Response to Referee 2

Periods of constant wind speed: How long do they last in the atmospheric boundary layer?

Referee's comment (RC) in blue
Author's comment (AC) in black

GENERAL COMMENTS

The authors have made responses and changes addressing most reviewer comments, and the manuscript has improved considerably. The longer measurement period also really helps the analysis and justification. The statistics of normalized Tc, and subsequent power-law exponents, now look quite reasonable (new Fig.3) and potentially useful.
There are some details missing around the scales used with synthesized turbulence from the Kaimal model, as well as the integral scales assumed. There also appear to be inconsistencies between the integral time and length scales (see details below). For consistency and replicability, these need to be addressed.
There are new/remaining language issues within the updated draft, which should be addressed through proofreading.
I suggest revision to respond and address lingering issues including the above and the line-by-line comments given below. An annotated version of the PDF file is also attached, with some language suggestions.

The authors appreciate the constructive comments and generally positive feedback from the referee. We hope that the missing information and the mentioned inconsistencies are now clear. In the following, we address the open questions and line-by-line comments.

SPECIFIC COMMENTS

l.2/3: one should not speculate about "jets" or "ramps" in the abstract without some evidence, or connection within the work or literature (as mentioned in the first review). Such a speculative "illustration" might be possible to include in the introduction, if citations are given to link to CWS.
In the previous revision, we mentioned that the concepts 'ramps or jets' were given for illustration, rather than for formal comparison. However, we accept that the connection of those phenomena to the CWS periods has not been proven. Therefore, we have now deleted the sentence from the manuscript.

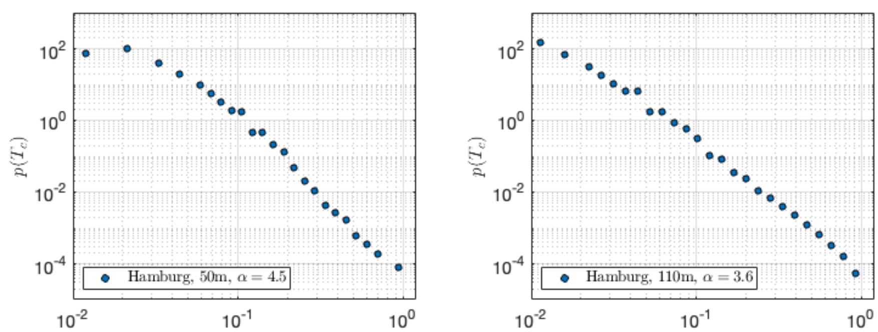
l.5: as mentioned in the previous review.
A modification to the word 'extremes' has been done.

l.7: how do we know these are also a feature over land? To be sure you have found the CWS periods to be a feature for flow offshore.
The following reply also covers the comment on l.158/Table 1.

The referee is correct. We have now emphasized the 'offshore' nature of the data considered for the analysis. Accordingly, we have modified l.6.

However, we want to point out that similar results on the behaviour of $p(T_c)$ have been observed when analyzing onshore data. Data from the Wettermast Hamburg have been investigated. The mast covers inland meteorological measurements up to 330m in height.

Fig. 1 shows the results of $p(T_c)$ from measurements of the wind speed at (a)50m height and (b)110m height. The representation of the $p(T_c)$ follows the same as in Figs. 3 and 4 in the manuscript. The periods T_c are normalized by T_{max} and the values of α for the power law are given in the legends.



(a) (b)

Figure 1: Normalized probability density functions $p(T_c/T_{c,max})$ for data from the onshore Hamburg met mast at heights of (a) 50m, and (b)110m. The value $T_{c,max}$ for each data set is defined after a binning process as the center of a bin containing at least ten of the largest measured periods. The threshold $\varepsilon = A\ \sigma_u$ for the CSR is calculated with $A = 0.3$. The data in Fig. 1 correspond to measurements from January to December 2022. Similarly to the FINO data in the manuscript, only 10-minute periods with mean wind speed between 3 and 25m/s.

