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:

The revised paper "Turbulence in a coastal environment: the case of Vindeby" by Putri et al. analyzes some turbulence characteristics of the marine boundary layer using measurement data from a met mast at Vindeby, which is a site in the Baltic Sea close to shore in confined waters. The authors compare their findings with an empirical model derived from measurements at the met mast Fino 1, which is an offshore site in the North Sea. In particular, the power spectral densities and the co-coherences are investigated for a large range of atmospheric stabilities.

The topic of the consideration of turbulence characteristics in the design of (offshore) wind turbines is of relevance to the wind energy community, especially as turbines grow in size and for the design of floating turbines. The findings are a valuable contribution to this and are therefore in the scope of the wind energy science journal. Aim to make the paper more focused on its main objective and therefore easier to read and also mitigate some concerns regarding the data analysis and validity of the results.

Reviewing the comments received for the original paper, the replies of the authors to the comments and the revised manuscript, I would like to comment on the readability of the paper, i.e. improving its focus, and on two aspects of the data analysis (flow distortion, computation of the stability measure z/L).

Improving readability / focusing

The paper documents the analysis done in great detail. While this is very positive for the aspects of the analysis which provide new insight, it reduces readability when used for aspects which are already known or not in focus of the paper. This has already been mentioned by reviewer 1 to the original manuscript, it has been improved in the revised manuscript, but in my view should be improved further by further focusing only on the most important aspects. These are well summarized in the well written conclusion at the end. Anything which is not needed to come to this conclusion should be removed from the paper or at least be greatly reduced.

Overall response:

Generally, the reviewer has recommended to shorten the manuscript to focus only to the points included in the conclusion. We are agree with the reviewer and some of the contents are shortened and omitted in the revised manuscript.

Suggestions from the reviewer

Q 1.1 *Chapter 5.1. discusses the applicability of MOST. Figure 5 and the discussion of it is sufficient for this. Figure 6 is not needed – the general distribution of stability classes versus wind speed is known and the number of data points in the stability classes for this particular data set are shown in the graphs later. The discussion of local isotropy and figure 7 are also not needed to conclude the applicability of MOST. They are also not compared to Fino 1 data and not needed for the conclusion or interpretation.*

Reply: We agree with the reviewer to remove Figure 7 and its discussions from the manuscript. On the other hand, Figure 6 is kept in the manuscript, because this figure contains the information of the mean wind speed distribution, which we feel is worthwhile to include for completeness. Furthermore, as suggested by the reviewer, the stability bins are now reduced to 5 only (see Q 1.3), and Figure 6 could demonstrate the relatively poor proportion of the stronger convective cases.

Q 1.2 *Chapter 5.2 is very interesting, but the discussion of the influence of the wave boundary layer is far too detailed for the scope of this paper. The conclusion that the effect is not important here can be drawn with a much shorter discussion of a few sentences. I admit that the analysis and the results are very interesting, but for a paper on turbulence in the context of wind energy aiming to compare turbulence parameters with Fino 1 observations this is out of scope. The authors might want to consider to expand this analysis and write a separate paper about it.*

Reply: Section 5.2 plays a key role to demonstrate that the sensor at 6 m is not located in the wave boundary layer during a significant amount of time. We agree to shorten it, but we cannot, unfortunately, remove it completely. This is contrary to to the opinion of the other reviewer who would like us to extend the discussion on the wave effects. We have therefore tried to streamline this topic whilst keeping the salient points in order to satisfy both reviewers.

Q 1.3 *Figure 10, 11, 12 show results for 9 stability classes. However, the conclusion only distinguishes between near neutral, unstable and stable conditions. Also, given the uncertainty in the calculation of z/L and the small number of samples in some of the classes, I would suggest using a maximum of 5 classes. An increase in the number of classes does not lead to new conclusions but increases the scatter and therefore reduces the clarity of the results.*

Reply: We agree with the reviewer. To avoid the scatter and to increase the clarity of the results, the stability class is now reduced to 5, for $-0.5 \leq \zeta \leq 0.5$ in Figure 10, 11, 12. However, the stronger convective cases are still mentioned briefly in the manuscript.

