this paragraph needs a bit more "coherence"*

Reply: The number of overlapping windows aims to reduce the uncertainties. Uncertainties are largest when a single window is used. However, using too many windows reduces the frequency resolution so using too short segments is not desirable either. Welch's algorithm has been widely used in atmospheric turbulence analysis for the past 50 years so we should keep the description of its parameters as concise as possible. The manuscript is about a new instrumental setup to remotely measure turbulence, not a discussion on parametric methods to study the power spectral density of stationary random processes.

Q 1.33 *at this point i forgot what we were fitting and had to remind myself – perhaps add that here eg. the fitting of empirical values to an analytical coherence function (i.e. the davenport model) is highly sensitive...*

Reply: We have reformulated the beginning of line 340 as "In summary, the estimation of the Davenport decay coefficient is [...]", which is now in section 3.4.

Q 1.34 *Line 349 & equation 8: say here that you computed Rib first and then used that to get to obukhov length, so the reader knows where you're going + introduce the symbol in the text before or after presenting it.*

Reply: We have reformulated the sentence on lines 349-350 as "In the present study, the bulk Richardson number Ri_b was used to calculate the dimensionless stability parameter $\zeta = zL^{-1}$ where L is the Obukhov length. The sea-surface temperature, mean wind speed measurements from the scanning wind lidars and temperature profile data collected by the HATPRO radiometer were used to estimate Ri_b."

Q 1.35 *it is a minor detail but it would be easier to interpret if you had negatives being blue and positives being red, or just a different colormap altogether. without reading my first interpreation was "whoa this offshore environment is unstable year round???" but then I realized the hot/cool shift doesn't coincide with the zero mark*

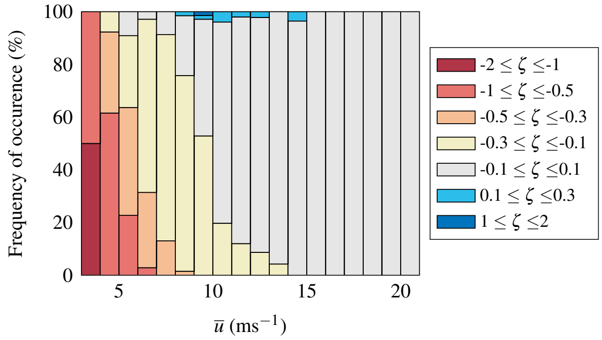

Figure 1: Histogram of the non-dimensional Obukhov length ζ using 50 min long records from February 2019 to march 2020, computed using the scanning lidars and the HATPRO radiometer.

Reply: Figure 9 has been redrawn using fewer bins (some of them were empty and, therefore, useless). The near-neutral conditions have been represented by grey colour to improve the clarity of the picture. The new figure reads as

---

Q 1.36 *Line 361: (not shown) ?*

Reply: There is not yet climatology data for the atmospheric stability on the west coast of Norway. Fortunately, such data are not necessary to know that the distribution of atmospheric stabilities shown in Fig 9 is not representative of the site climatology. The main reason is that the lidars configuration is dedicated to monitoring mainly the wind from the sea. Therefore, the data availability for easterly wind is much lower, which partly explain the low number of samples associated with a stable stratification. Also, the signal-to-noise ratio of scanning Doppler wind lidars is usually low for weak winds, which prevent systematic studies of moderately and strongly diabatic wind conditions.

Q 1.37 *Line 366: greater than? also without the vertical bars right?*

Reply: Yes, this was a typographical mistake. It should have been $\zeta > 0.1$. It is now corrected in the revised manuscript.

Q 1.38 *Line 372: what is usable? after quality control of inflow and the cnr filtering etc?*

Reply: The sentence has been removed as it was not necessary to the paragraph.

Q 1.39 *Line 375: is this primarily due to technical issues with the instruments, power, etc? or is it due to atmospheric inflow (eg wind direction being from land?) or environmental conditions (eg not enough aerosols?)*

Reply: This is the (rather) normal data availability for long-range scanning lidar instruments in scientific campaigns. They are due to both the hardware, software and environmental conditions. Note that one should not compare such data availability with the one from a Doppler wind profiler, which is usually higher than for scanning instruments.

