Reviewer comments on "Extreme wind speeds in tropical cyclones using parametric models"

February 12, 2026

1 General comments

The manuscript presents an application of the asymmetric tropical cyclone parametric wind speed model. The model contains a number of adjustable parameters, which were tuned in Vinour et al. (2026). In addition, the manuscript addresses vertical extrapolation of winds from the surface to hub height. The model is demonstrated for five tropical cyclone cases, and the parametric model results are compared with (a) the model of Ishihara and Yamaguchi (2015) referenced in the IEC standard, (b) best-track data, (c) numerical model data (ERA5 reanalysis and HWRF simulations), and (d) observations in coastal areas (met mast or lidar measurements). Results are presented in the form of spatial maps as well as time series at measurement locations, along with corresponding mean bias, root-mean-square error, and percent deviation from the maximum measured wind speed.

The manuscript addresses a relevant topic, and the validation against measurements is of potential value to the wind engineering community.

Regarding scientific quality, the Introduction provides an adequate overview of current approaches to extreme wind modeling in tropical cyclones within the wind energy sector. Overall, the proposed parametric method is described with sufficient detail. However, the manuscript lacks a discussion. In my opinion especially the following points deserve more attention:

1. It is understandable that only a limited number of tropical cyclones are analyzed due to the availability of measurements. However, particularly since the results are presented in terms of error metrics, the authors should include a discussion of the representativeness of both the small sample of typhoon cases and the location of the measurements. Similarly, horizontal wind fields are shown for specific time steps, and error metrics for these time steps are presented. The motivation for selecting these particular time steps, as well as their representativeness, is not explicitly addressed.

2. Vertical extrapolation of the wind speed from the surface to hub height is addressed by (1) estimating the drag coefficient through fitting a logarithmic profile to measurements, and (2) quantifying errors in hub-height wind speed obtained from extrapolation with the power-law with different shear exponents. According to Powell et al. (2003), for tropical cyclones over the open ocean, a logarithmic profile is, on average, a good description of the wind profile up to approximately 300 m. However, in coastal regions, especially near Taiwan, there may be deviations from the logarithmic wind profile. Therefore, and given that measurements are available at multiple heights, it would be valuable to additionally present or discuss time-averaged wind profiles for the different measurement stations.

3. The manuscript evaluates parametric models originally developed for open-ocean conditions when applied to coastal areas. In my opinion, it is valuable that the study highlights the associated challenges. However, I suggest (a) explicitly acknowledging that these models were not designed for coastal environments, and (b) providing clearer motivation in the introduction for their application to coastal areas.

In terms of presentation quality, the manuscript needs to be further improved. In particular, I suggest considering the following points:

1. The research gap and scientific questions should be stated more clearly. It would be beneficial to explicitly formulate how this study expands upon Vinour et al. (2026).

2. Sections 2 and 3 are rather long, and it can be difficult for the reader to maintain an overview of the different models, vertical extrapolation approaches, and averaging times used. This could be improved, for example, by adding a short motivation at the beginning of these sections that clarifies which model is used for which purpose, and/or by restructuring the sections.

3. The number of tables (16) and figures (20) is large, and the authors may wish to reconsider whether all of them are necessary for presenting the results.

4. There are several passages where the language could be more precise. One example is provided in Specific Comment 9.

2 Specific comments

The following comments address specific lines or sections of the manuscript, and provide suggestions to the authors.

1. Lines 69-76: The research gap could be stated more clearly. In particular, it would be helpful to explicitly describe how this study expands upon Vinour et al. (2026). Please also specify whether the parametric model is implemented exactly as described in that manuscript or whether there are any differences. For instance, was the extrapolation to hub height already performed in Vinour et al. (2026) or is newly introduced in the present study.

2. Section 2: The large number of subsections makes it difficult to maintain an overview. In particular, it is confusing to me that the vertical extrapolation (Sect. 2.2) and the local orographic effects (Sect. 2.3) are introduced before the Ishihara and Yamaguchi model is described (Sect. 2.4). As I understand it, vertical extrapolation is handled differently for the two parametric models, whereas the local orographic effect is applied in the same way to both. This could be further clarified.

3. Lines 142-150: You could help the reader to better understand why different methods are used to convert 3-hourly wind speeds to 10-minute wind speeds for time series and spatial maps. Also, it would be good to further clarify why is this not considered in the OROWSHI model.

4. Sect. 2.5.1: Please specify whether HWRF was run specifically for this study or whether an existing dataset is used. If a dataset is used, please provide an appropriate citation (and, if it is not publicly available, indicate whether related studies based on the same dataset exist). You describe the output format of the HWRF runs, if the model was run inhouse, it would be good to further specify the model set-up in more detail.

