Title: Underestimation of strong wind speeds offshore in ERA5: evidence, discussion, and correction
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Response to Review Comment 1: Anonymous Referee #1

The original comments from Referee #1 can be found at https://wes.copernicus.org/preprints/wes-2024-27#RC1. They have been reproduced below. Answers from the authors are marked in blue.

General comment

This manuscript is discussed about the reason of underestimation for strong wind speed of ERA 5 and proposed its correction method. The reason of underestimation is clearly identified by comparison with other reanalysis data and some modeling parameter. Also, the efficiency of correction method is validated with trustable in situ measurement data. Although there is lack of discussion, the method is clearly mentioned and methodology is useful.

Many thanks for your comment and your thorough review.

Specific comment

Section 1 Lines 24-29: The authors need to explain more detail about each paper referred here. Also, there is no explanation about correction in (DHI, 2023) refence web page.

We have added a short explanation about the context of these articles. The reference (DHI, 2023) has been added to the supplementary material (a version 2 of the supplementary material has been created).

Section 1 Line 34: "The methods published so far only partially address the bias". The authors need to add reference(s) mentioned here.

We have added references to the studies (Alday et al., 2021) and (DHI, 2023).

Section 1.1 Line 55 Eq. (1): (Peña et. al, 2008) and many other papers define eq.(1) as "$-\psi_m(z/L)$". Although it depends on how the authors define "$\psi_m(z/L)$", it is suggested to use "$-\psi_m(z/L)$", as long as the authors refer (Peña et. al, 2008) without special note.

Thanks for this suggestion. We have applied for the changes.

Section 1.1 Line 65: Numerical value for α_M is not appeared in this paper. These parameters are important to calculate z_0.

We have added a reference to the IFS documentation, and the value of α_M.

Section 1.1 Line 67: Correct "(GSF)" to (GFS).

Thank you, we have corrected this typo.

Section 1.1 Line 73: "τ is the wind stress ($u*02$)"τ is not equal to $u*02$ but equal to $\rho au*02$.

Thank you, we have corrected the expression.

Section 1.1 Line 74: "Drag neutral coefficient" is may not commonly used (sounds like drag is 0). It is recommended to correct the word to "neutral drag coefficient" or "drag coefficient under neutral condition".

Thank you, this has been corrected.

Section 1.1 Line 75: Is "C_{d2}" in Eq.(4) typo of $C_{d,n2}$?

Thank you, this has been corrected.

Section 1.1 Line 76: It is recommended to explain that U_n is wind speed for neutral condition.

We have added "for neutral conditions".

Section 1.2 Line 78: "wind energy measurement datasets" sounds like energy(power) related measurement data. It is recommended to change it to "wind measurement datasets (created/used by wind energy industry, or add specific project name(s), reference(s) etc.)."

We have reformulated with "measurement datasets from the wind energy industry".

Section 1.2 Table 1: It is recommended to add measurement height used in this study in this table, at least.

Thank you, of course this is a good suggestion. We have added a column.

Section 1.2 Figure 2: Geographical location of left bottom box in the figure is difficult to understand. The authors need to Add explanation such as location name or highlight box etc. in the map.

Thank you, of course this is a good suggestion. We have labels for the Mid-Atlantic Bight and New York.

Section 1.3 Line 127: "the range of air-sea temperature difference to $\Delta\theta = T_{4m}$ - SST < [-2; 0.5] °C (North Sea) and $\Delta\theta = T_{4m}$ - SST < 0.5°C (Atlantic Bight)"It is hard to understand the reason that stability criteria in North sea and in Atlantic Bight are different. Although this part is less important for this paper, the authors need to briefly explain or show references. Also, "< [-2; 0.5] °C" might be "∈ [-2; 0.5[°C (inconsistency with figure 3 etc.).

We have corrected the typo in the interval, and added "The reason for choosing two different ranges of temperature differences, is that in the Mid-Atlantic Bight strong wind conditions occur during winter for very unstable conditions".

Section 1.3 Figure 3: Is "[-2;0.5[" semi-open or typo of "[-2;0.5]"?There are many abbreviations (e.g. WS100, WD, PL, LL). in the figure. The authors need to explain in figure title or main body.

There was a typo in the interval stated in the text. We have added clarifications in the caption. We agreed these were missing.

Section 1.4. Line 141, Title: "1.3 Model data"Duplicated sub clause number

Thank you, this has been corrected.

Section 1.3 Lines 142-145: Model data is grided, so these are not located on exact position(longitude-latitude) of in-situ measurement. The authors should explain how these differences were handled. (e.g. interpolated, used nearest grid etc.)

