Community Comment by Dr. Højstrup

November 30, 2021

We thank Dr. Højstrup for the feedback on our manuscript. Below is our answer to his comments.

Sonic anemometer flow distortion

Q 1.1 *In addition to the Solent omni-directional anemometers during the basic measurements, three more sonics were added, at 32m (omnidirectional Solent), 3m and 10m (asymmetric Solent with less flow distortion). The omnidirectional Solent shows considerable flow distortion, here illustrated with the ratios of measured u* at 18m (omnidirectional) to measured u* at 10m (asymmetric sonic), fig. taken from a presentation by me at Oregon State University, 7 May, 1998:*

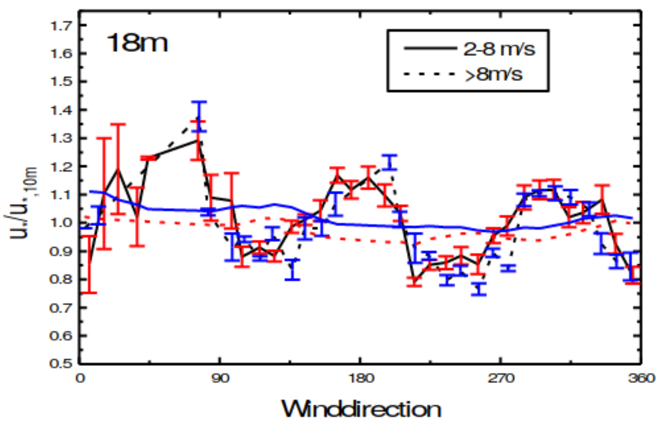

Figure 1: Ratio of the friction velocity estimated at 18 m (omnidirectional Solent) over the one at 10 m (asymmetric Solent) by Dr. Højstrup. Unknown time period.

Reply: The point raised by Dr. Højstrup is indeed relevant to the present study. We have added an appendix in the manuscript, to discuss the transducer-induced flow distortion. The following content takes the appendix and complements it when necessary. We remind that the present Community Comment is publicly available, which means that our reply is also available to anyone.

The dataset at 3 m was too short to be meaningful so it is not discussed hereinafter. So we will focus mainly on the asymmetric solent at 10 m. The dataset from the sonic anemometer (SA) at 10 m was from May 1994 to September 1994, which was still much shorter than the other instruments.

In the following, one assumes that the sonic anemometer at 10 m does not show significant flow distortion for the sector 220°-330°. The latter sector is the one that was selected in the original draft. It is possible to partly correct the friction velocity estimate at 6 m, 18 m and 45 m for the flow distortion by the transducer by using a multivariate regression analysis. The objective of the correction is to assess whether the corrected friction velocity changes substantially the results regarding the power spectral densities of the velocity fluctuations.

In the present case, the flow distortion is assumed to be a function of the angle of attack $\alpha(z)$ and wind direction $\theta(z)$ only. For the relatively narrow sector selected, it was found that cubic functions of $\alpha(z)$ and $\theta(z)$ were sufficient to describe this variability. This leads to the following relationship between the friction velocity at 10 m and the height z:

$$u_*(z) = (u_*)_{10} \cdot \mathbf{A}\mathbf{X}^\top \tag{1}$$

where

$$\mathbf{A} = \begin{bmatrix} a_1 & a_2 & a_3 & a_4 & a_5 & a_6 \end{bmatrix} \tag{2}$$

$$\mathbf{X} = \begin{bmatrix} \theta(z) & \theta(z)^2 & \theta(z)^3 & \alpha(z) & \alpha(z)^2 & \alpha(z)^3 \end{bmatrix} \tag{3}$$

The coefficients to be determined with the regression analysis are a_i, $i = \{1, 2, 3, 4, 5, 6\}$ as shown by eq. (2). In eq. (1), the error is modelled as a non-linear function of the angle of attack and wind direction. In this regard, we do not assume that the friction velocity is constant with the height nor that the flow distortion is similar for the three omnidirectional anemometers.

