Dear Referee #2,

Thank you for your review of our manuscript and helpful comments! We have reviewed the manuscript according to your comments and corrected the manuscript point by point.

<u>General Comments</u>

This study presents multi-year datasets combined to address differences in the wind profile characteristics throughout the year as a function of sea state, stability, season, and turbulence (via spectral analysis of inertial subrange and the encompassing scales). The different approaches toward conditioning the data made for an interesting read. The results were well discussed; however, there are areas that need further explanation and details before formal acceptance can be made. I would vote acceptance with minor reviews. The specific comments below are mostly related to the science within the paper and areas where additional explanation is needed. There were a lot of grammar/typo issues, and the reviewer likely missed many throughout the paper. It is suggested that the authors use the examples below in the technical corrections section to improve other areas of the paper beginning at the results section and continuing to the end.

<u>Specific Comments</u>

1. The authors investigate a multi-year dataset comprised of a tower, buoy, and a continuous wave lidar. Given the distribution of wind profiles and the discussion of "normal" v. "non-normal" profiles, where the latter includes profile types resembling LLJs as well as negative gradients, then it is strongly recommended that the title is adjusted so that it would not seem that the authors are specifically focusing on wind profiles of negative gradients. Afterall, that's only part of the story

In accordance with your Specific Comment 2 below, we have changed the title to *Classification and properties of non-idealized coastal wind profiles – an observational study*.

2. Comment of usage of "normal" v. "non-normal": I would argue that using "normal" v. "non-normal" to describe wind profiles is related to the region of interest. Certain regions, for example the great plains, experience LLJs during a fairly large proportion of days throughout the year. The same can be said with the great barrier jet off the coast of California. I would instead recommend the authors change "normal" to "idealized" and "non-normal" to "non-idealized" to describe departures from the ideal planetary boundary layer (PBL) model and the anticipated wind profile structure therein.

Throughout the manuscript we have changed "normal" to "idealized" and "non-normal" to "non-idealized".

3. Your section 3 is entitled "Methods" and yet it goes over the sites and the measurements used in addition to methods. I would change the title to reflect this. Something like "Site, Measurements, and Methods"

Thank you for this comment. We have changed the title of Sect. 3 to Site, Measurements, and Methods.

4. Lines 161-164: I don't quite understand this sentence fully. Are you also saying the buoy can measure turbidity? No results related to ocean turbidity is reported in your study. You do mention that wave properties have been evaluated against the turbulent state of the lower-atmosphere, but that is by making comparisons with the mast data. The sentence is confusing in other ways, too. Please improve your point here. We have clarified this sentence, which now reads (line 158-159):

Only occasions when mast, lidar and wave buoy observations were simultaneously available were used in the analysis.

5. Line 218: It's important to be sure that the linear interpolation is done over small time gaps. You probably should make that clear in the paper. Plus, I doubt that linear interpolation was done over large data gaps. You probably want to comment on any impact this could have on the results and what your gap threshold is. There are ways to approach this statistically as well if gaps are numerous and you have periods of similar turbulence intensity – with and without data gaps – then you can compare those data and apply statistical randomization such as what is used when solving for the p-values.

We agree that this is an important point. Lines 181-191 now reads:

By selection criterias from Vitale et al. (2020) we assure to include in our analysis only half-hours when the longest duration of gaps are less than 3 minutes and further half-hours are selected to always contain more than 85% data coverage following the SevEr thresholds suggested by Vitale et al. (2020). In practice due to additional criteria the half-hour with lowest data availability included in our analysis was 92.8% (corresponding to a total of 130 s of 20 Hz data missing in the 30 min averaging period). However, only for 0.02% of the time (ten half-hours in total), the data availability was lower than 99%. Additionally, we used the homogeneity test of fluctuations and differenced data based on Chebyshev's inequality theorem in combination with the SevEr thresholds as suggested by Vitale et al. (2020) to avoid cases of large aberrant structural changes (e.g. sudden shifts in the mean value or changes in variance) which could lead to violation of the assumption of stationarity (Vitale et al. 2020). The longest gap duration within the high-frequency time series was typically short and only 1 half-hour had more than 12 consecutive data points missing in any of our selected 20 Hz time series for wind components and sonic temperature.

