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

Q1.1 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 $\overline{u} \ge 5 \,\mathrm{m \, s^{-1}}$ 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.

Q1.6 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.

Q1.9 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.

Q1.11 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."

Q1.12 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

Q1.13 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 $180^{\circ}-270^{\circ}$ 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."

Q1.15 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. For-tunately, 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.

Q1.19 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.

Q1.21 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.

Q1.23 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 cocoherence 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.

Q1.32 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 interpretation 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.

Q1.39 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.

Q1.41 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.

Q1.42 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"

Q1.43 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 alongbeam 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 Wind-Cube 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 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.

Q1.51 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.

Q1.53 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.

Q1.54 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."

Q1.55 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

$$\gamma_u(d_x, d_y, f) \approx \cosh_u(d_x, d_y, f) \cos\left(\phi_x\right) \tag{1}$$

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 ($\overline{u} < 5 \text{ m s}^{-1}$) 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 $\overline{w} = 1 \text{ m s}^{-1}$, the contribution of \overline{w} to $\overline{v_r}$ would be $1 \times \sin(180/\pi \cdot 5) = 0.09 \text{ m s}^{-1}$, 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).

Q1.58 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.

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