In fig. 2, we have reproduced some of the results from fig. 1 but for the sector addressed in the present study, i.e. between 220° and 330°. The left (right) panel shows the uncorrected (corrected) ratio of the friction velocity estimates. Including larger sectors has limited usefulness for this comparison. In particular, there exist sectors where the transducer shadowing is much larger for the asymmetric solent at 10 m than the omnidirectional solent at 6 m, which is not clearly highlighted in fig. 1. Therefore, fig. 1 should be interpreted with caution.

In the left panel of fig. 2, the maximal variations of the friction velocity between the sonic anemometer at 10 m and 18 m are ±20%. When all the samples in the sector 220°-330° are averaged, the relative difference at 6 m, 18 m and 45 m with respect to the data at 10 m are 4%, 12% and 11%, respectively. After the multivariate regression, the mean error was close to zero, although it is clearly not zero for a given wind sector (fig. 2). On average, the friction velocity

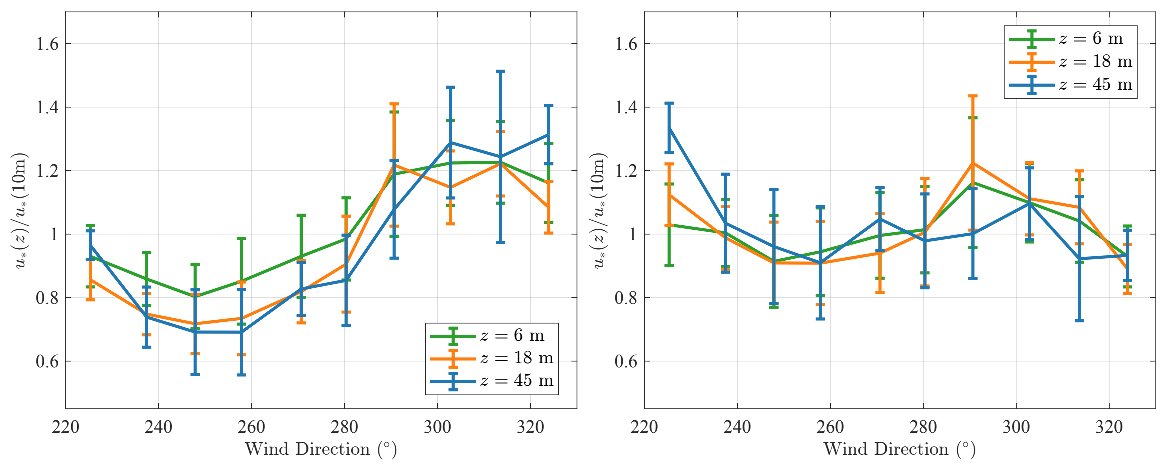

Figure 2: Ratio of the friction velocity at 18 m (omnidirectional solent anemometer) over the one estimated at 10 m (asymmetric solent anemometer) before (left panel) and after (right panel) correction using a multivariate regression analysis. Velocity data recorded between May 1994 and September 1994 for the sector 220°-330° were used (480 samples of 30 min duration) and $|z/L| < 2$ at 10 m asl.