6. Lines 345-347: It's hard to tell that the reason the wave age is higher is because of lower wind speeds. Clearly that could be born out the formulation that is used, but it is not shown clearly enough in Figure 3. The wind distribution over open seas is smaller during DJFM and the winds appear comparable when visually comparing the open sea sector distribution from DJFM with AMJJ. If you go ahead and multiply your wave age by U, then you should be able to compare the phase speeds between these periods to support this claim. Note your Figure 5 showing wind speed distributions and sea state. The AMJJ shows a much higher percentage of swell and more negative profiles, which weighs down the distribution toward lower wind speeds. The distribution, of course, can cannot easily be used to infer the wind rose distribution for open sector in figure 3.

Thank you for this comment. In Fig. AC2.1 the normalised distributions of phase speed of the dominant waves in the three seasons DJFM, AMJJ and ASON are plotted. Fig. 3 in the manuscript indicates that the wave age (defined as c_p/U) is generally higher in AMJJ than in the other seasons and this is despite the fact that the phase speed is generally lower in this season (Fig. AC2.1). Thus we conclude that the higher wave age in AMJJ is due to the lower wind speeds. We have clarified this statement in the manuscript (lines 370-373):

In Fig. 3 the median wave age and the 25 and 75 percentiles of the wave age are presented for the open sea sector, showing slightly higher wave age on average during AMJJ. This is attributed to the typically lower wind speeds compared to the fall and winter seasons and is despite the fact that the phase speed of the dominant waves is in general lower in AMJJ than in other seasons (see supplement, Fig. S2).

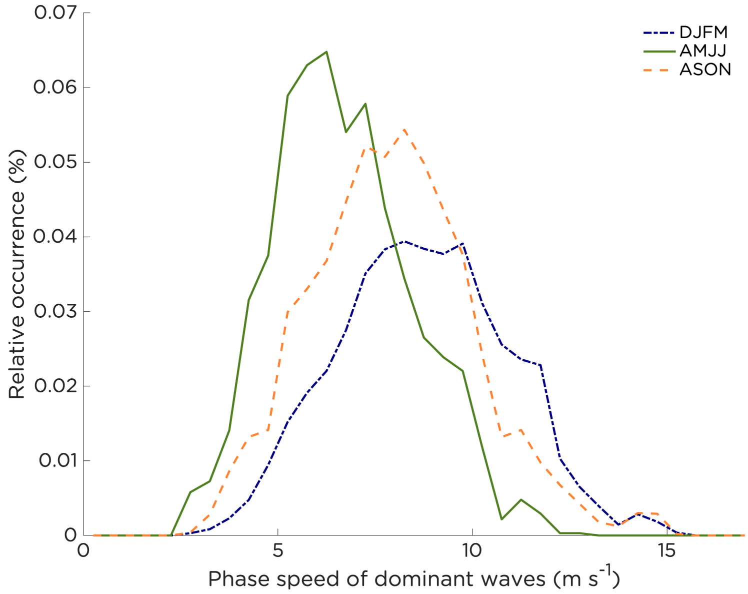

Figure AC2.1: Normalized distributions of phase speed of the dominant waves in the three seasons DJFM, AMJJ and ASON when the wind is from the open sea sector. Bin size: 0.5 m s^{-1}.

7. Lines 406-409: The mechanisms behind the LLJ were not determined in this paper which makes determining why LLJ-type profiles existed during different stability regimes and for different sectors difficult. The Gotland sector has more LLJs for neutral-to-unstable conditions while the open sea has more LLJs during stable conditions. The site that this data was collected is small, so it is likely to impact the vertical structure of the LLJ over the island but not cause it. Another tricky part of this analysis is differentiating between what the local stability is over land compared to the stability over the ocean. The air being advected over the land site would also impact stability, too, in addition to surface heating/cooling, and this relative impact on stability is not easily characterized with the Monin-Obukhov framework.

We agree with all these points. Although different mechanisms behind LLJ formation are discussed in the manuscript it is difficult to determine the cause for every single case as many of the mechanisms might interact, especially at this coastal site where local effects from surrounding land areas have an impact on vertical structure of the wind field. In the new version of the Discussion we have made an attempt to better explain the different causes for LLJ formation and the impact that the coastal zone has on the stability measured at the site.

