Response to Editor

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

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

In gray-italic: text from the revised version of the manuscript.

AUTHORS:
Dear Editor, we appreciate your comments and recommendations. In the following, we would like to address the open questions you have posted.

We use the following abbreviations: Constant wind speed (CWS), Atmospheric Boundary Layer (ABL), Wind turbine (WT), Probability Density Functions (PDFs). In this version of the manuscript we have introduced a new abbreviation for the Constant Speed Range (CSR).]

GENERAL COMMENTS

EDITOR:
1. You kind of argue that the CWS periods cannot be reproduced by standard turbulence models; if their pdfs/statistics are quite local, the question is whether they can be reproduced by weather models that one can run routenly anytime anywhere.

Whether a weather model can reproduce very long CWS periods is a very interesting question. The statistical characterization of data from such atmospheric models would provide insight into their potential use for the analysis of the effect of CWS structures on WTs. However, this analysis is beyond the scope of the current investigation. Valuable future work on the topic could be based on weather-modelled data (e.g., ECMVF, WRF).

Lines 309 - 311 have been added to the manuscript as part of the outlook:

Another interesting aspect for future work would be the statistical analysis of CWS periods from weather-modelled data (e.g. ECMVF, WRF models). The results would reveal whether such larger-scale models can reproduce the CWS structures within the atmospheric forecasting.

2. I think you overstress the word "conclusive" and "conclusions" throughout the manuscript without the need for it. Such use does sound like you want to settle the discussion but there is no need for it. There is a section "Conclusions" where you should focus your conclusions and concluding remarks. Please find such statements and rephrase.

We very much appreciate this comment. We agree that the use of the 'conclude' and 'conclusive' statements is excessive. We have carefully revised them throughout the paper. The changes can be identified in the diff file.

3. In Table 1 the standard deviation is taken as the average over all 10-min standard deviations, so it is not turbulence; if this info is not used I advice to remove table 1 and its details.

Yes, you are right. The Table was useful in the previous version of the manuscript. However, at this point it does not provide relevant information to the investigation and the results in the paper. The table has been removed from the manuscript.

4. In code and data availability you tell the readers that this could be done upon request. I recommend you already make these datasets and codes available in a repository or so. The "upon request" statement does not encourage people to use your findings and methods and we all have face no responses when trying to reach authors. Instead, inmidiate code/dataset publication will make your work open to the community (and you will be way much more cited).

Thanks a lot for the suggestion. We are working to make the codes/functions publicly available. Work in progress at:
https://github.com/danielamorenom26/cws.
We hope the codes will be available before the publication of the paper. In that case, the section *Code availability* would be modified.

SPECIFIC COMMENTS

You have general an issue with the references. They should be fully in brackets when they are passive, e.g., if one says: ".... compared to the standard models (IEC, 2019)." But the brackets should only cover the year when the reference is active, e.g., if one says "... as demonstrated by Moreno et al. (2019), the amount of energy...". Please revise all of your references.
Thank you for the constructive explanation. All the references have been accordingly revised in the manuscript.

Also in many instances you have an issue with the "math" mode, so please revise thoroughly. E.g., line 44 you have for H=90m; there should be a small space between H, the = sign, the 90 and the unit.
Thank you for highlighting the issue. All the "math" entries in the document have been revised.

In line 4 you use the word extreme that seems contradictory to a constant wind speed period.
The word "extreme" has instigated several discussions in the context of our investigation. As we analyze the statistics of the lengths of the CWS periods, an extreme event in this case corresponds to a very long CWS period.

In Appendix A of the manuscript, we recall the concept of wind speed excursions (i.e. wind speed $u(t)$ exceeding certain thresholds) which are often called extremes. Under such a definition of an extreme, we agree that a CWS period might be oppositely related. However, it is not rigorously defined.

In line 5 you say that the statistics of CWS are an intrinsic feature; a feature of the ABL could be the CWS periods but not their statistics.
Corrected in the manuscript.