Q 1.40 *Line 376: why not add this to table as well?*

Reply: Because table 1 shows the data availability based on the alignment between the scanning beams and the wind direction. This processing step does not apply to the radiometer or the Doppler wind profiler.

Q 1.41 *Figure 11: several people cannot tell green and red apart, so i wouldn't have these two colors on the same plot*

Reply: We confirm that people with Protanopia or Deuteranopia will be able to distinguish these two curves because the colours chosen are associated with significantly different brightness. Therefore, the green used in Fig 11 will appear as bright yellow and the red as dark brown.

Q 1.42 *Line 384-385: please provide some more context for this section, i got confused as you started by saying what you will do in the future but then started giving results. a sentence along these lines would help. "here, we provide an assessment of the suitability of the sonic and lidar measurements to provide co-coherence estimates and of the sonic to be used to validate the lidar estimates....."*

Reply: We have reformulated the first two sentences as

"This section provides an overview of the sonic anemometer data in terms of mean wind speed, mean wind direction and angle of attack (AoA). The AoA is defined here as the angle between the wind vector and the horizontal. A further study will use these sonic anemometer records to assess whether the lateral coherence of turbulence is captured properly by the long-range lidar instruments"

Q 1.43 *Line 390: can AoA be defined further (with respect to what coordinate system?) before its results are discussed?*

Reply: We have added the definition of AoA in the reformulated first paragraph of section 4.2.

Q 1.44 *Line 396: what portion of the wind speed comparison shows this? i don't get it*

Reply: The larger fluctuations in the lidar measurements indicate that the flow is spatially non-uniform around the mast. We have reformulated the sentence as "On the top panel off fig. 11, the wind velocity fluctuations measured with the lidar instruments are larger than by the sonic anemometers. This indicates that the flow is unlikely to be spatially uniform around the masts for a southern flow." Lines 396-398 give some reasons for the larger-than-expected lidar velocity fluctuations.

Q 1.45 *Fig 13: I assume the clock times are synchorinized so the x axis label can just be clock time and say whether local time or utc? then the lidar name can be a title to each subfigure*

Reply: Yes the clock time of the three lidars is based on the same reference clock time, although they do not start exactly at the same time. We have redrawn the figure with optimized spacing.

Q 1.46 *Line 428: what is a slant profile?*

Reply: The term slant profile was taken from the glossary of radar remote sensing. We have written a new paragraph to define what we call slant profile:

"A slant profile is defined herein as a profile of the mean value or standard deviation of the along-beam component using scanning beams with a non-zero elevation angle. Therefore, the measurement volumes at increasing heights are obtained at increasing scanning distances. In an idealized homogeneous terrain, the slant profile would be identical to a traditional vertical profile. On the seaside, the measurement volumes closer to the ground will be more affected by the local topography than further away from the shore."

Q 1.47 *Line 428: fig 15 says v_r so it's radial velocity not horizontal wind speed?*

Reply: Yes but the scanning beams are orientated toward the mean wind direction so $\overline{v_r}$ is approximatively equal to the mean wind speed. For the sake of clarity with have replaced the term "mean wind speed" with "along-beam mean wind speed". Note that we should not refer v_r as radial velocity for a LOS scan as there is no rotational motion of the scanner head.

Q 1.48 *Line 430: ambiguous, could be interpreted as the data from the scanning instruments is displayed as a solid line*

Reply: We have reformulated the sentence as "The mean wind speed profile calculated using the WindCube v1 is shown as a solid line and superposed with the slant profiles from the scanning instruments."

Q 1.49 *Line 432: What's the difference between this term and a coastal internal boundary layer?*

Reply: Both an internal boundary layer and an induction zone modifies the wind flow characteristics but are not related to the same physical process. An internal boundary layer is created downstream of a change of roughness. In the case of the COTUR campaign, internal boundary layers would be observed for easterly winds since the scanning beams would measure the flow downstream of a transition from high-roughness (land) to low-roughness (sea). However, such measurements were not the focus of the campaign and were seldom recorded. The term "induction zone" is taken from the field of wind energy. It reflects the blocking of the flow by the land and can occur without roughness change. For example, the coastal induction zone is responsible for the reduction of the mean wind speed close to the coastline.