Comments from the reviewer regarding data analysis

Q 1.4 Flow distortion of the sonic anemometers: *Following the comments by Dr Højstrup and the second reviewer, the authors performed a thorough analysis of the presence of flow distortion in their data set and the possible effect of it on their results. The finding are reported in appendix B. They find a clear and relevant (+/- 20%) effect of transducer flow distortion very much in line with the results provided by Dr. Højstrup in his comment. Following this, the authors developed a correction for this effect and recalculated their results including the correction. Comparing figure 10 and B2, the new results show significant differences which proves the relevance of the correction. However, to my surprise the authors decide not to use the correction. The argumentation in line 209 – 212 seems to be that the error cancels out in the selected wind direction sector. However, this is not shown and is only plausible if the data analysed later are evenly distributed over the wind directions within the sector. It seems very unlikely that this is the case for all of the stability classes analyzed later. Also that "no significant improvement was found for the ensemble-averaged normalized PSD estimates." (line 209) is not a valid argument, since the analysis is done to compare the PSD estimates with the theoretical expectations. The analysis method should therefore not be selected because of its fit to the expectations. Since the error is known, can clearly be found in the data set (figure B1) and can be at least partially corrected for (figure B1), I also do not see any danger of over-processing of the data. Given that the flow distortion clearly has an effect and that this clearly can be reduced by the correction, I recommend to use the corrected data.*

Reply: The differences between Figure 10 and Figure 2.B are significant for stable stratifications or strongly convective conditions only. As suggested by the reviewer, we have removed from the paper the cases where $\zeta > 0.5$ and $\zeta < -0.5$, which are seldom observed in the dataset. For the five remaining stratifications, we confirm that the differences between the corrected and the uncorrected are relatively small.

The reviewer argues that the error on the friction velocity does not cancel out when using sector-averaged values. This statement is contradicted by lines 508-511 in the first version of the revised manuscript. In these lines, we mention that the sector-averaged friction velocity calculated with and without correction differ by 11% or less, which is within the measurement uncertainty. We would like to highlight that the sector selected is relatively narrow (220°-330°), leading to the relatively small difference. In summary, we have decided not to apply the correction for the five selected stability bins since the correction may obscure the results of our analysis.

Q 1.5 Minor comment: *Please check the caption of figure B2, there seems to be a copy/paste mistake from the reply to the comment of Dr. Højstrup.*

Reply: The caption of Figure B2 has now been corrected to "Normalised spectra of the along-wind component on SMW with the corrected friction velocity and five stability bins. The red curve is derived from eq. (5) and N denotes the number of samples considered for ensemble averaging".

Q 1.6 Data processing of the atmospheric stability z/L: *In section 4 the processing of the data for the analysis of the turbulence is well described and a number of quality control measures are taken and explained. However, since for the analysis done later the data are analyzed depending on their stability, a correct assignment of atmospheric stability to each data point is crucial. Selecting the wrong stability class for a data point due to errors or uncertainties in the calculation of the stability parameter z/L can lead to significant changes in the results. However, the processing of the data for the calculation of z/L is not described in chapter 4, there is only a short description in lines 116-122. I suggest to move this paragraph from chapter 3 'Theoretical background' to chapter 4 'Data processing'. Also, the processing steps and especially the quality assurance measures for the fluxes should be discussed. The uncertainty in the calculated z/L data should be investigated, e.g. by comparing the z/L from different heights with each other.*

Reply: Figure 4 already discusses the momentum fluxes between the different sensors. We feel that a further discussion on ζ will be repetitive and unnecessary. It also contradicts the objective to shorten the manuscript. Finally studying the height-dependency of ζ addresses the question of the local similarity theory, which is beyond the scope of the paper.

