Wind Energ. Sci. Discuss., referee comment RC1
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Comment on wes-2021-161

Anonymous Referee #1

Referee comment on "Classification and properties of non-idealized coastal wind profiles – an observational study" by Christoffer Hallgren et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2021-161-RC1, 2022

Review of the manuscript wes-2021-161

Classification and properties of coastal wind profiles with negative gradients – an observational study

By Hallgren et al.

Summary

This paper uses 3.5 years of LIDAR and tower observations in the Baltic Sea to develop a climatology of characteristic wind speed profiles, with a special focus on wind profiles with negative shear since these are difficult to forecast in wind energy production assessments as well as for load assessments. The profiles are categorized according to wind speed profile types and atmospheric stability and swell/sea state. The paper is overall well written, though I find the introduction more rigorous than the remainder of the paper. The quality of the figures is overall good. Overall I find the dataset and analysis of high value for field, but there are some imbalances in the manuscript that needs major revisions before it can be accepted for publication in WES.

Recommendation: Major revisions required

Major remarks

- My first main concern is that the paper does not bring what its introduction tends to promise. The introduction talks rather thoroughly about theories behind anomalous spectra in wind profiles like shear sheltering due to "separated" layers etc. However, in the analysis of the dataset in the results section, the link to the theories is missing. The introduction raises the expectations that apart from the climatology of wind profiles that is promised and presented, it will also offer an explanation using these theories. Especially since the introduction concludes that there are a number of controversies about when anomalous spectra occur in which type of wind speed profiles. But that does not happen in the end. So I recommend to deepen the analysis by linking the anomalous profiles to the spectra and link them to the existing theories.
- Spectra: Concerning spectra I think the paper needs to justify more strongly why it needs the selected spectra to answer the research questions. I.e. why would for example wavelets not have given you a better of different (or similar answer). Secondly the manuscript needs to justify more whether the low frequency part of the spectrum in stable conditions (the ones you are interested in) are not affected or dominated by effects from atmospheric waves. The spectra show the traditional peak of turbulence, but then left of it there is a dip and then it increases again. This is typical for wave motions and their behaviour or impact (quantitative, time and height) on the wind speed profiles are likely different than by turbulence. See Enaudi and Finnigan papers from the 1980s how to separate waves from turbulence.
- I find the discussion section of the manuscript needs to be deepened. You correctly point out what are some of the weaker points of your analysis, or components that could not have been taken into account. But the discussion should go deeper about mainly a) what does these deficiencies concretely mean for the conclusions of the manuscript and b) how do the results compare to other studies, i.e. in other words please elaborate how this study has brought science a step forward. Some of the comments in the comments in the discussion section repeat what was already mentioned in the Introduction, but they remain disconnected from your own analysis and dataset.
- The paper discusses the wind speed profiles and turbulent spectra from a rather local point of view. To what extent can we assume the turbulence is homogeneous? I can imagine that sea surface temperature is spatially different across a gradient from north to south (and perhaps also east-west) which means the (in)stability is also spatially varying, which in turn affects the turbulence intensity. Hence one should be aware that part of the variances can have been advection from upwind. How would that affect your

analysis. Can the analysis be extended by adding the SST (from ERA5 or any other data source) as a classifier?

- Figures: The polar plots are nice but at the same time there are many datapoints plotted over each other with different colors, so it is a bit difficult to see what is the density of these categories of data points in the busiest polar plots. Are there any alternatives for plotting?

Minor remarks:

Ln 3: ....observational study...

Ln 5: non-normal wind profiles: unclear how it is defined. Do you mean wind profiles with negative shear only or do you mean something broader? Or just if the profile does not fulfill the logarithmic approach?

Ln 51: shear sheltering. I think it is good to add a sentence or two explain what shear sheltering is. Just to make the manuscript attractive for a broader audience. Or refer forward that a more theoretical explanation will follow below.

Ln 115: the sensible heat flux. Do you mean surface sensible heat flux here? Or the sensible heat flux near the LLJ? Please clarify in the text.

Figure 1: Perhaps add a few labels where countries like Finland, Sweden, Estonia etc are located. This may help the non-European reader to understand better where you study was done.