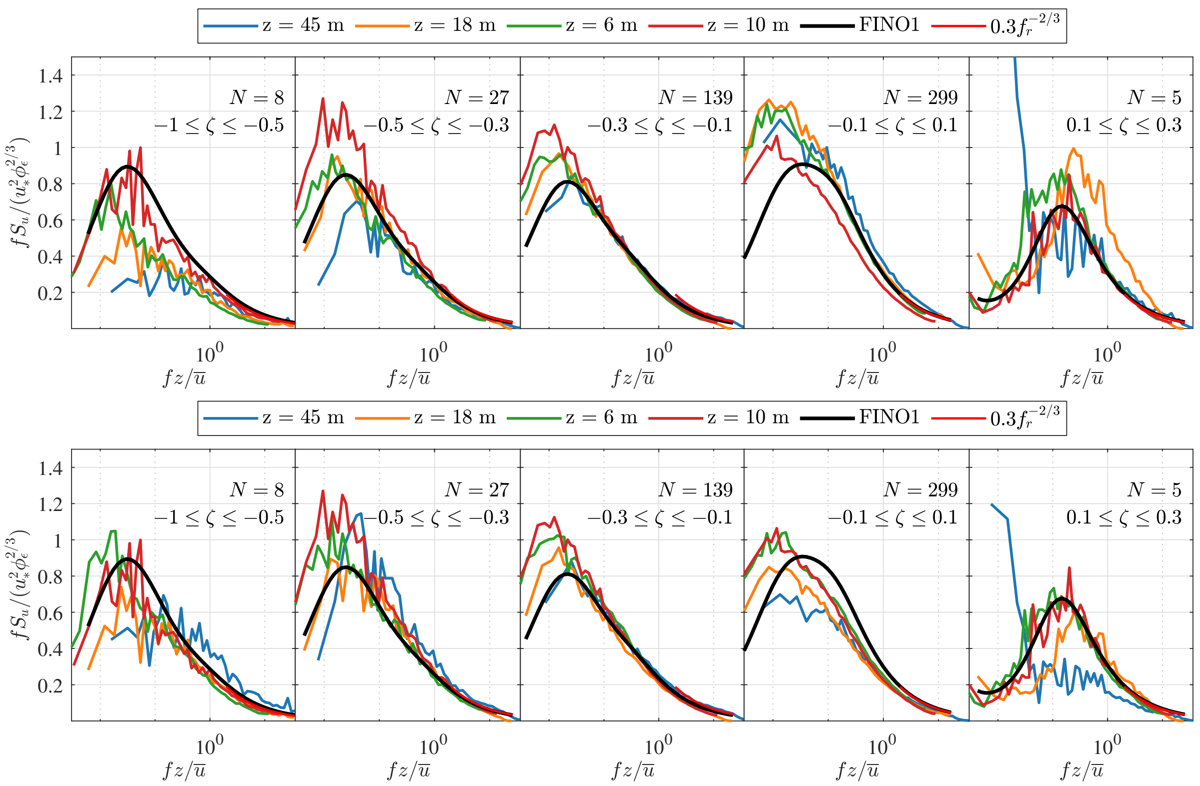

Figure 3: Power spectral densities of the along-wind component using the uncorrected friction velocity (top panels) and corrected one (bottom panels). The parameter z/L was estimated at 10 m and the data set relied on measurements from May 1994 to September 1994.

estimates at 6 m and 10 m are, therefore, almost identical, given that the random error on the friction velocity is above 10% for a sample duration of 30 min (Kaimal and Finnigan, 1994).

Using data between May 1994 and September 1995, the power spectral densities of the u component with and without corrected friction velocity is displayed in fig. 3. In this figure, the non-dimensional stability parameter is estimated using the anemometer at 10 m. For convective conditions with $\zeta < -0.3$, the uncorrected data shows a more realistic behaviour than the corrected data at low frequencies, where the spectral curves are not expected to collapse onto each other. For near-neutral conditions, the corrected data deviates from the semi-empirical slope in the inertial subrange, marked in red in fig. 3. For stable conditions, the corrected data shows an improvement of the spectral shapes, but the number of samples is relatively low. When the entire dataset (April 1994-July 1995) is used, the velocity spectra normalized with the corrected friction velocity do not show more realistic behaviour than those normalized with the uncorrected friction velocity.

In conclusion, a method to mitigate the influence of the flow distortion on the friction velocity estimate was applied using a multivariate regression analysis. While the flow distortion by the sonic anemometer at 10 m is likely smaller than for the others, the dataset for this sensor was much shorter. The corrected friction velocity did not clearly indicate that the ensemble-averaged normalized spectra were substantially affected by the flow distortion. Flow distortion seems to be mitigated by the fact we averaged samples from an entire sector (220°-330°). For this sector, both an underestimation and overestimation of the friction velocity may be obtained on the omnidirectional sonic anemometers, depending on the wind direction. This could justify the lower-than-expected discrepancies between the uncorrected and corrected averaged spectral flow characteristics.

