Answer to reviewers

Dear Sir/Madam,

We thank the reviewers for the feedback to better improve our manuscript. Below is our answer to the reviewer's comments.

Reviewer 1

Reviewer's summary:

The manuscript attempts to analyze sonic measurements at one of the masts of the Vindeby wind farm to describe the turbulence characteristics in the offshore marine boundary layer. As it is shown, I have some major comments and a number of minor comments. I think the manuscript has some potential but right now it reads more as an overly descriptive technical report than a journal publication.

Overall response:

The original draft of the manuscript, especially the abstract may mislead the reader and has now been reformulated in the revised version. To make the manuscript more concise, we have created appendices to accommodate some of the supporting information that were detailed in the main body of the original submission. The definition of the Monin-Obukhov similarity theory (MOST) and the surface-layer scaling have been re-evaluated carefully in the revised manuscript. Concerning the flow distortion issue, it is addressed in detail in the reply to the community comment by Dr. Højstrup.

Major comments

Q 1.1 *The manuscript is too long and overly descriptive. I understand that the authors think that the many different aspects they are trying to analyze deserve to be documented/published. However, this effort makes the text to be tedious and too extensive. Also there is the tendency to explain concepts/theories that do not need explanation. Most importantly, the overly descriptive and long aspect makes the study too unfocused and so the manuscript reads more as a technical report of different analyses, which were performed with these measurements during the course of a project. I suggest that the authors concentrate in a particular aspect (I will suggest later which one(s)) and develop the manuscript towards answering the questions that such aspect(s) rises.*

Reply: In the attempt to make the manuscript easier to read, we have moved some of the results in the appendix. The objective of the paper is to identify similarities between the spectral characteristics at FINO1 and Vindeby. The paper focuses on three aspects:

- A data quality assessment of the sonic data
- The one-point spectral characteristics with a comparison from the predictions from FINO1
- The two-point spectral characteristics through the co-coherence with another comparison with the predictions from FINO1

In this regard, the manuscript is quite specific and focused on the flow characteristics required for wind loading on offshore wind turbines. We are aware that we are quite detailed in the data processing and data quality analysis but we consider a stringent and well-documented data analysis of a study dealing with full-scale measurements.

Finally, we explain concepts and theories, even already known, these were found necessary to make the manuscript self-explanatory. The manuscript targets an audience with an engineering background (since it is about wind energy) so we feel it is necessary to be as pedagogical as possible.

Q 1.2 *In general, and particularly in the abstract, the authors claim that spectra follow/not follow MOST.*

MOST does not really predict the behavior of the velocity spectra. MOST basically says that within the surface layer, gradients (wind and temperature) when properly normalized (e.g. by u) are function of the dimensionless stability. Yes, one can also prove that similarly to MOST, proper scaling can be applied to the spectra but this does not mean that MOST itself suggested such scaling for the spectra. I guess you can call it "surface-layer scaling" or "surface-layer similarity".*

Reply: Our reference was to Kaimal et al. (1972) and his contribution to MOST, i.e. we did not imply that Monin and Obukhov suggested the scaling of the velocity spectra in the surface layer. To keep it consistent with the wording used by Kaimal and Finnigan (1994), in the revised manuscript, when we talk about the spectral characteristics, we now refer to "surface-layer scaling" instead of "MOST".

Q 1.3 *In the abstract the term "turbulence characteristics" is used. You should specify what do you mean by this. Is it about length scales? Spectral peaks? Turbulence anisotropy or dissipation? What are the characteristics you are referring to?*

Reply: We are referring to the one-point auto spectral densities and the real part of the root-coherence. These are now clarified in the abstract.