Yes, this was missing. We have explained which method was used for which dataset. For IJM and TNWA we have interpolated data available. For the others, we have used the nearest node as this is the method used for extracting the data on DHI's Metocean on Demand platform.

Section 2.1 Line 155: "the ERA5 model results show larger differences for short fetches" This expression is not good because CFSv2 is not true value. What we can recognize here is just relative relation between ERA5 and CFSv2 depends on fetches. I suggest to modify this to similar expression used for title of figure 5, line 166.

You are right, this a "model to model" comparison. We have replaced by: "Figure 5 shows that at the 62001 buoy location and when separating the dataset between short- and long fetches, the relative difference between the models seems to be larger for short fetches."

Section 2.1 Figure 4: Explain the meaning and values of the colors using color bar etc. Also, it is needed that the explanation of the white circle plots.

It is not straightforward to add a colorbar for this plot, so we have added in the caption that "The density of the scatter plot using a colormap, from blue (low density, few points) to yellow (high density, many points)." About the markers: we have added that "The white dots at the binned mean values (for bins with more than 10 points)."

Section 2.1 Lines 161.163, title of Figure 4: CFSR data is not shown in this figure. It is recommended to remove explanation regarding CFSR data due to avoid confusion.

Yes, we concur. We have removed the statement from the caption.

Section 3 Title: (dot) in the end of clause title is not needed.

Thank you, this has been corrected.

Section 3.1 Figure 7: I recognized $WS_{10m,CDS}$ and $WS_{10m,MOST}$ are essentially derived from the same numerical model, both based on ERA5. However, the scatter between those data seems almost the same level of scatter as, for example, top-right of figure 6. The authors have to explain the possible reasons.

Yes, this is an important point which we hope will be fixed in ERA6. We have added: "As explained above, $u_{(*0)}$ is provided as a forecast value while the 10 m wind speed from the CDS comes from the analysis (ERA5 contains both analysis and forecast fields); also, the Obukhov length L is computed from other model fields, and does not necessary correspond, for every timestamp, to the value used in the IFS. These differences explain the scatter between the reconstructed 10m wind speeds and the ones from the CDS, on the second plot in Fig. 7."

Section 3.1 Figure 10: Wind speed/direction is not uniform in area displayed in figure. Explain the reference coordinate.

Thank you, we realize this explanation was missing. We have added in the caption: "The filters have been applied for each ERA5 node, that is: the timestamps selected for computing the Charnock coefficient values are not necessarily concurrent between all the nodes."

Section 3.3 Line 243: There is no detail expression that why the authors chose the value of $\alpha_{ch} = 0.018$. The authors have to explain the reason.

Yes, this was missing. We have added "The value of 0.018 has been chosen empirically by the authors of the report, from experience and preparatory works. Other values reported in the literature include: 0.012 in (Araújo da Silva, 2022), 0.012 to 0.014 in (Peña et al., 2008b), 0.018 for the IFS ran without the ocean model and in (Brown et al., 2013)." (see the two added footnotes)

Section 3.3 Line 243-244: "ERA5 with max(α_{ch}) = 0.018 (bottom-left)"Cap method or reduced drag coefficient (z0 as well) for strong wind is commonly used for ocean surface layer modeling. The authors should show reference(s) caused to get this idea, if there is.

We have added: "The method where α_Ch is capped to 0.018 is analogous to reduced drag methods is commonly used for ocean surface layer modelling, see for instance section 2.6 of (DHI, 2017) or the SWAN model documentation".

Section 3.3 Fig.11: Quantitative evaluation is not done in this paper. The authors need to show general statistics such as bias, slope, correlation coefficient etc., at least in this figure (recommended to add for other figures).

Yes, you are right. We have added a table, Table 2, where statistics are provided for the comparisons displayed in Figure 12, that is the most important one in this article from our perspective. The text now says "As shown in Table 2, the suggested correction method (constant $\alpha_{Ch} = 0.018$, or $\max(\alpha_{Ch}) = 0.018$) leads to a slight overestimation of strong wind speeds, while the original ERA5 data show an underestimation of almost 10% for the largest values.".

Section 4: The authors need to discuss about generality, limitation, applicability etc. of proposed methodology. For exampleα_{ch} = 0.018 or this method is ERA5 specific? Applicable for extreme wind speed (validated wind speed is up to 25m/s, validated against 1-hour average wind speed but 10-min average is needed for extreme wind)? etc.Also, α_{ch} = 0.018 is originally used in non-coupled ECMWF IFS, so people may think that it better to use non-coupled IFS model directly. Showing separate validation results for long and shot fetch may be good to explain the advantage of present method.

Thank you for this important comment. We have added a new section, 3.4, to answer each of your points. We hope this is satisfactory.