When comparing the onshore results in Fig. 1 to the results from the FINO data in the manuscript, it is clear that the value of the exponent α in $p(T_c) \propto T_c^{-\alpha}$, as well as the values of $\overline{T_c}$, σ_{T_c} and $T_{c,max}$ are not universal. They depend on the terrain conditions and the assumptions for the calculation of T_c. However, the power-law behavior at the tails of the distributions $p(T_c)$ appears in both, offshore and onshore conditions. A statement on this is given in l.177 in the manuscript.

l.8: "strictly" is superfluous and overwrought, suggest deleting.
Deleted from the manuscript.

l.37-38: what does "The persistence phenomenon is straightforwardly recalled" mean? I'd suggest fusing it with the sentence it precedes, or remove.
We thank the referee for the suggestion. The sentence was merged with the one after it.

l.39: "inversely correlated" does not make sense alone; I presume you do not mean negative correlations, so this should be re-worded (perhaps without 'correlated'). It appears you are referring to persistence times being inversely proportional to some gust occurrence rates.
The referee is right, 'inversely correlated' might be misleading. With 'correlated' we meant 'associated' or 'related'. We thank the referee for the suggestion of 'inversely proportional to some gust occurrence rates'. Accordingly, we have modified the sentence in the manuscript.

l.158/Table 1: note these values might apply only for offshore since over land the surface-induced turbulence and flow accelerations are significantly stronger (consistent with the findings of e.g. Alcayaga 2017 [M.Sci.] or Kelly 2024). At any rate, it should at least be noted here (also probably in the abstract and conclusions) that the findings here are for the marine atmospheric boundary layer / offshore.
Please refer to the comment on l.7.

We thank the referee for the comment and the provided reference. A sentence and the referenced work have been added to the manuscript in l.155-158

Fig.3: this looks much better than the original, now with alpha∼=4. However, it is a bit confusing regarding the magnitudes. Could it be possible to plot all four heights/lines together, just with smaller dots? Or, just the small dots, since the straight lines are quite apparent?

We appreciate and have considered the suggestions from the referee for replotting Figs. 3 and 4 in the manuscript. However, we believe that in such a representation (i.e. without vertical shifting), the individual distributions $p(T_c)$ for different heights are difficult to distinguish and the figure looks chaotic.

On the other hand, vertical shifting is a common representation for individual PDFs in a unified plot. Unlike spectral densities, the magnitude of $p(T_c)$ is fixed by the normalization condition (i.e., T_c, max) and has no further meaning.

We decided to keep the vertical shifting in Figs. 3 and 4. Nevertheless, we have now decreased the width of the lines depicting the power laws $\propto T_c^{-\alpha}$ for better visualization. In addition, we have now added the '[a.u.]' to the label of the y-axis.

Fig.4: are the different $p(T_c)$ again shifted vertically? Suggest plotting them together as in Fig.3 suggestion, with much smaller dots (or just thin lines connecting the points).

Please refer to the previous comment.

l.192-195 / Fig.5: what σ_u, U do these correspond to? Further: if using different CWS periods, one must normalize the data by respective σ_u^2 (or u_*^2 or U^2) in order to combine into one plot. Presumably, you did this, or did all the CWS have the same variance?

We thank the referee for the question. The information regarding the normalization of $u(t)$ for calculating the spectra was not given in the previous version. A sentence has been added to the text (l.191) and the caption of Fig. 5 in the manuscript.