Q 1.50 *Line 440: how do you know? because of the zig zag?*

Reply: As highlighted out in the manuscript, the vertical profile of σ_{v_r} changes by less than $0.05\,\mathrm{m\,s^{-1}}$ at heights between 100 m and 200 m above the surface. This is a strong indication that σ_{v_r} is nearly constant with the height. The fluctuations of σ_{v_r} are likely below the measurement uncertainties of a scanning lidar instrument. In fact, the fluctuations may be close to the random error associated with the estimation of the standard deviation of horizontal turbulence under neutral conditions with an averaging time of 50 minutes.

Q 1.51 *Line 445:-450 i am not sure what the objective is in comparing a single 50-minute record with long-term mean values expected offshore. a single value could be an extreme, could be spot on the mean, could be anything really*

Reply: The comparison is based on the scientific literature documenting, for the last six decades that atmospheric micro-scale turbulence can be modelled fairly well by random process theory. A stationary random process is not a chaotic system. The data processing, including outlier removal, stationary test and assessment of the atmospheric stability allows us to reduce measurement biases. Because we have selected a case associated with stationary fluctuations, a random atmospheric stratification and a wind from the ocean without roughness change, a turbulence intensity between 6% and 10% at 100 m amsl was expected. Finally, it should be noted that for the single event selected, we have not studied one single time series but one time series per range gates for the three lidars instruments, which gives a total of 234 time series in different locations above the sea.

Q 1.52 *Line 453: and the Davenport coherence model?*

Reply: The decay coefficients C_x and C_y mentioned on line 453 are those from the Davenport model, as described in section 2.

Q 1.53 *Line 454: explain a little more please. larger co-coherence therefore larger eddies which are* *associated with more convective conditions?*

Reply: We have reformulated the sentence as "Lower decay coefficients imply a larger co-coherence, therefore, larger eddies and an increased turbulent wind load on structures".

Atmospheric turbulence includes a large range of different eddies, independently from the thermal stratification. Although the atmospheric stability affects the coherence, the latter depends on many more parameters (cf. section 3.3). In summary, larger eddies do not necessarily implies convective conditions.

Q 1.54 *Line 458: link back to the sonic/lidar comparison you made earlier and make it clear that it is possible to do that with cotur data*

Reply: We have reformulated the sentence as

"The ability of long-range lidars to describe properly the co-coherence of turbulence relies on a rigorous comparison with data from sonic anemometers on met-masts. As highlighted by section 4.2, the instrumental setup of the COTUR campaign allows such a validation study."

Q 1.55 *Figure 15:did you try plotting the top three panels in 3d axes that would accurately show the spatial separation between the measurements made by the scanning and profiling lidar? then the wind speed could be shown in colors. i think the way it's plotted it's a bit misleading but i see it's hard to show these "slant" profiles. since this is the first cotur paper and is meant to demonstrate what type of data is available (ie it is not focused on results) perhaps some effort in displaying these differences across instruments is worth it*

Reply: We found that the 2D representation in Fig 15 was a good trade-off between traditional 2D wind speed visualization and 3D slant profile visualization. Using 3D axes should be avoided whenever possible in academic papers as they lack clarity. In the suggested 3D plot, the superposition of the wind profile from the WindCube V1 with the slant profiles would be confusing. In the 2D representation of Fig 15, the wind profile from the WindCube V1 is the same in the different subplots, which allows a direct comparison between the different profiles.