Q 1.4 *In the last part of the abstract and I think later in the conclusions, the authors mention that their findings are relevant for load estimations of offshore wind farms. However, as the authors acknowledge, the levels they study do not cover those in which current offshore turbines operate. So what did we learn for offshore wind energy? In combination with my first point, I think that the authors can concentrate on understanding aspects we have not explored much in wind energy although they might not have an impact on the loads of turbines (I am fine whether this is important or not to loads). For example, I was particularly happy to see that they were looking at the influence of waves on the turbulence measures. However, I was disappointed because the authors do not seem to make an effort on continuing analysing this influence. For me, it seems to be the most interesting aspect that the paper could explore and I would recommend that a future revision of manuscript focuses on this*

Reply: Coherence is a key parameter for wind loading on structures. In this study, the similarities we identify with the one estimated at FINO1 support our statement that our findings are highly valuable for wind turbine design.

Our findings regarding the one-point velocity spectra for wind turbine design are also crucial for any researcher working on (offshore) wind loading because such a description is rarely available in the scientific literature. Furthermore, since we use surface-layer scaling, the description of the power-spectral densities is adapted for the entire surface layer. Also, we highlight multiple similarities between the velocity spectra at Vindeby and FINO1, which is one of the key aspects needed for offshore wind turbine design.

It should be pointed out that the study of wind-wave interaction is limited by the available instruments near SMW. Also, the paper is about the characterization of atmospheric turbulence for wind turbine design. Elaborating excessively on the air-sea interaction would mean that the paper goes off-topic and that it would not be suitable any longer for Wind Energy Science.

Q 1.5 *You are using Gill 3-axis sonics. These are known to be affected by flow distortion. Do you apply any flow distortion correction to these measurements? If not, why not? I think you should elaborate* *more on this as you also point out (see line 272) that w in particular could be highly affected by probe-induced flow distortion. So if there is flow distortion (I think there is) why will this affect more the 6 m than the 18 or 45 m measurements? By how much you will reduce or increase your fluxes using corrections for flow distortion (in relation to your quest on finding out the differences between u_* at the different levels)? Until this is not clarified, then I would omit Figs. 4 and 5 (and so help a little bit with shortening the paper as part of my comment 1). You kind of "deny" the flow distortion issue by saying that your Sw/Su ratios (1.2) are close to the ones of Fino. This is however not an argument as at Fino there might be other things happening and the same ratio can be achieved by the combination of two opposite issues, for example (two or more wrongs can make the result to look good). As you also mention (lines 297-298) the spectral ratios are more easily reached by Sv/Su than Sw/Su, which is a sign of flow distortion!*

Reply: We have added a new section in the appendix to address the flow distortion by the sonic anemometers. More details about this topic are given in our reply to Dr. Højstrup, who wrote a community comment (CC) available at https://doi.org/10.5194/wes-2021-75-CC1. The statement of the reviewer that Gill 3-axis sonic anemometers are known to be affected by flow distortion is partly true only. It has been documented for more than one decade that all sonic anemometers are affected by flow distortion (Peña et al., 2019; Horst et al., 2015). If the reviewer refers to the Gill "w" bug (Instruments, 2016), this only applies to a number of Gill anemometers produced between 2006 and 2015. Flow distortion is partly corrected during wind tunnel tests at the time of the instrument's calibration. A posteriori correction is not straightforward unless the sonic anemometer is tested again in a wind tunnel.

***Q 1.6** Line 278: friction velocity averaged between two heights in Fig. 5? I guess you mean Fig. 6?*

But anyway, you should not do that. How are you computing du/dz? Simple wind speed differences between heights? You seem to have the opportunity to use the cup anemometers that are just above and below the sonic to do this (and avoid please the friction velocity averaging). So these dimensionless wind shears need to be recomputed. If the cup data is not there then you should use at least three speed levels to do a better fitting, e.g. using a wind speed polynomial but still using the local friction velocity. And yes, measurements below 10 m might be outside the surface layer but inside the viscous or wave layer (in the offshore case). So it is actually ok to find that ϕ_m at this heights are not following MOST.