References

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 2

Reviewer's summary:

I realize that you have performed an extensive revision of the paper. You also made the paper much more concise and short, which is really good. However, this study troubles me because as I think I mentioned in my first review, most of the aspects that you investigate are not really innovative/new. You basically validate the findings from Fino 1 at another site at heights closer to the surface. But since you had measurements much closer to the surface, the innovation (in my opinion) would have been the thorough investigation of the effect of the waves on turbulence. But this later aspect is barely touched. So right now, the study still reads as a technical report and not as a research paper (I am trying to provide with my comments some ideas on how to make it look more like a research paper). My comments are based on the trackchanges version of your revision.

Overall response:

We thank the reviewer for the constructive comments. We agree that having measurement close to the sea surface was indeed a great opportunity to study the wave effects on turbulence. However, there are two constraints that the authors face with regard to expanding this part of the analysis. The first is that the first reviewer would like us to remove this part of the manuscript and mainly focus on the analysis of the wind measurements ("the discussion of the influence of the wave boundary layer is far too detailed for the scope of this paper. I admit that the analysis and the results are very interesting, but for a paper on turbulence in the context of wind energy aiming to compare turbulence parameters with Fino 1 observations, this is out of scope"). The second is that the Sea Mast West (SMW) is located in an enclosed sea with limited fetch and as a result, the highest recorded and the median wave heights are 1.5 m and 0.4 m respectively (based on our available data). Wave heights greater that 0.9 m are available only for 60 samples, which mean that it was difficult to draw significant conclusions based on the data availability. This is the reason why we included this limited analysis as a discussion in the paper to highlight the limited wave effects on wind turbulence at this location. In order to satisfy both reviewers, we have tried to streamline this discussion on wave effects but have retained it, in this paper, to show the limited wave effects observed in this location. Again many thanks to the reviewer for their contributions to improving the manuscript and we hope that we have managed to satisfy both, given the constraints outlined above.

Major comments

Q 2.1 *The new abstract reflects my main issue. There is no clear signs of new findings or important conclusions from the analysis of the measurements. Can the message be changed? Could you say that all IEC turbulence and coherence models are not in agreement with offshore measurements? If so, then what is the impact of suchdisagreement? Are important such differences? Well, for that you will need to also apply the Mann model, so perhaps you could do that as well both for the spectra and coherence.*

Reply: Firstly, the motivation for this study comes from the lack of rigorous turbulence analysis in the marine atmospheric boundary layer. This is considered to be an ongoing challenge for wind energy science, as summarised in e.g. Veers et al. (2019). Secondly, the present study demonstrates how the datasets from Vindeby and FINO1 complement each other, so the study is not limited to just a comparison. Furthermore, additional novel findings in the study are mentioned which are (1) The scalability of the vertical coherence with a model derived from Bowen et al. (1983) is demonstrated in the marine atmospheric boundary layer. This is a major finding for turbulent loading on wind turbines; (2) The velocity spectra studied on FINO1 and Vindeby shows that for design purposes, using site-specific data, advised in the IEC standards, may be conveniently replaced by a more universal spectral model when the site-specific turbulence data are not available; (3) We demonstrated that, even though the measurements are above the wave sublayer during a significant amount of time, the current definition of the wave sublayer depth $h = 5Hs$ is not conservative; (4) The clear variability of the coherence with the atmospheric stability is highlighted, which calls for a new methodology for the fatigue life design of offshore wind turbine (5) The potential applicability of Klipp's method for diabatic conditions. This is a minor finding for wind energy by more significant for atmospheric science.

The IEC turbulence models are applicable for the ultimate limit state of wind turbines only, where the stratification of the atmosphere is assumed to be neutral. Stating that the IEC turbulence and coherence models are or are not in agreement with offshore measurements would be an oversimplification. The question of the applicability of the IEC turbulence models in the marine atmospheric boundary layer has already been addressed in several studies, including Cheynet et al. (2018) for the coherence and Cheynet et al. (2017) for the one-point spectra. Therefore, the IEC turbulence models are not the main focus of the present study, as they would overlap too much with previous studies.