Q 1.56 *Line 462: can you further explain what the negative values mean and why they are explained by dx=55 m?*

Reply: Section 3.3 has been reformulated to partly answer this question. The phase difference between the two velocity records increases with the longitudinal distance. In Fig 17, A longitudinal distance of 55 m and a mean wind speed of 15 m/s correspond to a time delay of 4 sec. This time lag is reflected in the 2-point power spectral density estimates by a complex exponential. The real part of the root coherence coh_u is the co-coherence γ_u. If $d_x \neq 0$, the time-delay is reflected by a cosine term, such that

$$\gamma_u(d_x, d_y, f) \approx \mathrm{coh}_u(d_x, d_y, f)\cos(\phi_x) \quad (1)$$

where $\phi_x \approx \frac{2\pi f d_x}{\overline{u}}$. For a frequency of 0.1 Hz and $d_x = 55$ m, $\cos(\phi_x) = -0.7$, which explain the large negative value for the bottom panel of Fig 17. If $d_x = 5m$, $\cos(\phi_x) = 0.98$ and the phase delay is not visible, as shown by the co-coherence estimates in the middle and top panels of Fig. 17.

Q 1.57 *but if it's a convective, mixing layer then w is not negligible instantaneously? can you comment on how good of an assumption this is? throughout the manuscript you say "wind speed" where v_r is used and i can't recall any numerical justification for it (based on literature or measurements?)*

Reply: We have added some new sentences that read as

"It should be noted that the assumption $\overline{v_r} \approx \overline{u}$ can be challenged if the misalignment between the scanning beam and the mean wind direction is large. Nevertheless, the influence of the vertical mean wind speed $\overline{w}$ on $\overline{v_r}$ is likely negligible as the elevation angle is small (Cheynet et al., 2016b) but also because the study does not focus on weak wind conditions ($\overline{u} < 5\,\mathrm{m\,s^{-1}}$) which are of limited relevance for wind energy application."

The beams are almost horizontal, therefore, even if w had a strong mean value, its contribution to v_r would be negligible as the along-beam component is the projections of the three velocity components onto the scanning beam. Therefore, the assumption that $v_r \approx \overline{u}$ does not rely on the assumption that $\overline{w} \approx 0$ but on the assumption that the scanning beam is fairly well aligned with the mean wind speed. For a non-horizontal scanning beam with an elevation angle of 5° and an unusually large value $\overline{w} = 1\,\mathrm{m\,s^{-1}}$, the contribution of $\overline{w}$ to $\overline{v_r}$ would be $1 \times \sin(180/\pi \cdot 5) = 0.09\,\mathrm{m\,s^{-1}}$, which is negligible compared to the uncertainties regarding the misalignment error between the beams and the instantaneous wind direction. The only exception is if weak wind conditions are considered, but these are not the focus of the present study.

It should be noted that a convective atmosphere covers a wide range of wind conditions and that a near-neutral atmosphere on the unstable side, as observed in the present study, differ from free convective conditions. For a sonic anemometer, the mean value of w would impact significantly the mean wind speed for free convection only, which was not the case in the dataset selected. Some quantifications of the error due to misalignment between the scanning beams and the wind direction or the horizontal plane can be found in Cheynet et al. (2016b, Fig. 3).

Q 1.58 *Line 515: what is natural wind?*

Reply: Natural wind is the wind naturally generated in the atmosphere, by opposition to wind artificially generated in wind tunnels. It is a term commonly used when studying atmospheric flow characteristics, see e.g. Kristensen and Jensen (1979); Cermak (1981); Petersen et al. (1998) or Toriumi et al. (2000) among others.

Other comments

- Line 124: We have replaced WLS100-37 with LidarN. WindCube can refer either to the profiling instrument (WindCube V1) or the scanning instruments (WindCube 100S), which were named LidarN, LidarS and LidarW. Their original name was WLS100-37, WLS100-34 and WLS100-40, respectively. They were renamed in the manuscript for the sake of simplicity. The original draft included erroneously some of the original names. The revised manuscript has now replaced them with the simplified names.
- Line 148: The names WLS34, WLS37 and WLS40 have been replaced with LidarS, LidarN and LidarW in the revised manuscript.
- Line 207: The name WLS100-37 has been replaced with LidarN in the revised manuscript.
- Lines 247-257 were removed as they were not found necessary to the manuscript.
- Line 404: the UTC time is used (we have added this information in the revised manuscript).
- We have proofread the resubmitted manuscript to ensure grammatically concise and proper use of English. One of the co-authors is a native speaker and she could confirm that the English is adequate and correct.