Reply: The results presented in the original draft showed that ϕ_m follows fairly well MOST but deviations from MOST were found for ϕ_w. An improved agreement of ϕ_w with MOST was obtained by using local surface-layer scaling, which was not the case in the original draft. In the revised manuscript, the discussion about ϕ_w was removed, as suggested by the reviewer for the following reasons: (1) there exist some uncertainties due to transducer-induced flow distortion, and (2) the study of ϕ_w is not among the main objectives of the paper.

Regarding the study of ϕ_m, we thank the reviewer for the different suggestions. We have tested different approaches to compute ϕ_m using a polynomial fit with an order of 1 or 2 to the wind speed gradient and the friction velocity to obtain local values. In the present case, we found that the approach presented in the original draft was more appropriate. The polynomial fit led to some spurious results.

Regarding the choice of friction velocity as an average or a local measurement, the choice is not as straightforward as implied by the reviewer. The friction velocity is responsible for the largest uncertainty in the calculation of ϕ_m since it is derived from the covariance of two weakly correlated time series. Therefore, the use of spatial averaging reduces the uncertainties. If the friction velocity changes significantly with the height, it may reflect the presence of an internal boundary layer. In this case, ϕ_m is not defined.

In this study, we decided to focus on the sonic anemometer data as the cup anemometer records have been analysed in earlier studies. In our investigation of the wind-wave interactions, we have demonstrated that the number of samples at 6 m located in the wave sublayer is not significant. Since the manuscript is about the comparison of the spectral characteristics at Vindeby with the predictions from FINO1, we do not think it is necessary to further elaborate on the different approaches to compute ϕ_m.

Q 1.7 *I am not sure if the amount of records you are using to derive the spectra (Figs 12-14) are the same that you use to present the other stability-related results, but when looking at these figures I can see that your records in the most unstable and stable cases are too few and too noisy particularly in the very stable plot. So I recommend you do not use those stability bins and I recommend you combine the next two stables ones in one and the next two unstable ones in one to increase the significance of the results and reduce the noise.*

Reply: The number of samples for $\zeta > 1$ is 18, which is good enough to obtain meaningful averaged spectral characteristics. For comparison, Kaimal et al. (1972) had only five hours of data for stable conditions and could still draw useful conclusions. The number of samples is not the only parameter that plays a role in the estimation of the averaged power spectral density. Knowledge of the method to compute the power spectral densities is also needed. In the present case, we used Welch's modified periodogram with three segments, which "artificially" increases the number of samples. We do not recommend merging the stability bins, given that the noise level at 18 m and 45 m increases with the stability. Furthermore, the turbulence can change significantly for $0.5 < \zeta < 2$. For this reason, we decided to keep the 9 panels and simply warn the reader that with fewer samples, the uncertainties are larger.

Minor comments

Q 1.1 *Line 2: The second line should read "Sonic anemometer measurements at 6, 18 and 45 m ...", so that we already know you are using sonic observations. Also for this and all instances, compact the listing: so instead of saying "6 m, 18 m and 45 m" replace by "6, 18 and 45 m"*

Reply: Replaced, as suggested by the reviewer.

Q 1.2 *Line 6: replace "empirical spectra established on" by "that from"*

Reply: Replaced, as suggested by the reviewer.

Q 1.3 *Line 9: Replace "with those at" by "that at"*

Reply: Replaced, as suggested by the reviewer.

Q 1.4 *Line 37: Replace "are justified for" by "relate to"*

Reply: Replaced, as suggested by the reviewer.

Q 1.5 *The sentences between lines 39 and 43 need to be rewritten. First I thing you are mainly talking about the Mann model and second I am not sure of what model you refer to when citing Kelly (2018) (from what I can see there is no model there other than the Mann model)*

Reply: We agree with the reviewer. However, we prefer the use of "uniform shear model" rather than "Mann model" because there are two different models proposed in Mann (1994): the uniform shear (US) and the uniform shear with blockage by the surface (US+B). These sentences have been reformulated as: "In this regard, the present study addresses similar challenges as discussed by Kelly (2018) but focuses on some specific aspects not covered by the spectral tensor of homogeneous turbulence (Mann, 1994). Firstly, the low-frequency fluctuations are generally underestimated by the uniform-shear model, especially under convective conditions (De Maré and Mann, 2014; Chougule et al., 2018). Secondly, the vertical coherence of turbulence is not always described accurately by the spectral tensor (Mann, 1994; Cheynet, 2019)."