Q 2.2 *The local isotropy quest. Around line 350 you are evaluating local isotropy. First you mention that the references you provide show that the Sv/Su ratio reaches isotropy easier than Sw/Su. This is not true. With or without correction the Sv/Su ratio is close to 4/3, whereas Sw/Su is never 4/3 unless properly correcting for distortion. So when you say Sw/Su 4/3 ratio is only reached for the 6 m, it is natural to think that the other two sonics need to be corrected. You also say that at 18 m Sw/Su converges towards 1.2 in near-neutral conditions, which is not really shown in the figure (yes the values might reach 1.2 but convergence is perhaps not the right term here). So the message, I think, is not that you need to try to find out whether the u* estimates are good or wrong (as you now do in an appendix) but to try to get the best time series of your velocity fluctuations as you used these to compute the spectra and coherence as the main results of your analysis (independently whether u* is good or bad).*

Reply: The discussion concerning the local isotropy is now omitted from the manuscript, as suggested by Reviewer 1. The answer to this comment has partly been covered in our initial reply to the reviewer in Q.1.5. In Fig 7, the Sw/Su ratio at 18 m is equal to or above 1.20 for reduced frequency above 5. This can be easily verified using digitalization software or the free online tool WebPlotDigitizer. It should be noted that the ratio 4/3 may not be reached, even without flow distortion, see. e.g. Chamecki and Dias (2004). So the quest for local isotropy is not trivial. The relevancy of studying u_* is addressed in our reply to Dr Højstrup as well as in his short comment.

Q 2.3 *And so then the question is why you kind of reach the value of 4/3 for Sw/Su for the 6 m sonic only, although you have not corrected this? After the coherence analysis you did between the wave velocity and the vertical velocity of the sonic at 6 m you conclude that the waves have a limited impact on the 6-m sonic. So the question is, what would be then the result of this analysis if you have corrected the velocity time series for flow distortion?*

Reply: As elaborated in our previous reply to the reviewers, a direct correction of the vertical velocity component needs to rely on wind tunnel tests. The correction discussed in the appendix is on the friction velocity. Following the approach used by Peña et al. (2019), we could argue that if the ratio

S_w/S_u reaches 1.3 in the inertial subrange, no correction would be needed. The short comment by Dr Højstrup suggested that using the ratio alone was not sufficient to guarantee that flow distortion is negligible. Following the suggestions by Reviewer 1, Figure 7 and the associated discussion are now removed from the manuscript (see Q 2.1).

Q 2.4 *The comparison of measurements in Figs. 10-12 with the model by Cheynet et al is really good and it seems one the most important findings. Why is not the model introduced/explained? It seems to be stability dependent so where and how is stability accounted for in the model?*

Reply: The description of the model used in Cheynet et al. (2018) would lengthen the manuscript unnecessarily. Therefore, we decided to be more concise and to simply refer to the paper, the post-print of which is publicly available online. The model is indeed stability dependent, where it relies on empirical parameters that were established on FINO1.

Minor comments

Q 2.1 *Line 3 delete "(sea . . . MW)"*

Reply: We agree with the reviewer. The following "(Sea Mast West/SMW)" is now removed from the manuscript.

Q 2.2 *Line 6 delete "above. . . amsl)"*

Reply: "above mean sea level (amsl)" carries an important information for the whole sentence, therefore it is not deleted.

Q 2.3 *Line 11 delete "to some extend"*

Reply: Because the flow distortion affects the anemometers up to a certain degree only, therefore the term "to some extent" is not deleted.

Q 2.4 *Line 12 replace "spectrum" to "spectra"*

Reply: We agree with the reviewer. The term is "spectrum" is now replaced to "spectra".

Q 2.5 *Line 13 delete "(z/L. . . surface)"*

Reply: We agree with the reviewer. The term is now deleted.

Q 2.6 *Line 18 "predictions from FINO1": FINO1 cannot make predictions; predictions are made by models for example.*

Reply: We agree with the reviewer. The line is now revised to "The co-coherence of the along-wind component, estimated for vertical separations under near-neutral conditions matches remarkably well with the predictions from the dataset at the FINO1 platform."