References

Cermak, J. E. (1981). Wind tunnel design for physical modeling of atmospheric boundary layer. *Journal of the Engineering Mechanics Division*, 107(3):623–642.

Cheynet, E. (2018). Influence of the measurement height on the vertical coherence of natural wind. In *Conference of the Italian Association for Wind Engineering*, pages 207–221. Springer.

Cheynet, E., Jakobsen, J. B., and Reuder, J. (2018). Velocity Spectra and Coherence Estimates in the Marine Atmospheric Boundary Layer. *Boundary-Layer Meteorology*, 169(3):429–460.

Cheynet, E., Jakobsen, J. B., Snæbjörnsson, J., Mann, J., Courtney, M., Lea, G., and Svardal, B. (2017). Measurements of surface-layer turbulence in a wide norwegian fjord using synchronized long-range Doppler wind LiDARs. *Remote Sensing*, 9(10):977.

Cheynet, E., Jakobsen, J. B., Snæbjörnsson, J., Mikkelsen, T., Sjöholm, M., Mann, J., Hansen, P., Angelou, N., and Svardal, B. (2016a). Application of short-range dual-Doppler lidars to evaluate the coherence of turbulence. *Experiments in Fluids*, 57(12):184.

Cheynet, E., Jakobsen, J. B., Svardal, B., Reuder, J., and Kumer, V. (2016b). Wind Coherence Measurement by a Single Pulsed Doppler Wind Lidar. *Energy Procedia*, 94(1876):462–477.

Emeis, S., Frank, H. P., and Fiedler, F. (1995). Modification of air flow over an escarpment – results from the Hjardemål experiment. *Boundary-layer meteorology*, 74(1-2):131–161.

Irwin, H. (1979). Cross-spectra of turbulence velocities in isotropic turbulence. *Boundary-Layer Meteorology*, 16(3):237–243.

Jakobsen, J. B. (1997). Span-wise structure of lift and overturning moment on a motionless bridge girder. *Journal of wind engineering and industrial aerodynamics*, 69:795–805.

Kristensen, L. and Jensen, N. (1979). Lateral coherence in isotropic turbulence and in the natural wind. *Boundary-Layer Meteorology*, 17(3):353–373.

Kristensen, L., Lenschow, D., Kirkegaard, P., and Courtney, M. (1989). The spectral velocity tensor for homogeneous boundary-layer turbulence. In *Boundary Layer Studies and Applications*, pages 149–193. Springer.

Mann, J. (1994). The spatial structure of neutral atmospheric surface-layer turbulence. *Journal of fluid mechanics*, 273:141–168.

Petersen, E. L., Mortensen, N. G., Landberg, L., Højstrup, J., and Frank, H. P. (1998). Wind power meteorology. part i: Climate and turbulence. *Wind Energy: An International Journal for Progress and Applications in Wind Power Conversion Technology*, 1(S1):25–45.

Solbrekke, I. M., Sorteberg, A., and Haakenstad, H. (2021). Norwegian hindcast archive (nora3) – a validation of offshore wind resources in the north sea and norwegian sea. *Wind Energy Science Discussions*, 2021:1–31.

Toriumi, R., Katsuchi, H., and Furuya, N. (2000). A study on spatial correlation of natural wind. *Journal of Wind Engineering and Industrial Aerodynamics*, 87(2-3):203–216.

Vasiljević, N., Lea, G., Courtney, M., Cariou, J.-P., Mann, J., and Mikkelsen, T. (2016). Long-range WindScanner system. *Remote Sensing*, 8(11):896.

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.