Q 1.6 *Lines 48 and 50 and maybe other instances: be consistent so it is either heights, levels or altitudes (first is preferable)*

Reply: The term 'height(s)' is now used in the revised manuscript whenever possible for consistency.

Q 1.7 *Line 45: "semi-empirical models from FINO1": this is the first time we hear somebody came up with such models from FINO1, so you need to provide some context, a reference, and probably also say models of what exactly*

Reply: We have reformulated the sentence as: "Then, the one-point velocity spectra and co-coherence estimates from Vindeby are compared with predictions from semi-empirical models established on the FINO1 platform (Cheynet et al., 2018) to assess the similarities of the spectral characteristics between the two sites."

This sentence implies directly that the semi-empirical models we are referring to are the one-point velocity spectra and the coherence. The reference is deemed adequate so that we do not need to elaborate further on the model. Otherwise, the manuscript may become too large.

Q 1.8 *Figure 1: Denmark in the left and particularly Lolland in the right look quite flooded (blue areas where green should be). I guess this is because your Digital Elevation Model (DEM) shows 0 m for areas that are not water areas*

Reply: Yes this is a correct guess. We have updated the figure with the corrected land cover.

Q 1.9 *Lines 65 and 84 delete "of" after "comprised"*

Reply: Deleted, as suggested by the reviewer.

Q 1.10 *Section 2: I do not think you mention what kind of cups and vanes you have and the heights where they measure*

Reply: The measurements from the cup anemometers were not used, therefore we did not provide the cup anemometers' detail in the manuscript. To prevent confusion, we added a statement in the manuscript to clarify that the measurements from the cup anemometers were not used, which reads as "There were seven cup anemometers mounted on SMW as shown in fig. 3. However, their measurements were not used here". The heights of the vanes are already provided in the following line "Two Risø P2021 resolver wind vanes with wind direction transmitters P2058 were located on the northern booms at 43 m and 20 m amsl using a sampling frequency of 5 Hz".

Q 1.11 *Line 82: delete "the wind"*

Reply: Deleted, as suggested by the reviewer.

Q 1.12 *Line 94: add "as" after "denoted"*

Reply: Added, as suggested by the reviewer.

Q 1.13 *Line 96: Replace "To study turbulence for wind turbine design" by "Here,"-> this is an example of lengthy sentences that can be shortened without deteriorating and makes the paper shorter (there are many like this so please make an effort to be more concrete and short)*

Reply: Replaced, as suggested by the reviewer. The other changes are marked with magenta-coloured fonts.

Q 1.14 *Line 98: "modeling the v-component" I guess you mean modeling the v-spectrum or v-variance as v=0 in most cases as we align u with the mean wind (you do that actually)*

Reply: What we meant by modelling the v-component is the time histories of the lateral wind velocity component. This implies the modelling of both its one-point power spectral densities and its root-coherence. This has been clarified in the following line "Although the u-component drives the wind turbine's rotor fatigue loads, proper modelling of the v-component in terms of power spectral density (PSD) and root-coherence may be necessary for skewed flow conditions, which can occur because of a large wind direction shear (Sanchez Gomez and Lundquist, 2020) or wind turbine yaw error (Robertson et al., 2019)".

Q 1.15 *Line 110: add "vertical" before "flux"*

Reply: Added, as suggested by the reviewer.

Q 1.16 *Line 113: why is θ_v not reliably measured by a sonic?*

Reply: The mean sonic temperature was known to have a measurement bias. This was documented to us through private communication with Dr. Kurt Hansen. However, the fluctuating sonic temperature is unaffected by such a bias since, per definition, the fluctuating component is detrended. The mean virtual potential temperature was thus obtained using other sensors, as described in the manuscript.