Q 2.7 *Lines 20 and 23 dataset or data set. . . use one convention for all entries*

Reply: The term "dataset" is selected for the entire manuscript.

Q 2.8 *Lines 25-26 delete them*

Reply: The lines are not deleted. We believe that these lines are essential to be kept at the end of the abstract. To improve clarity, the lines are smoothen as “Yet, the dataset recorded at Vindeby and FINO1 covers only the lower part of the rotor of state-of-the-art offshore wind turbines. Further improvements in the characterisation of atmospheric turbulence for wind turbine design will require measurements at heights above 100m amsl”.

Q 2.9 *Line 39 “non-neutral… than on land”: on-land non-neutral conditions are more common, so quite the opposite*

Reply: We confirm that non-neutral conditions are likely to be more common above the ocean than on land. For a given mean wind speed at a reference height, the higher roughness on land implies that mechanically-generated turbulence is greater than offshore, leading to a greater Obukhov length. The predominance of diabatic conditions in the MABL has been documented in e.g.Archer et al. (2016). To provide a more accurate information, this line has now been reformulated as “The characteristics of the MABL differ from the overland atmospheric boundary layer (ABL) due to the large proportion of non-neutral atmospheric stability conditions (Barthelmie, 1999; Archer et al., 2016) and low roughness lengths.”

Q 2.10 *Lines 54-55 “low-frequency fluctuations…. convective conditions”. This is not really true. All microscale turbulence models underestimate the low-frequency fluctuations in unstable conditions as they do not account for mesoscale fluctuations. Other models attempt to account for this extending the microscale range they used to cover.*

Reply: We agree that there is indeed a strong interaction between mesoscale and microscale atmospheric motion under convective conditions. However, in the present case, the duration of the time series selected is 30 min. Under convective conditions, this duration is likely too short to include mesoscale fluctuations. See for example the study by Smedman-Högström and Högström (1975) which focuses on the time-scale associated with the spectral gap. The reason why microscale models underestimate low-frequency fluctuations in unstable conditions offshore has been discussed in the past. The uniform shear model is not designed for convective conditions, so larger eddies due to buoyancy-enhanced turbulence are not accounted for, as discussed in Chougule et al. (2018). Spectral models designed for unstable conditions are a rarity. They are based on the Minnesota experiment, where the low-frequency fluctuations were high-pass filtered (Kaimal et al., 1976; Drobinski et al., 2004). Finally, many spectral models are based on onshore measurements whereas offshore measurements are known to be associated with greater low-frequency turbulent fluctuations, so onshore models cannot be directly applied to offshore conditions.

Q 2.11 *Line 59 delete “in the frequency space”*

Reply: Keeping the term “in the frequency space” is crucial here as it directly implies that we do not focus on integral turbulence characteristics but spectral characteristics. Therefore, we decided not to remove this term.

Q 2.12 *Line 70 replace “from the established” by “, which are based”*

Reply: We agree with the reviewer. The term is now replaced.

Q 2.13 *Fig 1-right maybe add a line in the limits of the sector you describe in line 151; and maybe add the distance to Langeland from the mast*

Reply: The sector limit is now added in the caption of Figure 1: "Only the wind from 220° to 330° is considered for SMW". The distance of SMW mast to the land is now added in the caption of Figure 2: "SMW is located approximately 1.5 km from Lolland".

***Q* 2.14** *Line 190 around this line, the reader got to know that Davenport's model is applicable to vertical separations, but suddenly Kristiansen and Sacre & Delaunay questioned its applicability to lateral separations? Why they use a model for vertical separations then?*

Reply: The sentence mentions Davenport's similarity, not the Davenport model. The Davenport model is the exponential coherence model. The Davenport similarity is the assumption that the coherence (a 2D function) can be reduced to a 1D function by using a non-dimensional frequency $fd/\overline{u}$ where d is a distance.