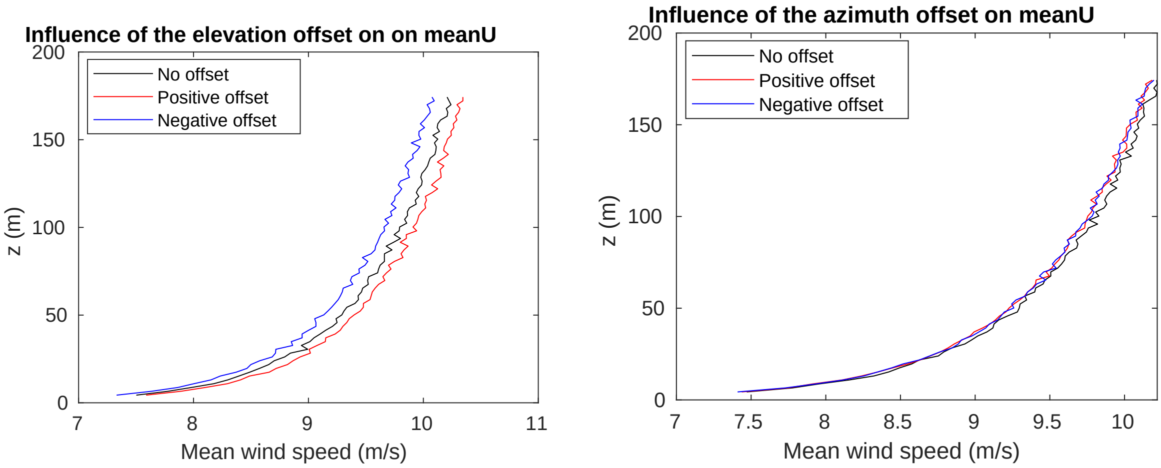

Figure 1: Influence of an elevation offset of 1° (left) and an azimuth offset of 6° (right) of a noisy idealized logarithmic mean wind speed profile.

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

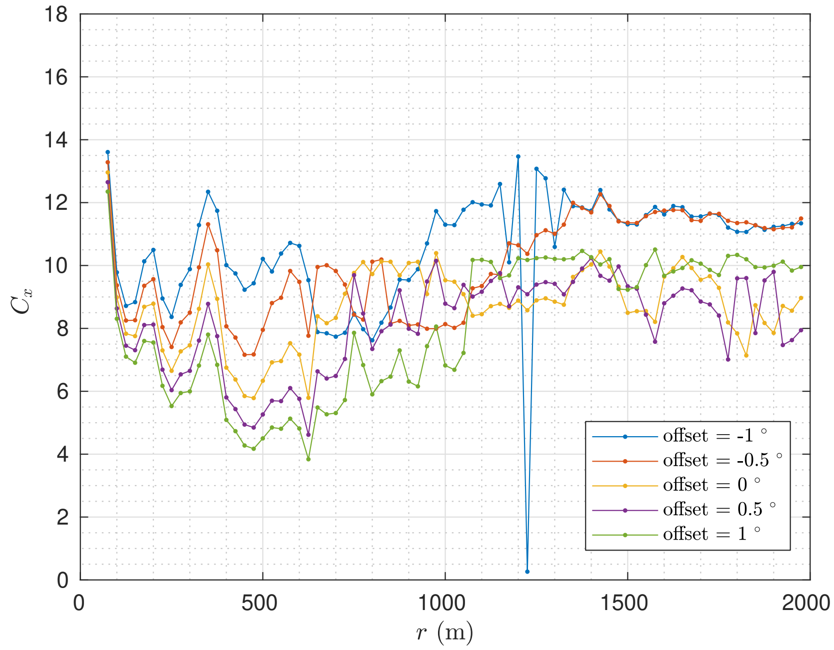

Figure 2: Sensitivity of the lateral decay coefficient on a positive or negative offset introduced in the azimuth of LidarN for the case study on 25-10-2019 from 13:35 to 14:25.