Q 1.17 *Line 121: ϕ_w is not commonly used to assess MOST. Perhaps ϕ_m and ϕ_{temp}*

Reply: As emphasised by e.g. De Franceschi et al. (2009), ϕ_w has become widely used in the past 30 years to study the applicability of MOST. The study of ϕ_w has a considerable advantage over methods based on a gradient or bulk parametrisation as it allows using point-measurements with ultrasonic anemometers. The use of 3D sonic anemometer in the 1990s has made highly relevant the study of the ratio σ_w/u_* as a function of z/L to assess the applicability of MOST.

Q 1.18 *Line 135: the spectrum is not a quantity. Anyway, the whole paragraph between lines 135 and 138 is not needed*

Reply: This line is formulated as "An appropriate modelling of the one-point velocity spectrum is required to compute reliably the dynamic wind-induced response and the power production of wind turbines (Sheinman and Rosen, 1992; Hansen and Butterfield, 1993)" in the revised manuscript. The lines 135-138 are not omitted since they provide an important point that is not addressed in the IEC 61400-1. Turbulence intensity (TI) is considered as an important input for turbulence modelling according to IEC 61400-1. Nonetheless, as pointed out by Wendell et al. (1991), turbulence intensity may not always be a reliable characterization of turbulence because TI does not carry information concerning the distribution of eddies in the frequency domain.

Q 1.19 *8: remove the 2/3 as exponent of ϕ_ε*

Reply: Removed, as suggested.

***Q* 1.20** *12-14: Between the description of these equations you should give some values for c1 and c2 so that c in Eq. 12 is negative otherwise you need to add a minus in the argument of the exponent*

Reply: A minus sign is added in Eq. 12.

***Q* 1.21** *15 is the cross a dot product?*

Reply: It is not a cross but an ordinary multiplication symbol. The ‘cross’ symbol has been removed from Eq. 15 to avoid confusion.

***Q* 1.22** *Line 197: so is data plural or singular?*

Reply: According to Copernicus Publication, the word "data" is considered as a countable noun (e.g. data are, data were, data include). In the revised manuscript, we evaluate the word "data" as countable noun.

***Q* 1.23** *Line 200: reliable estimation of Obukhov length means turbulence flux estimations. Why not completely taking out the 45 m sonic anemometer measurements, at least for the spectra analysis? I mean you continuously mention that this sonic is highly affected by noise. For the coherence it could be fine to use as the noise reduces by the cross-spectrum computation.*

Reply: We also mention that the measurement quality is satisfying for wind speed above 8-10 $\mathrm{m\,s^{-1}}$ at this height. Therefore, it would be inaccurate to state that the measurement noise prevents any analysis. The reviewer’s comment regarding the noise reduction by the normalisation of the cross-spectrum for the study of the coherence is correct. The co-coherence is not substantially affected by the noise because the noise is more important at high frequencies, where the co-coherence is typically zero for the separation distances considered.

***Q* 1.24** *Line 212: how do you know the planar fit gives better estimates of covariances compared to double rotation? I mean compared to what? In my understanding, it is completely the opposite*

Reply: We did not conclude that the planar fit method gives better estimates of covariances, it was simply stated in the work by Wilczak et al. (2001). To avoid confusion, we added a reference to the work by Wilczak et al. (2001) in this sentence. The sentence is now read as“It should be noted that this finding is likely specific to the Vindeby data-set as the planar fit method usually provides better estimates of the turbulent fluxes (Wilczak et al., 2001)”.

***Q* 1.25** *Line 234: to compute that mean wind speed you need also a friction velocity value at least (which you do not mentioned) or need to do perform another computation/assumption (such as a geostrophic drag law)*

Reply: We did compute the mean wind speed using the following relation:

$$u_{z2} = \frac{\ln(\frac{z_2}{z_0})}{\ln(\frac{z_1}{z_0})} u_{z1} \tag{1}$$

where z_2 is taken as 90 m (hub height) and z_2 is taken as 18 m. The value of z_0 was not calculated but taken from the scientific literature (it is a widely used value roughness for calm sea). To avoid further misunderstanding, we have added some references to justify the use of $z_0 = 0.0002$ m in the manuscript, for example WMO (1983).