Tower flow distortion influence on ϕ_m

Q 1.2 *When calculating ϕ_m with measurements from a tower like the one used in Vindeby, you need to take into account the variation with height of the flow distortion caused by the tower (fig. 6 in [2]). There were anemometers on both sides of the mast which enabled modelling of the flow distortion and its influence on the wind profiles [2]. Furthermore it was shown that ϕ_m varies with sea fetch [2], which was also not taken into account in the WES paper:*

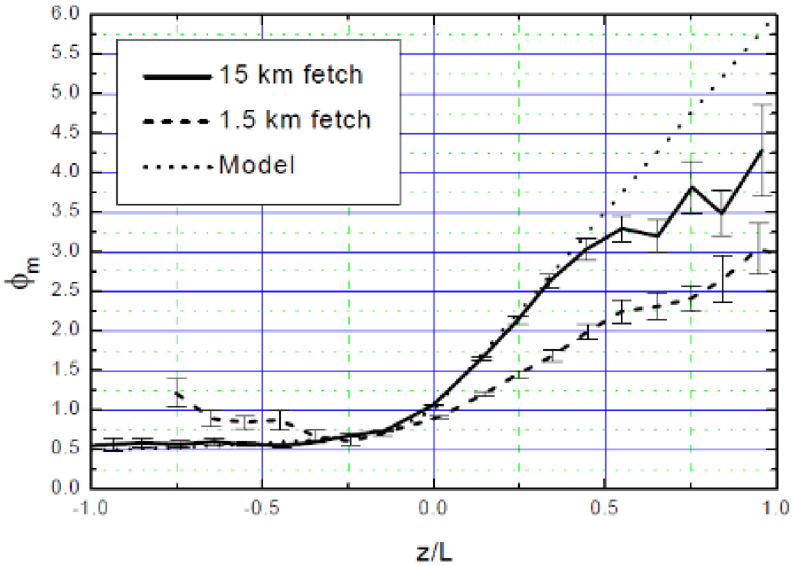

Figure 4: Nondimensional profiles as a function of the stability.

Reply: In the manuscript, we focused on wind direction between 220° and 330° only, such that the fetch was uniform and at least 15 km. Therefore, the sonic anemometers were not affected by tower shadowing. Also, the choice of this sector implies that, for the heights from 6 m and 45 m above the surface, all the sensors are in the internal boundary layer representative of the sea surface. Therefore, the second comment is not applicable in our present study.

Finally, it should be reminded that a fundamental condition to calculate ϕ_m is that there is no discontinuity in surface roughness, i.e. the measurement heights are in the same internal boundary layer. Otherwise, ϕ_m becomes meaningless. In fig. 4, there is a clear indication that for the fetch of 1.5 km, the measurement heights are located in different internal boundary layers. In this regard, ϕ_m does not satisfy MOST. Therefore, the statement from Højstrup, J. (1999) that ϕ_m varies with sea fetch should be interpreted with caution since in this particular case, ϕ_m is actually not applicable.

Spikes in data

Q 1.3 *On page 9 the authors refer to a fairly crude method for removing spikes. Checking for spikes using a much better method [3] was part of the QC routine and the data analysis – and of course, filtering out data with strong precipitation left data with very small amount of spiking (precipitation sensor on mast LM).*

Reply: We thank Dr. Højstrup for the suggestion regarding his algorithm (Hojstrup, 1993), but the conclusion that the despiking approach adopted in the present manuscript is too crude is overhasty. We have, therefore, completed the paragraph on page 9, which now reads as

"The time series were sometimes affected by the outliers. In the present case, outliers were identified using a moving median window based on 5 min window length. The same outlier detection algorithm was also used for the sea surface elevation data, but with a moving window of 180 s. The local median values were then used to compute the median absolute deviation (MAD), as recommended by Leys et al. (2013). Data located more than five MAD away from the median were replaced with NaNs. The generalised extreme Studentized deviate test (Rosner, 1983) was also assessed to detect outliers but did not bring significant improvements. When the number of NaNs in the time series was under 5%, they were replaced using a non-linear interpolation scheme based on the inpainting algorithm by D'Errico (2004) with the "spring" method. A more adequate but slower approach using an autoregressive modelling (Akaike, 1969) was also applied but yielded a similar conclusion and therefore was not used. Time series containing more than 5% of NaNs were dismissed. Although other spike detection and interpolation algorithms exist in the literature (e.g. Hojstrup (1993)), the approach adopted here was found to provide an adequate trade-off between computation time and accuracy."