8. Spectral Analysis: I know that the focus is mostly the inertial subrange and and frequency from which we depart from the inertial subrange into larger eddies. I agree with much of the discussion related to the spectral analysis, but I see that almost all "non-normal" profiles have lower energy containing eddies at the low frequency end. My guess is that one cause stems from dealing with much smaller sample sizes with the "non-normal" cases. It is interesting to note that only the negative profiles have examples where the largest energy containing eddies are larger than the "normal" profiles despite having the smallest wind shear and lightest wind conditions. Both Figure 8 and 9 show this. The production of turbulence is typically rooted in buoyancy or wind shear. Wind shear is small for the negative shear profiles so the thought is that buoyancy production should be responsible. However, the spectral energy is larger for stable conditions as well (Figure 9). Island meteorology and boundary layer internal advection is difficult. Can this be discussed or addressed in the paper?

This is an important note and a complicated matter, which – as you also note – is partly outside the scope of our manuscript. We have added the following to the manuscript (lines 585-590):

As seen for the stable case in Fig. 8, there was a clear spectral gap separating the turbulent part of the spectra from larger scale non-turbulent motions such as atmospheric waves. While it could be argued that these low frequencies should be filtered out from the analysis (see e.g. Finnigan and Einaudi 1981), we decided to perform the analysis in a similar manner as e.g. Smedman et al. (2004), keeping all the turbulence data and analyse changes in the observed turbulence spectra, no matter if atmospheric waves were present or not. It can also be noted in Fig. 8 that the increase in spectral energy at the lowest frequencies was present not only for the case of an idealized wind profile, but also under non-idealized wind profile conditions.

9. Line 538-539: There are studies that look into turbine wake characteristics as a function of stability. Look at these such examples: Zhan et. al. (2020) and Iungo et. al. (2014)

Thank you for this comment. We have added the suggested references to the paragraph discussing wake characteristics under different atmospheric stabilities which now reads (lines 692-696):

Wind turbine wake behavior under different stabilities and for different wind speeds has been studied for both onshore (e.g. Iungo and Porté-Agel 2014; Zhan et al. 2020) and offshore wind farms (e.g. Platis et al. 2022) but more research is needed to accurately describe the wind resource within wind parks and how power production can be optimized using wake steering. The extent of the wake behind a wind turbine is likely to be very sensitive to LLJ conditions (e.g. Vollmer et al. 2017) as the potential to mix down momentum from above the jet core is greatly diminished.

<u>Technical Corrections</u> **(T and G will denote typos (T) v. grammar (G); grammatical suggestions will be marked by S)**

We thank you for taking the time to comment on all these technical corrections. All of the suggested changes have been implemented throughout the manuscript and based on comments from Referee #1 also other changes in the manuscript have been implemented, An overall oversight for grammar and typos have been conducted and we are ready to implement any additional adjustments required at this or later stages in the review process as is found necessary.

T1 (Line 8). Change "...that the the zone with strong shear during low-level..." to "...that the strong shear zone of low-level..."

S1 (Lines 15-16). Change "...This variation, the wind shear, plays a..." to "The vertical structure of the wind profile (i.e., wind shear and wind veer) plays an...." With this statement you could lump the two sentences mentioning wind shear and veer together

G1 (Line 22). Change "...grow with an..." to "...grow at an..."

G2 (Line 23). Change "...scenario and that..." to "...scenario since it is anticipated that...."

S2 (Line 27). Change "...Either replace the commas at the beginning and end of "a high latitude semi-enclosed" with hyphens or insert "which is" after "The Baltic Sea,"

G3 (Line 30). Insert "of" between "GW" and "offshore"

S3 (Lines 32-33). Recommend replacing ".. and expansion has to be performed with care." To "...and therefore expansion must be handled with care."

T2 (Line 35). Change to "increases". Forgot to add "s"

G4 (Lines 36-37). Change "...are prone to have wind profiles with partly negative gradients that can occur under certain meteorological and..." to "...are prone to having wind profiles with partly negative gradients under certain meteorological and..."

S4 (Line 40). Suggest changing "Note that also wind..." to "Note, also, that wind..."

G5 (Line 44). You use "both" to mention three additional factors. Both would apply to two additional factors. Perhaps change the following "...effects, both to assess the longevity of the turbines, the extension of the wake behind a single turbine and behind the park, and..." to effects; as well as to both to assess the longevity of the turbines, the extension of the wake behind a single turbine and behind the park, and..."