***Q* 2.15** *Line 199 "This implies that fitting. . . .wind turbines" well this depends on whether you believe more in Bowen than in Davenport and until this point of the manuscript you have not given arguments to believe so*

Reply: The argument is elaborated in Cheynet (2019), where the influence of the coherence model on the modal wind loading is explored. We have added a reference to Cheynet (2019) at the end of this sentence.

***Q* 2.16** *Line 208 "Kristiansen & Jensen (1979). . . 1/T" so K&J had a similar model to the Bowen model. . . I mean they seemed to already have given an expression for c3i*

Reply: Kristensen and Jensen (1979) discuss only the limits of the Davenport model in terms of turbulence length scales. The coefficient c_3^i was introduced in Cheynet (2019) to modify the Bowen model, based on the discussion from Kristensen and Jensen (1979).

***Q* 2.17** *Lines 246-247 This statement is debatable and you only had a reference to support it*

Reply: The statement contains new information, and the one reference we were referring to is a study containing a well-known method. The reference we used to support this statement is the second most cited paper in Boundary-Layer Meteorology since this journal was established in 1970s.

***Q* 2.18** *Lines 331-332 "It should be noted. . . 18 m amsl": well, yes, the conditions will appear closer to neutral conditions as the friction velocity at 6 m is higher than at 18 m.*

Reply: In the present case, the variability of the friction velocity with the height (cf. Fig 4 in the manuscript) is not sufficient to explain the variability of ζ with the height. The Obukhov length seems to fluctuate relatively little between 6 m and 18 m, whereas the height is three times lower at 6 m than at 18 m. Therefore, $\zeta = z/L$ is much smaller at 6 m than 18 m. The statement that u_* is much larger at 6 m than at 18 m was part of the original manuscript but was discussed in our reply to Dr Højstrup.

***Q* 2.19** *Line 133 and Fig. 5: you do not mention how du/dz is estimated in the computation of ψ_m. I think you still need to use a polynomial in the logarithmic of the height to better compute the local gradient at the given height where your z/L value is derived from. What about using the polynomial form of Hoegstroem (1988)? The left and right panels show that du/dz should be computed at the height where z/L is computed. In the text you say that in the right panel you use the friction velocity from 45 m but in the caption you say that z/L is at 18 m.*

Reply: We have addressed this comment in a thorough detail in our reply to the reviewer for the original manuscript (Q.1.6).

Q 2.20 *Lines 482-496: I recommend to delete all these lines, they are expendable.*

Reply: We believe, on the contrary, that these comments are valuable to illustrate the variability of the spectral gap with the thermal stratification of the atmosphere. These lines directly address the challenge of the flow modelling under a stable stratification, which is relevant for floating offshore wind turbines with low-frequency modes of vibrations.

Q 2.21 *When presenting Fig. 13 you do not talk about the w co-coherence results. There, the Fino 1 model performs quite badly with the three combinations you present*

Reply: We agree with the reviewer. Additional lines are added in the manuscript "The vertical co-coherence observed from SMW shows deviates substantially from the one fitted to observations at the FINO platform. The source of such deviations remains unclear."

Q 2.22 *Appendix A: the angle in 629 is between the stresses only (you say stress and wind vector). What will happen if w in particular needs to be corrected?*

Reply: The correction used in the present study relies on a multivariate regression for the covariance terms, which are used both on the nominator and denominator. So, in that case, the angle value is unlikely to change significantly. Also, these angles are calculated based on sector-averaged covariance terms, which were observed to differ by less than 11% from the corrected values. A simple sensitivity study can show that using angular value without decimal (as we did) is adequate for the present study.

Q 2.23 *Appendix B: you test your "correction" by looking at the u-spectrum, but the corrections affect the w fluctuations mostly, so I do not understand your idea.*

Reply: As elaborated in Appendix B, the correction is for the friction velocity because a direct correction of the w component would require specific wind tunnel tests. Since the friction velocity is used as scaling velocity, it affects every velocity component. In appendix B it is also mentioned that in the present study, using sector-averaging (220°-330°) helps reduce the influence of flow distortion on the spectral flow characteristics.

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