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Comment on wes-2021-75

Anonymous Referee #1

Referee comment on "Turbulence in a coastal environment: the case of Vindeby" by Rieska Mawarni Putri et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2021-75-RC1, 2021

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.

Major comments

- 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.
- 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”
- 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?

- 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 analyzing 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
- 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!
- 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 \phi_m at this heights are not following MOST
- 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.

Minor comments

- 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"

- Line 6: replace "empirical spectra established on" by "that from"
- Line 9: Replace "with those at" by "that at"
- Line 37: Replace "are justified for" by "relate to"
- 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)
- Lines 48 and 50 and maybe other instances: be consistent so it is either heights, levels or altitudes (first is preferable)
- 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
- 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 DEM shows 0 m for areas that are not water areas
- Line 65 and 84 delete "of" after "comprised"
- Section 2: I do not think you mention what kind of cups and vanes you have and the heights where they measure
- Line 82: delete "the wind"
- Line 94: add "as" after "denoted"
- 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)
- 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)
- Line 110: add "vertical" before "flux"
- Line 113: why is \theta_v not reliably measured by a sonic?
- Line 121: \phi_w is not commonly used to assess MOST. Perhaps \phi_m and \phi_temp
- Line 135: the spectrum is not a quantity. Anyway, the whole paragraph between lines 135 and 138 is not needed
- 8: remove the 2/3 as exponent of \phi_\epsilon
- 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
- 15 is the cross a dot product?
- Line 197: so is data plural or singular?
- 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.
- 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
- 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)
- Line 241: delete "which testified"
- 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)
- 4: friction velocity "computations" or "calculations" not "estimations". Also the dimensionless stability in the legend appears with units of m
- 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

- 7: units in m/s or in m s^-1?
- Line 285: the sentence does not makes sense as the 18 m value is a local value
- 5.3 is not needed and can be removed without endangering the study (see major comment 1)
- Line 340: did you introduce the quad-coherence before? I mean you do introduce the Co-coherence
- Line 378: didn't you already introduce the reduced frequency?
- Line 379: I guess you need to delete the 2/3
- 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
- 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?
- 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
- Sentences between lines 387 and 391 can be removed without detriment
- 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)
- 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
- 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
- Fig 12 and similar: delete the "for references… in the data" of the caption.
- Line 423: is the -2> not a -2<?
- Line 433: delete "since we aim… Vindeby"
- Line 435: did you omit the value of C3v?
- 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
- Caption Fig. 15: change "empirical values computed" by "predictions"
- Line 447: "lateral co-coherence is also required"…. For what?
- Lines 448-450 can be deleted without detriment
- Lines 454-455: do people use vertical coherence models for aeroeslatic turbine simulations when not using the Mann model?
- Line 459: replace "The first one is related to the fact that the" by "The"
- Line 460: why nonstationary time series are not reliable?
- Lines 466-467 Delete "Therefore,… factor"
- Line 469 what does invariant here mean?
- Lines 471-477: these lines are not needed
- In the conclusions you again start to introduce acronyms; this is not needed
- Line 482: "relevant for the design of offshore wind turbines"… this is not true (major comment 4). Similar issue in line 504
- Line 501-502: well the 45 and 18 m are not that close to the surface