Q 1.26 *Line 241: delete "which testified"*

Reply: Deleted, as suggested by the reviewer.

Q 1.27 *Line 265: flat or uniform "terrains" no plural... not the only instance with this issue similar happens with "noises"... no need for plural (line 267 and maybe other instances)*

Reply: Changed, as suggested by the reviewer.

Q 1.28 *4: friction velocity "computations" or "calculations" not "estimations". Also the dimensionless stability in the legend appears with units of m*

Reply: The friction velocities were computed, however, these numbers are an 'estimation' since we are working with times series that are described as ergodic, stationary random processes. We applied temporal averaging operators. Therefore, all statistical quantities computed are, indeed, biased or unbiased estimates of the "true" quantities. The unit m for the dimensionless stability is removed from fig. 4.

Q 1.29 *5: For 18 and 6 m, the error bars are quite small the more stable or unstable (the most unstable is nearly zero error for the 6 m). So the uncertainty should be presented with other metric (standard error or deviation). I would definitively skip this graph as the w component might be too affected by flow distortion*

Reply: We removed this figure in the revised manuscript as suggested by the reviewer.

Q 1.30 *7: units in m/s or in m s^{-1}*

Reply: For consistency, the unit is re-written to m s^{-1}.

Q 1.31 *Line 285: the sentence does not makes sense as the 18 m value is a local value*

Reply: Initially, the term 'local value' used in this sentence refers to the friction velocity values at each corresponding height. Nonetheless, this sentence has been removed in the revised manuscript because it is related to fig. 5 (variation of σ_w/u_*) which has been taken out in the revised manuscript, as suggested by the reviewer (see Q 1.29).

Q 1.32 *5.3 is not needed and can be removed without endangering the study (see major comment 1)*

Reply: We move Section 5.3 (Estimation of the friction velocity) to appendix for shortness and simplicity of the manuscript.

Q 1.33 *Line 340: did you introduce the quad-coherence before? I mean you do introduce the Co-coherence*

Reply: We did not introduce the quad-coherence, therefore a brief wording is added to introduce the quad-coherence in this line, "The interactions between wind turbulence and the sea surface were explored in terms of the co-coherence and the quad-coherence (the imaginary part of the root-coherence) between the vertical velocity component w and the velocity of the wave surface $\dot{\eta} = \mathrm{d}\eta/\mathrm{d}t$".

Q 1.34 *Line 378: didn't you already introduce the reduced frequency?*

Reply: Yes, the reviewer is correct. The reduced frequency f_r is already introduced in the first paragraph of Subsection 3.2, therefore the definition of f_r in this line is now removed in the revised manuscript.

Q 1.35 *Line 379: I guess you need to delete the 2/3*

Reply: The spectra were normalised by $\phi_\varepsilon^{2/3}$, therefore the ''2/3'' is not removed, but in Eq. 8 which it was referred to is now deleted, kindly refer to minor comment Q 1.19.

Q 1.36 *Line 382: "empirical model established at Fino1... (Cheynet et al., 2018)". So you have not introduced this. If you refer to Eq. (15) you actually attributed this to Cheynet 2019. By the way this also points me to the references: you have way too many references to your own work (Cheynet) and I am sure many others have done similar studies. I am also sure that you do not need to cite all of your studies but a couple of them*

Reply: In our manuscript, the number of self-citations is around 5%, which is well below the median value of self-citation documented in the literature, that is between 10% and 13% (Ioannidis et al., 2019; Szomszor et al., 2020). We refer often to the same references because we are conducting a comparison with predictions from another dataset. The reviewer is welcomed to suggest other relevant references to the topic that we may have missed.