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

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

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

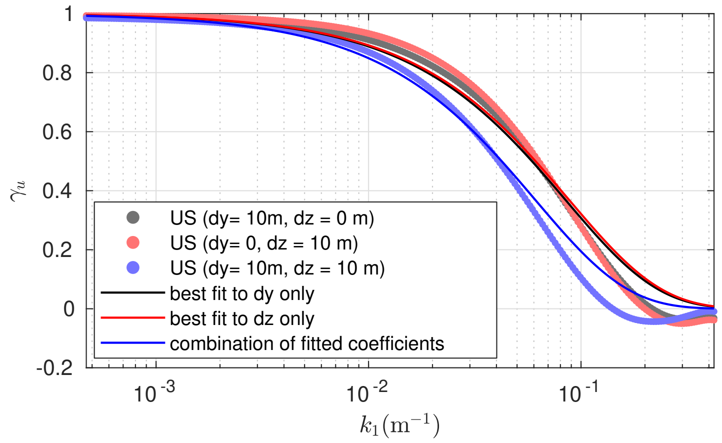

Figure 3: Co-coherence of the along-wind component computed for lateral separations d_y and vertical separations d_z usig the uniform shear (US) model (Mann, 1994) based on the Great Belt experiments (scatter plot). The red and black lines are the least-square fit of the Davenport model to the computed 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

