Answer to reviewers

Dear Sir/Madam,

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

Reviewer 1

Reviewer's summary:

Dear authors,

This new revised version is much shorter and much more concrete than the original one and this is appreciated. I think that, as you mention, your analysis nicely complements that of Fino1. However, as I mentioned in repeated times, the great value of the analysis in my opinion would have been the exploration of the sea interaction on the turbulence characteristics at the lowest sonic or the sonics. You touched the subject but you quickly disregard its importance at Vindeby. I also think that the analysis is not relevant for offshore wind turbines (as you still try to portrait it) as the levels are nowadays far from those of modern turbines; however I think the analysis is very relevant for marine boundary layer and that it surely may benefit meteorological studies over the sea and wind engineering. I would have stressed this instead of connecting it to the turbines per se.

But since you insist on connecting these findings to wind turbines, then I do not understand why you choose to compare the u co-coherence only with the Davenport-like coherence model only (from the IEC standard models). You can only do it for u using Davenport's model, so why not doing the comparison with the Mann coherence from which you can get also the coherence of v and w? The Mann model has the advantage of describe these 3D spatial variations of velocity fluctuations and it is nowadays very much used for load validation on turbines, bridges and structures. I think such a comparison would give the reader comprehension of the need (or not) to derive better turbulence models.

Overall response:

We thank the reviewer for the constructive comments. The main purpose of the present study is to discuss the turbulence spectra from the measurements at Vindeby Wind Farm (at heights ranging from 6 to 45 m above sea level) and to compare them with the measurement at FINO1 (Cheynet et al., 2018) (heights ranging from 40 to 80 m above sea level). This is in line partly with the reviewer's statement "however I think the analysis is very relevant for marine boundary layer and that it surely may benefit meteorological studies over the sea and wind engineering".

We acknowledge that the data at 6 m means a good opportunity to investigate further the wind-wave interactions. We discussed the topic briefly and decided not to disregard the topic. Since the main purpose of this manuscript is to investigate the characteristics of the marine atmospheric boundary layer (MABL) turbulence, we decided not to touch deeper on the topic. Moreover, from the available Vindeby dataset, we did not observe the wind-wave interaction of the magnitude reported and as what was described by Kondo et al. (1972), for instance.

We are fully aware that the Vindeby dataset is located below the recent wind turbines' sizes, but this does not mean that the dataset may not be useful for wind turbine design purposes. In fact, the widely-known Kansas spectra are based on the observations at heights not exceeding 30 m (Kaimal et al., 1972) and have been adopted in the IEC 61400 (IEC 61400-1, 2005). From the Vindeby dataset, we found some similarities with the Kansas spectra for near-neutral conditions. For non-neutral conditions, the turbulence spectra at height 45 m above sea level have a consistent behaviour with the predicted spectra at FINO1 at 41.5 m which do not vary much with the observations at 61.5 m and 81.5 m above sea level (Cheynet et al., 2018). We believe these conclusions from the Vindeby dataset are worthy scientific findings relevant for both the marine boundary layer characterisation and the design of large wind turbines.

Regarding the coherences, the co-coherence of the along-wind component for a near-neutral condition from the Vindeby dataset is predicted most accurately with the modified Bowen model (Cheynet, 2019). It was shown by Cheynet (2019) that the uniform shear model (Mann model) (Mann, 1994) did account for neither the influence of measurement height nor the presence of the surface. These lead to an overestimation of the co-coherence of both the along-wind and the vertical wind components for near-neutral conditions. This is mentioned in the present manuscript in lines 43-44 “Secondly, the vertical coherence of turbulence is not always described accurately by the spectral tensor (Mann, 1994; Cheynet, 2019)”.

Minor comments

Q 1.1 *line 5 remove "(amsl)"*

Reply: The term “(amsl)” is now removed from the manuscript.

Q 1.2 *line 6 remove "to some extent"*

Reply: The term “to some extent” is now removed from the manuscript.

Q 1.3 *line 12 ”predictions from the dataset”: a dataset cannot make predictions*

Reply: The term “predictions from the dataset” is now changed to “results from the dataset”.

Q 1.4 *You need to introduce c1i and c2i afer Eq. 13*

Reply: The two variables are now introduced after Eq. 13 as “where c_1^i and c_2^i are constants”.

Q 1.5 *Line 197 c3i has units so it cannot be just zero*

Reply: The statement “For such structures, assuming $c_3^i \approx 0$ may no longer be appropriate.” does not imply that $c_3^i = 0$. This line emphasises that the co-coherence does not necessarily converge toward unity as the frequency becomes close to zero (for a small separation distance compared to a typical turbulence length scale).

Q 1.6 *Similar to point 5 in line 428 some units are missing*

Reply: Indeed that the c_3^i coefficients in line “The decay coefficients used were, therefore, $[c_1^u, c_2^u, c_3^u] = [6.0, 17.8, 0.02]$ and $[c_1^w, c_2^w, c_3^w] = [2.7, 4.0, 0.16]$ as well as $[c_1^v, c_2^v, c_3^v] = [0, 23.0, 0.09]$” have a unit of an inverse of time. However, the physical interpretation of c_3^i is that the eddy size is limited in the vertical direction, as quoted from Kristensen and Jensen (1979), “If we assume that D is much smaller than a scale of the turbulence L, the exact behavior of the spectrum at wavenumbers $K << 1/L$ is not important since the coherence on these wavenumbers is unity.” (where D is a separation distance). Therefore, the “unit” for c_3^i is not necessarily shown.

Q 1.7 *I think some of the plots, like that in Figure A1 have units in italics*

Reply: The italic units in Fig. 11 and Fig. A1 have now been corrected.

References

Cheynet, E. (2019). Influence of the measurement height on the vertical coherence of natural wind. In *Conference of the Italian Association for Wind Engineering*, pages 207–221.

Cheynet, E., Jakobsen, J., and Reuder, J. (2018). Velocity spectra and coherence estimates in the marine atmospheric boundary layer. *Boundary-Layer Meteorology*, 169(3):429–460.

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Kaimal, J. C., Wyngaard, J. C. J., Izumi, Y., and Coté, O. R. (1972). Spectral characteristics of surface-layer turbulence. *Quarterly Journal of the Royal Meteorological Society*, 98(417):563–589.

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