S5 (Line 53). Change "...what are the driving mechanisms for this." To "...what are the driving mechanisms that lead to turbulence production."

G6 (Lines 58-59). Change "In addition to this, not only the turbulent characteristics of the LLJs compared to normal profiles are analyzed...." to "In addition, not only are the turbulent characteristics of the LLJs compared to normal profiles..."

S6 (Lines 69-70). The sentence starting with "Already in 1957,..." is awkward. It just so happens that this is the year where this phenomenon was rigorously documented. I would consider starting this sentence with "One of the first proposed mechanisms related to the formation of..." You can fill in the rest.

S7 (Line 71). Instead of "During the evening and night,..." I would argue to use "During the evening transition,.."

S8 (Lines 74-75). The sentence beginning with "As a consequence..." is a bit of a mouthful. Perhaps make the following change: "...gradient force unbalanced, with a subsequent...." to "gradient force unbalanced. This imbalance subsequently leads to an acceleration of the wind: a process known as frictional decoupling."

G7 (Line 80). Change "As an effect..." to "As a result..." and change "compared to a water..." to "...compared to the water..."

S9 (Line 86). Recommend changing "at least if the swell and the wind direction are aligned which is the most studied case." to "...if the wind is approximately aligned with the swell direction"

T3/G8 (Lines 91-92). Change "...on theses matters, but for an introduction to uncommon wind profiles over the Baltic Sea and North Sea, we refer to Kettle (2014) and Moller et. al. (2020)." To "...these different wind profile types. We refer to studies by Kettle (2014) and Moller et. al. (2020) for a description of less common wind profiles of this type."

G9 (Line 119). Change "In a following..." to "In the following...."

T4 (Line 169). Remove "is" after "as" and before "possible"

G10 (Line 185). Do not need comma after "properly" and you remove "those" after "remove"

G11 (Line 210). I was confused with the wording here: "...site of especially swell conditions." Were you trying to say "...site, especially during swell conditions."

G12 (Line 228). Change "on" after "laser" and before "a" to "at"

G13 (Line 246). I would change the sentence starting with "In this additional..." to something like "Application of additional quality controls led to the removal of 6.7% of the data."

G14 (Line 253). Replace "on" between "depth" and "buoy" with "at"

General comment: When introducing a variable, say, C_p , it is common to stick two commas between the variable.

G15 (Line 257). Replace "in" between "10.4 m" and "the" with "from"

G16 (Line 258). Don't need a comma between "sector" and "since"

G17 (263). Replace "on" between "mast" and "Ostergarnsholm" with "at". I've seen this for multiple occasions. I would make sure that this checked elsewhere.

Sincerely,
Christoffer Hallgren and co-authors

References

Finnigan, J. and Einaudi, F. (1981). The interaction between an internal gravity wave and the planetary boundary layer. Part II: Effect of the wave on the turbulence structure. *Quarterly Journal of the Royal Meteorological Society* 107.454, pp. 807–832. DOI: 10.1002/qj.49710745405.

Iungo, G. V. and Porté-Agel, F. (2014). Volumetric lidar scanning of wind turbine wakes under convective and neutral atmospheric stability regimes. *Journal of Atmospheric and Oceanic Technology* 31.10, pp. 2035–2048. DOI: 10.1175/JTECH-D-13-00252.1.

Platis, A. et al. (2022). The Role of Atmospheric Stability and Turbulence in Offshore Wind-Farm Wakes in the German Bight. *Boundary-Layer Meteorology* 182.3, pp. 441–469. DOI: 10.1007/s10546-021-00668-4.

Smedman, A.-S., Högström, U., and Hunt, J. (2004). Effects of shear sheltering in a stable atmospheric boundary layer with strong shear. *Quarterly Journal of the Royal Meteorological Society* 130.596, pp. 31–50. DOI: 10.1256/qj.03.68.

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Zhan, L., Letizia, S., and Valerio Iungo, G. (2020). LiDAR measurements for an onshore wind farm: Wake variability for different incoming wind speeds and atmospheric stability regimes. *Wind Energy* 23.3, pp. 501–527. DOI: 10.1002/we.2430.