Ln 155,156: This is a paragraph of one sentence, please merge with another paragraph.

Ln 157: with an undisturbed fetch of at least 150 km. Please add some words about the possibility that parts of the Baltic Sea can become frozen in winters. Did that happen in the years covering your dataset and within the footprint of the site (i.e. the 150 km you mention).

Ln 210: could you add which percent of the time the wind speed was below 3 and 1 m/s

respectively?

Ln 235: 30 minute averaging window. Can you elaborate a bit about what are the consequences of the relatively large averaging time for your results. If 5-min or 10-min averages would be used (so less averaging), one may catch more anomalous wind profiles than when one averages for 30-min. So are your results and conclusions conservative estimates?

Ln 266: This is a paragraph of one sentence, please merge with another paragraph.

Ln 268: Can you elaborate how much your results and conclusions are affected by the LLJ criterion? The manuscript is rather brief about it here.

Ln 277: This is a paragraph of one sentence, please merge with another paragraph.

Ln 279: This is a paragraph of one sentence, please merge with another paragraph. Make more coherent please.

Ln 290: why does g deviate from the traditional value of 9.81?

Section 3.6: The paper needs to elaborate more on how robust the findings are concerning their dependency on the atmospheric stability. The stability categorization is somehow arbitrary and other classification could have been chosen as well, i.e. in terms of Richardson number, or Pasquill classes and others. This need to be addressed in more depth. L becomes undefined when turbulence vanishes.

Equation 5: Why is the Su spectrum not scaled with the variance of u? I.e. similar as for Sw. The paper needs to discuss why this normalization is suitable for the goal you want to achieve with the study. I would say the Su scaling as done now is more sensitive to measurement uncertainties than just scaling with sigma_u^2.

Ln 320: This is a paragraph of one sentence, please merge with another paragraph.

Figure 3: the description of this graph does not explain whether and how the wind roses or climatology are consistent with earlier studies over the site or over the Baltic. Please bring in context.

Figure 4: caption: panel c does not show the normal type, while the caption suggests it is present. Please adjust the caption.

Ln 358: goes to zero -> "vanish"

Ln 363: with a peak value of approximately 40% in May and June. Please add some words about the physical reasons why this peaks in May-June. I.e. interpret the climatological values.

Figure 6: Please extend the legend and make explicit what H, S, WS, ...U represent. It can be helpful to add the season labelling at the top of the rows of figures and the LLJ strength along the rows, such that the reader does not need to read the figure caption 5 times to unravel what is in all these plots.

Ln 403-404: naturally, the higher the LLJ core is located, the higher the core speed. I would say this is not so natural. If you look in the Van de Wiel paper that you refer to in the Introduction you will see that the largest amplitude of the inertial oscillation occurs near the ground and as such the highest LLJ magnitude is expected not to be at higher levels. Please revise.

Section 4.4 as a whole: I recommend to make this section more quantitative in its discussion. The reader now has to do the analysis of figure 6 by her/himself and this may introduce different conclusions than what the authors intend to say.

Ln 419: Negative profiles (Fig. 7) predominantly occurred when the air was advected from the open sea sector and the conditions were unstable or weakly unstable. Can you provide a physical explanation for this. Exactly in unstable conditions I would expect that the anomalous profiles would be mixed away by rather rigorous turbulence. Did you mean the negative profiles are related to the swell?

Ln 420: local stratification. This is confusing for the reader. How do you define stability in this study? Is there also a non-local stratification or spatial evaluation of the stratification involved? I thought it was just based on the measured L on the site.

Ln 424: it can be seen. Please avoid passive sentences.

Ln 435, 436: is the difference systematic, i.e. can you support evidence with results from a statistical test?

Ln 445,446,448: significant difference. If you make the statement it is significantly different then one should also add the results from an appropriate statistical test.

Ln 445,446,448: you mention that there were substantial differences between the spectra taken in the LLJ profiles and the normal profiles. But you do not describe how they differ (higher, lower, in which spectral ranges etc). Please expand.

Ln 466: in this respect I think it is better to refer to Kalverla et al (2018, https://onlinelibrary.wiley.com/doi/10.1002/we.2267) here since they focus on the evaluation of NWP models over sea (North Sea).