Review

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. Some suggestions:

- 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.
- 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.
- 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.

Comments to the data analysis

1. *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.

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.

2. *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.