We clarify under our statement regarding two aspects: (1) the spike detection, and (2) data removal and interpolation. We have evaluated multiple spikes detection algorithms, also called outlier detection algorithms. In particular, we have explored the use of the generalized extreme Studentized deviate test (GESD) as well as the moving median window technique. Both techniques performed equally well but the moving median filter was much faster. So it was adopted. The spike detection technique relies on the median absolute deviation (MAD), which is known to be superior to methods relying on the mean and variance of the time series (Leys et al., 2013). In this regard, the spike detection algorithm by Hojstrup (1993) may be criticized for not relying on the MAD.

When the percentage of detected outliers was under 5%, outliers were removed and interpolated values were used instead. We have explored two interpolation approaches. The first one relies on autoregressive modelling (Akaike, 1969) which is similar to the approach by Hojstrup (1993), although the latter paper does not refer to Akaike (1969). Another approach was explored using the inpainting algorithm by D'Errico (2004). The latter was found to provide acceptable performance compared to the autoregressive model while being considerably faster. Velocity data heavily affected by precipitation are associated with the non-Gaussian distribution or time-variable characteristics which are filtered out in the stationary tests and study of the kurtosis and skewness of the velocity data.

In conclusion, our outlier and peak detection algorithms were compared with more robust and accurate but slower algorithms, which yielded similar results. The algorithm proposed by Dr. Højstrup (Hojstrup, 1993) is interesting but is not fundamentally different or superior to those we have tried.

References

Akaike, H. (1969). Fitting autoregressive models for prediction. *Annals of the institute of Statistical Mathematics*, 21(1):243–247.

D'Errico, J. (2004). inpaint_nans (http://kr. mathworks. com/matlabcentral/fileexchange/4551-inpaint-nans), matlab central file exchange. *Retrieved*, November:2021.

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Højstrup, J. (1999). Vertical extrapolation of offshore wind profiles. In proceedings from EWEA conference in Nice, France.

Kaimal, J. C. and Finnigan, J. J. (1994). *Atmospheric Boundary Layer Flows: Their Structure and Measurement*. Oxford University Press.

Leys, C., Ley, C., Klein, O., Bernard, P., and Licata, L. (2013). Detecting outliers: Do not use standard deviation around the mean, use absolute deviation around the median. *Journal of Experimental Social Psychology*, 49(4):764–766.

Rosner, B. (1983). Percentage points for a generalized ESD many-outlier procedure. *Technometrics*, 25(2):165–172.

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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Answer to reviewers

Dear Sir/Madam,

We thank the reviewers for the feedback on our manuscript. Below is our answer to the reviewer's comment.

Reviewer 2

Reviewer's summary:

The paper "Turbulence in a coastal environment: the case of Vindeby" by Putri et al., provides measurements of turbulence from a field campaign conducted offshore surrounded by complex terrain at Vindeby. The measurements of non-dimensional shear were compared to similarity theory estimates and show reasonably good agreement. Other estimates of co-coherence and turbulence characteristics relevant to offshore wind turbines is also provided. The paper is well structured but there are some aspects of the paper that are not clearly mentioned and could change the result. Reviewing the other comments received for this paper, the two other reviewers have clearly stated some of my concerns as well (regarding flow distortion, computation of the shear, averaging time periods are not clear etc.). In addition to the previous reviewer comments, some additional comments are provided below which would be helpful if clarified in upcoming version. The topic is of interest to the wind energy community and relevant to wind energy science journal.