Q 1.37 *Line 384: "the behavior of surface-layer spectra" does not the spectra of velocities above surface layer also behave like this in the asymptotic limit?*

Reply: Not necessarily, because the red curve is obtained for surface layer scaling only. Another scaling is needed above the surface layer. Kindly refer to Section 2.6 from Kaimal and Finnigan (1994).

Q 1.38 *Line 387: "which is another... properly" as mentioned earlier two or more wrong things can make the result to be ok so no this is not an indication that the estimation is properly done*

Reply: We agree with the reviewer. We have removed this statement from the line in the revised manuscript.

Q 1.39 *Sentences between lines 387 and 391 can be removed without detriment*

Reply: We agree that the sentences in line 388 to 391 may be repetitive, therefore these lines are now removed.

Q 1.40 *In page 19 there are many entries with reference to goodness of the spectra with respect of MOST so this needs to be rewritten (major comment 2)*

Reply: We have replaced the word "MOST" with "surface-layer scaling", as detailed in our reply to major comment Q 1.2.

Q 1.41 *Line 414: the reasoning of the flatness of the spectral peak is not the difficulty in estimate the integral length scale... on the contrary it is difficult to estimate the length scale due to the flatness of the spectra*

Reply: We agree with the reviewer. This sentence was poorly written. We have reformulated it as "This leads to a flat spectral peak. As a result, the integral length scale would be estimated with large uncertainties".

Q 1.42 *Line 418: Well this is nice that you state that these deviations are due to the contribution of waves, but how do you know this? Following my major comment 4, this could be something to concentrate efforts in the study; I mean demonstrating that these deviations are caused by the waves*

Reply: In the manuscript, we demonstrate that the number of samples showing a clear correlation between turbulence and the waves is too small to be significant. We cannot demonstrate that the roughness upstream of the mast is due to a heterogeneous sea state. Therefore, we concluded that flow distortion may explain (at least partly) the variability observed, which is a safer assumption.

Q 1.43 *Fig 12 and similar: delete the "for references... in the data" of the caption.*

Reply: We agree with the reviewer. These are now removed from the caption for fig.10, fig.11, and fig.12.

Q 1.44 *Line 423: is the -2> not a -2<?*

Reply: It is corrected to $-2 \leq$.

Q 1.45 *Line 433: delete "since we aim... Vindeby"*

Reply: Deleted, as suggested by the reviewer.

Q 1.46 *Line 435: did you omit the value of C3v?*

Reply: No, it was not omitted. The values are [0, 23, 0.09], however it seems that we missed a coma after "23". To be consistent, we added a coma after 23, so it now becomes [0, 23.0, 0.09].

Q 1.47 *Line 439: so why is the coherence of v negative? It seems to be also the case in Fino1. So, why don't you use the Mann coherence here? I think it could provide you with negative coherences*

Reply: The co-coherence of the v-component is negative because the vertical shear introduces a time lag between measurements at different heights. The lateral component is more affected than the along-wind component (Bowen et al., 1983). Chougule et al. (2012) offers a possible interpretation for the larger phase angle for the cross-wind component compared to the along-wind component. The negative part is more visible if measurements are located close to the ground, where the mean shear is larger. For this reason, the negative values of the co-coherence were more visible in the Vindeby database than in the FINO1 database. At FINO1, the negative part was small enough to be neglected. The co-coherence estimated with the uniform shear model (Mann, 1994) is, indeed, able to model the negative part of the co-coherence. However, it is unclear whether this model can reliably model the negative part. On the other hand, the uniform shear model is known to overestimates the co-coherence at vertical separations and low frequencies (Mann, 1994). This drawback is of greater importance for wind turbine wind-induced load predictions.

Q 1.48 *Caption Fig. 15: change "empirical values computed" by "predictions"*

Reply: Changed, as suggested by the reviewer.

Q 1.49 *Line 447: "lateral co-coherence is also required".... For what?*

Reply: The sentence is now reformulated as "Finally, additional data collection is needed to study the co-coherence at lateral separations, which is required for wind turbine design since it was not available at FINO1 nor SMW".