Fig.5: Since a log-log plot is made to show the power-law, one cannot easily guess the variance "by eye"; however, perhaps to help display better the character you could plot f*E(f) to show the implied spectral peak.
We thank the referee for this important comment. This is certainly true for a more classical turbulence spectrum E(f), e.g, a von Karman or Kaimal spectrum that exhibits a deviation from $\mathrm{f}^{-5/3}$ at small f, implying a spectral peak of $\mathrm{f} * \mathrm{E(f)}$. Nonetheless, due to the filtering of the CWS periods, Fig. 5 shows that E(f) solely possesses an inertial range $\propto \mathrm{f}^{-5/3}$ and $\mathrm{f} * \mathrm{E(f)}$ would simply result in $\propto \mathrm{f}^{-2/3}$.

l.198/§4.1: rather than the Kaimal spectral shape, it is the Gaussian prescription that is the problem; this should be directly stated (instead of just "Kaimal"). Also the heading should indicate your experimental data is from a wind tunnel. This reviewer suggests a subsection title like "Experimental wind-tunnel turbulence and synthesized IEC-standard Gaussian Kaimal"

We appreciate the suggestion from the referee and have adopted it in the manuscript. The title of the subsection has been modified. Moreover, the adjective 'Gaussian' has been added when referring to the Gaussian assumptions within the IEC-standard Kaimal model. Lines 198-202, 229, 233, 239 were modified.

l.226-234 / Fig.7 caption: how was 10s "chosen", not calculated for the synthesized dataset? This appears to be an arbitrary choice, and should be consistent with the Kaimal parameters you chose (Appendix E). It appears that 10s was chosen to make a better comparison, but is inconsistent with L_{int}=10m.

We thank the referee for realizing this inconsistency.

The referee is right, the parameters for generating the synthetic data were intended to be comparable to the atmospheric FINO data. That is certainly the case of the mean $\bar{u}$ and standard deviation σ_u at the reference height (or 'Hub height') $H_H = 90$m. Therefore, $L_{int} = 170$m and $T_{int} = 17$s for the Kaimal data. The calculation follows the definition of the Kaimal spectrum now given in Appendix E.

The value of $T_{int} = 10$s was incorrectly used for the normalization of the periods T_c from the Kaimal data in Fig. 7 Therefore, the analysis was re-calculated with the correct value $T_{int} = 17$s. Fig. 7 is updated and the values in the text revised (lines 228-235). Note that the analysis and discussion are still valid after the correction from $T_{int} = 10$s to $T_{int} = 17$s.

l.298-299: note both the Kaimal/Veers and Mann models assume Gaussian-distributed spectral amplitudes.
We agree with the comment. The plural form 'IEC model**s**' has been adopted in the manuscript.

§4.2 / §5: it is nice that the CTRW model can replicate the alpha (i.e., tail of $p(T_c)$) found from the FINO3 data. However, as seen in Fig.9, it is quite sensitive to the Levy exponent; this should be mentioned. You could also remind the reader in the conclusion that the CTRW model was able to capture the statistics of rare (long) CWS events, when tuning α_{Levy}.
This is a valid observation. l.275 and l.301 have been added to the manuscript to account for the suggestion.

Appendix A/l. : Fig.A.1 is a nice clarifying addition. However, to this reader, the blue and red lines in the bottom half do not appear to match what is shown in the top half (timeseries); the explanation does not seem to cover this either. Is there an error in the bottom half, or could this be explained better in the draft?
We thank the referee for this comment. We have revised Fig. A.1 and there is no error with respect to what we wanted to show. However, we agreed on a deficient explanation of the figure. Therefore we have improved it. We hope now it is clear, why the lines in the bottom half are not 'exactly' matching the crosses over the time series on the top half.

l.384-386/Appendix E: you should show the Kaimal form here, that you used.
We thank the referee for suggesting to describe here the Kaimal spectrum. By doing so, it became clear the calculation of the L_{int} and T_{int} values (aligned with the comment on l.226.).

l.384-385: setting the integral length scale to 10m is not consistent with your choice in §4.1 (see comments above around l.226-234/Fig.7) of $T_{int} = 10s$, unless U=1m/s.
Please refer to the explanation in the comment on l.226-234. This was a mistake. We have revised our calculations and the correct value is $L_{int} = 170$m. We again thank the referee for noticing.

A new version of the manuscript is provided along with a diff file.

References

Wettermast Hamburg. URL https://wettermast.uni-hamburg.de/frame.php?doc=Home.htm.