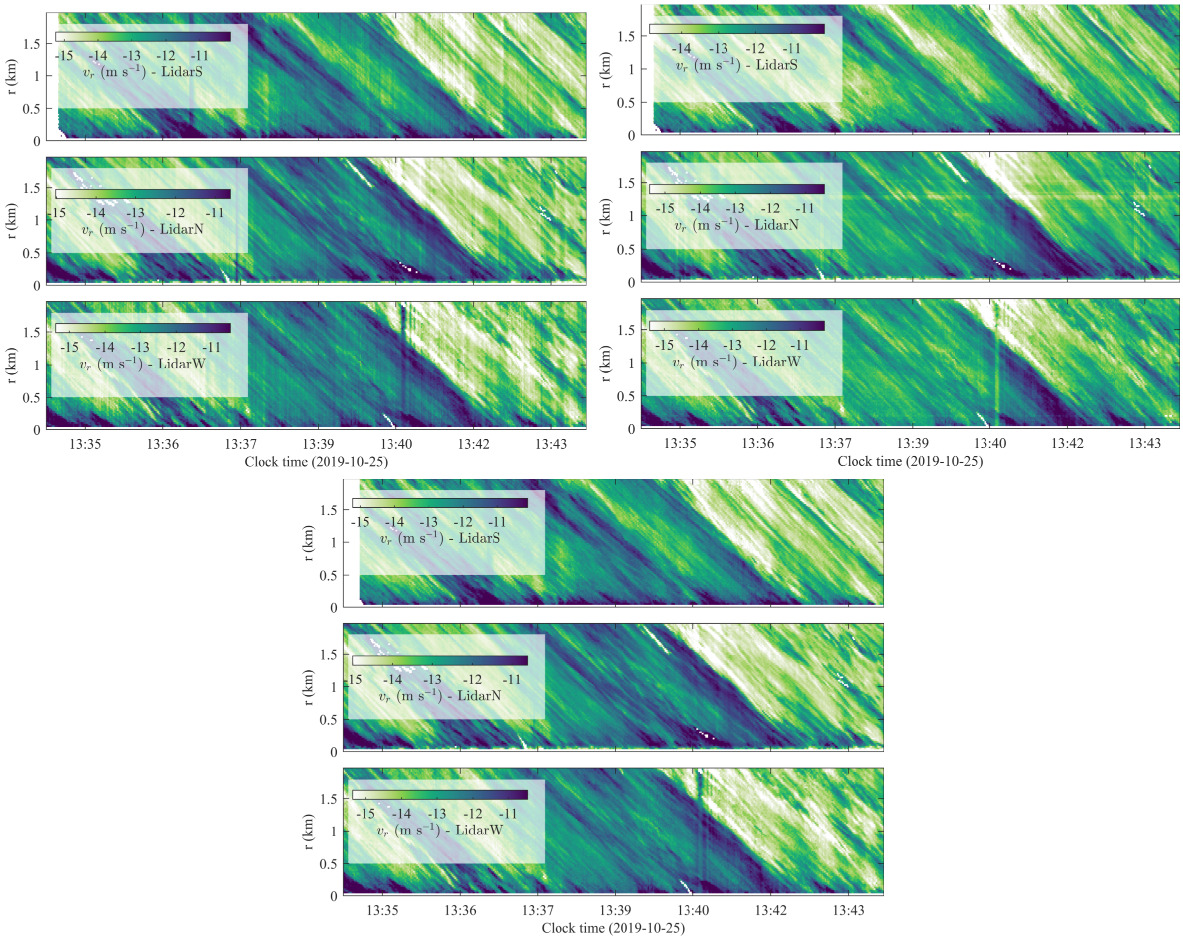

Figure 4: Top left: Without filtering out the vertical stripes. Top right: After filtering-out the vertical stripes using the method by Lange et al. (2019). Bottom: Vertical stripes filtered using the time-smoothed spatial average.

References

Andersen, O. J. and Løvseth, J. (2006). The Frøya database and maritime boundary layer wind description. *Marine Structures*, 19(2-3):173–192.

Bowen, A., Flay, R., and Panofsky, H. (1983). Vertical coherence and phase delay between wind components in strong winds below 20 m. *Boundary-layer meteorology*, 26(4):313–324.

Cheynet, E., Jakobsen, J. B., and Snæbjörnsson, J. (2019). Flow distortion recorded by sonic anemometers on a long-span bridge: Towards a better modelling of the dynamic wind load in full-scale. *Journal of Sound and Vibration*, 450:214–230.

Cheynet, E., Jakobsen, J. B., Snæbjörnsson, J., Mann, J., Courtney, M., Lea, G., and Svardal, B. (2017). Measurements of surface-layer turbulence in a wide norwegian fjord using synchronized long-range Doppler wind LiDARs. *Remote Sensing*, 9(10):977.

Cheynet, E., Jakobsen, J. B., Snæbjörnsson, J., Mikkelsen, T., Sjöholm, M., Mann, J., Hansen, P., Angelou, N., and Svardal, B. (2016). Application of short-range dual-Doppler lidars to evaluate the coherence of turbulence. *Experiments in Fluids*, 57(12):184.

Chougule, A., Mann, J., Kelly, M., Sun, J., Lenschow, D., and Patton, E. (2012). Vertical cross-spectral phases in neutral atmospheric flow. *Journal of Turbulence*, 13(8):1–13.

ESDU (2001). *ESDU 85020, Characteristics of atmospheric turbulence near the ground. Part II: single point data for strong winds (neutral atmosphere).*

Kristensen, L. and Jensen, N. (1979). Lateral coherence in isotropic turbulence and in the natural wind. *Boundary-Layer Meteorology*, 17(3):353–373.

Lange, J., Mann, J., Berg, J., Parvu, D., Kilpatrick, R., Costache, A., Chowdhury, J., Siddiqui, K., and Hangan, H. (2017). For wind turbines in complex terrain, the devil is in the detail. *Environmental Research Letters*, 12(9):094020.

Mann, J. (1994). The spatial structure of neutral atmospheric surface-layer turbulence. *Journal of fluid mechanics*, 273:141–168.

Peng, Y., Wang, S., and Li, J. (2018). Field measurement and investigation of spatial coherence for near-surface strong winds in Southeast China. *Journal of Wind Engineering and Industrial Aerodynamics*, pages 423–440.

Perry, S. G., Norman, J. M., Panofsky, H. A., and Martsolf, J. D. (1978). Horizontal coherence decay near large mesoscale variations in topography. *Journal of the Atmospheric Sciences*, 35(10):1884–1889.

Pielke, R. A. and Panofsky, H. A. (1970). Turbulence characteristics along several towers. *Boundary-Layer Meteorology*, 1(2):115–130.

Ropelewski, C. F., Tennekes, H., and Panofsky, H. A. (1973). Horizontal coherence of wind fluctuations. *Boundary-Layer Meteorology*, 5(3):353–363.

Toriumi, R., Katsuchi, H., and Furuya, N. (2000). A study on spatial correlation of natural wind. *Journal of Wind Engineering and Industrial Aerodynamics*, 87(2-3):203–216.