A wind turbine cannot be approximated as a line-like structure like a tower or a bridge. Therefore, information on the coherence for both lateral and vertical separation distances is needed. Whereas the vertical coherence can be studied using anemometers mounted on a single mast, the study of the lateral coherence requires either multiple masts or excessively long booms, which may not be feasible offshore for financial or technical reasons.

Q 1.50 *Lines 448-450 can be deleted without detriment*

Reply: Based on the response for minor comment Q 1.49, these lines are not deleted but reformulated instead: "Further studies are, however, needed to better quantify this possible overestimation in terms of dynamic wind loading on the wind turbine's rotor and tower, as well as on the floater's motions in the case of a floating wind turbine. Finally, additional data collection is needed to study the co-coherence at lateral separations, which is required for wind turbine design since it was not available at FINO1 nor SMW."

Q 1.51 *Lines 454-455: do people use vertical coherence models for aeroeslatic turbine simulations when not using the Mann model?*

Reply: Yes, people use the vertical (and lateral) coherences when simulating a spatially correlated turbulent wind field when not using the Mann's model. For example, the traditional approach for turbulence generation method by Veers (1988) requires knowledge of the coherence of turbulence.

We have a lot of respect for the uniform shear (US) model, especially regarding its remarkable ability to describe the second-order structure of homogeneous turbulence with a limited set of parameters. However, as mentioned in the introduction, there are several limitations of the US model for aeroelastic loading calculation. One of them is the limited ability of the US model to describe realistically the vertical coherence. On the other hand, the US model is known to perform well when it comes to describing the lateral coherence. In this regard, the US model could still be used to complement the semi-empirical models we have mentioned in the manuscript. Another limit is that the three parameters of the US models do not change in space, which is known not to be the case in reality.

Q 1.52 *Line 459: replace "The first one is related to the fact that the" by "The"*

Reply: Replaced, as suggested by the reviewer.

Q 1.53 *Line 460: why nonstationary time series are not reliable?*

Reply: We are estimating turbulence characteristics from time histories using operators such as mean, standard deviation, or power spectral densities. A fundamental condition to use these operators is that the time histories are stationary. Otherwise, the turbulence characteristics are biased. More generally, turbulence is here described as a stationary random process. The description of the non-stationary characteristics of turbulence is beyond the scope of the present study. It can be noted that in standards and codes, the design of offshore wind turbines also relies on the assumption that turbulence is stationary.

Q 1.54 *Lines 466-467 Delete "Therefore,... factor"*

Reply: Deleted, as suggested by the reviewer.

Q 1.55 *Line 469 what does invariant here mean?*

Reply: The term 'invariant with height' in this context means does not change with height. The sentence is rephrased to avoid ambiguity, and read as: "Above the surface layer, the velocity spectra may become independent of the height above the surface, which is coarsely accounted for in IEC 61400-1 (2005) and suggested by preliminary observations from Doppler wind lidar instruments in coastal areas (Cheynet et al., 2021)"

Q 1.56 *Line 471-477: these lines are not needed*

Reply: These lines are deleted, as suggested.

Q 1.57 *In the conclusions you again start to introduce acronyms; this is not needed*

Reply: A conclusion is not part of the manuscript's body, so acronyms need to be redefined for the sake of clarity. Intuitively, it also makes sense since many readers of a paper only read the abstract, introduction, and conclusion.

Q 1.58 *Line 482: "relevant for the design of offshore wind turbines"... this is not true (major comment 4). Similar issue in line 504*

Reply: The latter opinion is refuted by Veers et al. (2019) since wind loading on offshore wind turbines is included in two of the three great challenges in wind energy.

Q 1.59 *Lines 501-502: well the 45 and 18 m are not that close to the surface*

Reply: It was not stated in the manuscript that 18 m and 45 m are close to the sea surface. Instead, it is written 'closer to the sea surface' because in this context, we are comparing 18 m and 45 m at SMW with 60 m and 80 m heights at FINO1.

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