

Reponses to the reviewer and editor

We thank the editor and reviewers for their final check and quality control. The comments and suggestions further improve the quality of our paper. In the following, we provide replies to each of the comments in black.

Note, we also updated Fig 1 which is more colorblind friendly, following the suggestion from the Technical Support team.

- **Notation – Boundary Layer Height (Line 37 and elsewhere):** In boundary layer meteorology, z_i is typically used for convective conditions and refers to the height of the first inversion. In neutral and stable conditions, the ABL height is usually denoted h . I recommend avoiding the use of z_i for neutral or stable boundary layer depth.

The places where z_i is used in our paper are:

1. To Eq. (A1) from Wang et al. (2014)
2. Reference to Højstrup et al. (1982)
3. Eq. (23) from Stull (1988)

In all studies, “ z_i ” was used in the original papers.

In Wang et al (2014) and Højstrup (1982), the focus was on unstable conditions.

Stull (1988) cites the studies of the KONTUR experiment from Grant (1986) which represents “near-neutral to slightly convective conditions”. Our attempt is to test Eq (23) with missing information of stability or u^* , for which a neutral stability is assumed and a range of u^* values is assigned. We are not sure if we should change z_i to h for this case, as we think it is better not to change it in order to reserve the flow of discussion.

Nevertheless, we added in the text “Note that the height of the boundary layer for neutral stability is often denoted as h ; here, for a simple denotation, we use z_i for all stability.”

- **Equation (1) and Lines 30–35:** The current wording is unclear, as the text starts with “For offshore conditions” and then states “Equation (1) is valid over land only”. This could be reformulated more efficiently. If U is not explicitly defined as hub-height wind speed, the equation introduces an implicit dependence on height through the mean wind speed. According to IEC 61400-1 Ed. 4, U in Equation (1) is defined at hub height and therefore depends on turbine height (smaller turbines generally experience higher turbulence intensity than larger ones). The notation for wind speed should be clearly distinguished from that used in Equation (2).

Good point. The corresponding text has been revised.

- **Consistency of notation (general comment):** Please, ensure that notations are unambiguous and consistent throughout the manuscript. IEC standards typically use hub-height wind speed, whereas this is not always the case in the scientific literature. Please verify that all equations follow a consistent convention and adjust where necessary.

Good suggestion. We went through the whole text where IEC was mentioned, making sure this concept of hub height is in place.

- **Technical issue – references:** Some LaTeX references appear as “?”. Please check that all BibTeX entries are valid and correctly compiled.

Thanks for spotting this. We have corrected the corresponding reference.

- **Equation (24) – applicability:** As noted by Reviewer 1, Equation (24) (Högström et al. 2002) applies to the neutral surface layer only and is “approximately valid in the surface layer down to at least 1.6 m above the ground”. This limitation should be made explicit. Currently, the equation may appear applicable to the entire ABL, which is misleading. More generally, Equation (24) and the surrounding discussion may not be appropriate unless the paper specifically focuses on neutral surface layer flow (i.e. below ~100 m). If Equation (24) is not used in the analysis, it should be removed.

We owe the readers a clearer description of this part.

Eq 24 was used to provide approximate estimates of height dependence of $(\sigma_u)^2$. The result is shown in Figure 2b. This calculation has nothing to do with our LUT method, but used as a reference, an independent, theoretical study, also one of the few, that also addresses the height dependence of wind variance.

Högström et al. (2002) pointed it out that this expression is derived for the range (iii), which in theory is relevant for the entire boundary layer (for which the scaling u^*/f_c is used). However, the measurements they had can confirm its validity for from a few meters to the measurement height which was in the surface layer. One of their discussion focuses was when the theory breaks down when coming closer to the surface.

As the use of Eq 24 from Högström et al. (2002) is for comparison only, not used in our development of method, we do not see its assumption of neutral stability a problem.

We revised the text to make the above part clearer.

- **Response to Reviewer 1 – Comment 7:** The current response does not fully address the reviewer’s concern. The relation $h = \alpha u^*/f$ is only valid under neutral or stable conditions. The reviewer questioned the choice of $\alpha = 0.3$. While Högström et al. (2002) use this value, it remains somewhat arbitrary and should be better justified.