Overall response:

We have re-explored the dataset with respect to the transducer-flow distortion. As highlighted by Reviewer 1 and Dr. Højstrup, flow distortion can be detected in the sonic anemometer records. However, we also found that when we ensemble-average the turbulence characteristics over the sector of interest (220° to 330°), the bias from flow distortion is reduced, provided that a sufficiently high number of samples is considered.

As suggested by Reviewer 1, we have shortened the section discussing the applicability of MOST because of the uncertainties associated with the transducer-flow distortion. We have also reformulated our findings regarding the influence of the wave height on the turbulence characteristics. The detail concerning the flow distortion is addressed in the reply to the community comment by Dr. Højstrup. In addition, we have created appendices to accommodate some of the supporting information that were detailed in the main body of the original submission.

Comments

Q 2.1 *Maybe some text related to the initial spectral formulations can be moved to an Appendix. There is nothing new in here, but still relevant to the paper and would reduce the length of the paper.*

Reply: We thank the reviewer for the suggestion. We believe that the text related to the spectral formulations is not necessarily be removed to an appendix since it would provide easiness for the reader to understand the results. Furthermore, the length of the text is considerably shortened.

Q 2.2 *Why is friction velocity averaged within the two levels, if you feel the measurements at 6 m are affected by the wave boundary layer? You should revisit this part or provide more justification.*

Reply: We used the average at two levels because the friction velocity is usually the parameter associated with the largest uncertainties and the spatial averaging helps reduce them. We revisited this part and we found that the measurements are, to some extent affected by the transducer-induced flow distortion. In the revised manuscript, we carefully review our statements that the measurements at 6 m are affected by the wave boundary layer.

Q 2.3 *Figure 6 shows good agreement in non-dimensional shear when using 6 m measurements with similarity theory. This is confusing and I was under the impression that the friction velocity and z/L were estimated from 18 m and not 45 m. Some consistency/clarification is required here.*

Reply: We have clarified in the caption of fig. 6 (fig. 5 in the revised manuscript) that z/L was indeed estimated from the measurements at 18 m.

Q 2.4 *With average significant wave heights (Hs) below 1 m (line 64), and the wave boundary layer is typically 5*Hs, so it would mean most of the time the wave boundary layer is below the lowest measurement height (6 m). There may be instances when the waves can affect the measurements, but this would be very small for such low Hs. Please refer to Hristov et al., 1998 (Wave-Coherent Fields in Air Flow over Ocean Waves: Identification of Cooperative Behavior Buried in Turbulence) for more details on how to assess the impact of the wave boundary layer on measurements. After filtering for nonstationary etc., what is average Hs in Figure 5? Small set of measurements affecting the average shear is somewhat surprising. The deviations observed in MOST are probably not due to the presence of the wave boundary layer impacting measurements, but probably flow distortion. This needs to be investigated further.*

Reply: As pointed out by the reviewer, the results in Subsection 5.4 (Subsection 5.2 in the revised submission) indicate that there are only a few instances where we see a clear interaction between the waves and the wind turbulence. The median H_s value was 0.4 m for the period considered, which supports our initial statement that the number of records located in the wave boundary layer is insignificant. We have reformulated the first paragraph of Subsection 5.2, which now reads as " The objective of this subsection is to identify whether the wave-induced turbulence can be detected in the velocity records at 6 m amsl due to the observed turbulence characteristics in Subsection 5.1. Here, the measurements at 6 m amsl are explored in terms of wind-wave interactions, using the wave elevation data collected by the AWR near SMW. A total of 925 high-quality samples collocated in time with the wind velocity data studied were identified. Each wave elevation record was 30 min long and corresponded to a wind direction between 220° and 330°. There exist methods to filter out the wave-induced velocity component from the turbulent velocity component (e.g. Hristov et al. (1998)), but these methods are not addressed herein for brevity."

Following our reply to Reviewer 1 and Dr. Højstrup, we removed fig. 5 from the manuscript as the uncertainties regarding the transducer-induced flow distortion may be too large.

References

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