Line 11 maybe replace "a typical multiscale effect. Given the conclusive results," by "multiscale."
Replaced in the manuscript.

Line 21 delete "extensively"
Deleted in the manuscript.

Line 59 to extrapolate the response of the MW turbine to what?
This sentence has been reinforced in the manuscript (still Line 59):

In the context of wind energy, for instance, a Pareto distribution has been tested as an extrapolation method to estimate extreme loads on a multi-megawatt wind turbine generator [Dimitrov, 2016] with a 1-month return period.

Lines 84-86: the sentence along these lines is at this point weird. One can use a standard spectral model to generate a time series characterized by specific turbulence parameters; so in principle, one could manage to synthetically produce a time series with small "turbulent amplitudes" (by playing with the spectral model parameters) so that the fluctuations do not surpass the threshold of choice.
Yes, we agree with your statement. In principle, the generation of time series with small amplitudes is possible from a spectral model. However, here the analysis of data from the IEC standard model aims to investigate the characteristics of the CWS periods within the wind fields currently used for wind turbine simulations. Modifying the characteristics of such standard wind fields is not our goal.

Eq. 2 the dot is normally a dot product which you do not imply here
The dot is removed from the equation and from other instances where it was wrongly used.

Table 2 summarizes some first results; what is the interpretation of these? Is this what one expects?
Thank you, a comment on the data in the table (Now Table 1) is quite appropriate. We included a sentence in the manuscript (Line 151.)

From the values in Table 1, comparable $\overline{T_c} \approx 4\,s$ and $\sigma_{T_c} \approx 3\,s$ are obtained for the four heights H. More interestingly are the longest measured CWS periods $T_{c,max}$ at each height H. Periods with lengths up to $T_c \approx 40\,\sigma_{T_c}$ which correspond to more than $100\,s$ are measured.

Line 180: in line with general comment 2 "We conclude that..." At this point you cannot conclude this; maybe you can hypothesize that these are the type of distributions.
Modified in Line 179 of the manuscript.

This confirms our hypothesis on the Pareto-like distributions of $p(T_c)$ for large T_c already observed in Fig. 3

Lines 183-184 if you observe that by increasing A, alpha and the statistics of Tc change, then don't you know already you are not dealing with laminar flow?
Hopefully, the answer to the next comment will clarify this one. The change in the statistics of T_c for different factors A is not directly related to the turbulent nature of $u(t)$.

Lines 184-187 I am not sure that is really clear how the wind speed time series is selected for the spectral analysis. Do you only take a 5-day time series if there is at least one period of CWS with Tc>10 s, or do you look for all times where Tc>10 s and take the 5-day time series around it?
We modified the explanation of how the wind speed time series are selected for the spectral analysis. It should be clear that the analysis is done with excerpts of time series $u(t)$ within the CWS periods. Starting at Line 185:

The spectra E(f) are calculated from the extracted time series of $u(t)$ during CWS periods larger than $10\,s$. A time window of roughly five days was considered for extracting the definite time series $u(t)$ during $T_c > 10\,s$.

Figure 5 why are not all the lines for each height start at the same frequency in the left part of the plot. And are not these 5-day periods, and so the lowest frequency should be much lower? Maybe by explaining better the previous comment this graph becomes much clearer
Hopefully, it is now clear what is shown in Figure 5. The intervals $u(t)$ (within CWS periods) have different lengths.

Figure 6: all frames can be combined in one single graph
The three frames have been included in a single plot. Thanks for the suggestion.

Line 230: in line with general comment 2 "We have shown conclusive..." Well you have done some good analysis so far for the observations but you only tried one standard wind model with one set of parameters (see my previous comment of lines 84-86)
Modified in Line 231 the manuscript.

We have shown results on the distributions of CWS periods $p(T_c)$ in the ABL and their underestimation by the IEC standard Kaimal wind model...

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

References

N. Dimitrov. Comparative analysis of methods for modelling the short-term probability distribution of extreme wind turbine loads. *Wind Energy*, 19(4): 717–737, 2016.