The corresponding text has been re-written. This includes the following: "...The coefficient 0.3 was used, following Högström et al. (2002), where no theoretical arguments were provided why this coefficient was used; this expression nevertheless was suitable for interpreting their derived measurements behaviors. In Fig. 2b, we plot $\sigma_u(z)/\sigma_u(10\text{ m})$ for both $u^*=0.1$ and 0.2 m/s, to show the sensitivity. The use of a range of u^* values brings the analysis to a qualitative level, thus also reducing the importance of the absolute value for the coefficient associated with z_i .

- **Turbulence scaling (general comment):** The relationship between σ_u and u^* is known not to scale with z/L , particularly under stable conditions. Panofsky et al. (1977) discuss this in detail and show that σ_u/u^* and σ_v/u^* are independent of height in convective conditions, whereas σ_w/u^* behaves differently. Their formulations are valid across the entire ABL and appear to contradict some equations in Section 2.5 (e.g. Equation 23), which should be reassessed.

Thanks for bringing up this discussion. As Stull explained, Eq. 23 was based on a field experiment. We agree with the editor that further assessment would be good for their work, and for this subject in general. We do however believe that it is beyond the scope of the current study. We only use their derivation to compare with our results.

- **Reviewer 1 – Comment 9 (Figure 4):** My suggestion would be to avoid filled contourplots and use a pseudocolor plot instead. I recommend avoiding the jet colormap and use a perceptually uniform colormap. Consider limiting the colorbar lower range to 0.02 or 0.03 instead of 0.00 to better highlight local variations in turbulence intensity. The figure appears to be dominated by low wind speed regions, which inherently increase turbulence intensity; this effect is not adequately discussed (lines 395–400) and should be explicitly addressed.

Thanks for this suggestion. We now replaced the contour lines with a pseudocolor plot ('pcolor'), adjusted the data range for the plots (0.04 to 0.14 for 10 m, 0.03 to 0.1 for 100 m – which is also pointed out in the text). This indeed adds more layers of variability. We also changed the colormap from 'jet' to 'turbo'.

There are more factors than the low wind speed contributing to the high TI areas. We had the following analysis of data: "...based on the ERA5 data, the wind speed is low, and z/L is negative and of large magnitude, suggesting very convective conditions. Thus these large values of TI is a combination of effect from z/L (see Fig. 3d) and the relatively large 2D turbulence contribution at such low winds (see Fig. 3c)" (lines 353 - 357).

- **Equation (7) – turbulence spectrum definition:** The term "turbulence power spectrum" is unclear, as it depends on the velocity component considered. Please clarify whether the spectrum refers to the horizontal wind component or the along-wind (streamwise)

component. Note that Kaimal and Mann models decompose turbulence into along-wind and cross-wind components rather than directly describing the horizontal component. There is currently an inconsistency between Equation (6), which refers to the horizontal component, and Equation (8), which refers to the along-wind component. If Equations (8)–(9) are used to derive σ_U , the manuscript becomes inconsistent by conflating these components. In that case, the horizontal component should be reconstructed by combining along-wind and cross-wind contributions. This point should be clarified, as it may affect the internal consistency of the methodology.

Thanks for pointing this out. The description can indeed be improved. We adjusted the text so that the following is clear. In connection with Eq. 7, it is the wind speed we are referring to. This allows us to extend the application to lower frequencies where we need to apply the 2D-turbulence. This is made possible due to 1) the fact that the spectrum of the along-wind component is very close to that of the wind speed, 2) decomposing the wind vector into along-wind and cross-wind components requires stationary condition, which becomes more challengingly met when we are talking about a time series longer than half an hour. See section 2.1 in the new version.

References

Panofsky, H. A., Tennekes, H., Lenschow, D. H., & Wyngaard, J. C. (1977). The characteristics of turbulent velocity components in the surface layer under convective conditions.

Boundary-Layer Meteorology, 11(3), 355-361.

Högström, U., Hunt, J. C. R., & Smedman, A. S. (2002). Theory and measurements for turbulence spectra and variances in the atmospheric neutral surface layer. *Boundary-Layer Meteorology*, 103(1), 101-124.