5. Vertical extrapolation: Please note that the shear exponent α depends on height. Therefore, please specify which heights are used, especially in Sect. 5.2. Regarding Sect. 2.5.3, also note that extrapolating winds using α is often reasonable over short vertical distances; however, the height range between 10 m and 220 m for HWRF is large, it would be better to use the two output levels closest to the measurement height.

6. Sect. 4.3.1 and Sect. 4.3.2: Please specify the type of mast measurements.

7. Horizontal maps are given for certain time steps; it would be good to always state what time step is shown and why it is selected. You could also consider marking these time steps in the time-series figures. Further, presenting scores for these time steps based on best-track data is, in my opinion, problematic because (a) there is no clear motivation to assess exactly these time steps, (b) the domain maxima depend strongly on how time scales are converted. Here, the I&Y15 model is converted to give the 10-minute maximum over a 3-hour period, HWRF output is argued to provide a 10-minute average, ERA5 has a comparatively smaller effective temporal resolution, and what OROWSHI aims to represents was not clear to me. In my opinion, the number of tables is already large. Therefore, it could be better to indicate the corresponding 1- and 10-minute maximal sustained wind speed from the best track dataset in the colorbar of the corresponding figure.

8. Line 342: *"The proposed model shows a reasonable agreement with the measurements with low MBE and RMSE (see Table 9), but all models underestimate the main wind peak."* Please note that HWRF is in good agreement with the measurements at the analyzed output time steps (rectangles in Fig. 13). In Fig. 13 the peak wind speeds appear underestimated in HWRF because the wind speed was linearly interpolated between different output time steps.

9. In different parts of the manuscript, especially in Sect. 6 and Sect. 7, the language could be more precise. To give a concrete example, I provide the following text from the manuscript, with suggested formulations in green: *"Figure 14 shows the surface wind field obtained with the four models. The TC had landfall at the shown time step. The high-wind area is above the ocean according to HWRF. The TC intensity (Table 10) is reasonably estimated. Differences between the modeled and IBTrACS maximum wind speeds are limited to 3.6* $\mathrm{m\,s^{-1}}$ *or less for all four models. The faster decay of the wind profile of the wind speed with increasing distance to the cyclone center using the I&Y model causes the TC to have a smaller coverage e.g. smaller area of wind speeds larger than xx* $\mathrm{m\,s^{-1}}$ *than the other methods, which explains the wind speed underestimation at the measurement site being xx km to the north west of the cyclone center at the shown time."*

3 Technical corrections

The following comments are minor, technical suggestions meant to help the authors.

1. Line 6: *"WASP"*, write out the abbreviation, especially to avoid confusion with the Wind Atlas Analysis and Application Program (WAsP).
2. The term *"High-fidelity models"* is not very accurate, particularly since ERA5 is has a relatively low-resolution and is a freely available dataset. It would be more accurate to refer to ERA5 as a "global reanalysis dataset" and HWRF as a "mesoscale numerical model."
3. Lines 25–28: I suggest writing *"They found [..]" or "According to their analysis [..]".* Note that it is not surprising that the model can fit SAR well due to the number of adjustable parameters.
4. Line 43: *"IEC"*, please write it out once as "International Electrotechnical Commission's standard."
5. Lines 43–44: *"The shear exponent is set to 0.10 for offshore conditions[..]"* Could you specify where, do you mean in Ishihara and Yamaguchi (2015)?
6. Line 47: *"Vertical wind distribution"* — "vertical wind shear" would be more accurate.
7. Lines 53–54: *"In Ma et al. (2021), the gradient wind speed is extrapolated at hub height (100 m) using several formulations, some of which assume a strictly increasing drag coefficient."* Consider: *"In Ma et al. (2021), the gradient wind speed is extrapolated down to hub height (100 m) using several formulations, some of which assume a strictly increasing drag coefficient with wind speed."*
8. Unit "$\mathrm{m.s^{-1}}$", why use a point between units? (You can add spaces using the tilde in LaTeX.)
9. Equation on Line 91: You can help the reader by explicitly stating that p stands for the adjustable parameters.
10. Equation on Line 106: Could you specify whether the values 156.1, etc., were taken from Olfateh et al. (2017) or Vinour et al. (2026)?
11. Fig. 1: Instead of citing Larsen and Ott, it might be better to cite the SWAN model, i.e., Zijlema. I also wonder whether Ma 2021 is the best reference here, please also add which of the formula's provided in Ma 2021 is shown? Comparing against a well-known Charnock formulation might be better.
12. Line 128: *"MASCOT"*, the software can be cited using a proper citation entry.
13. Line 160: You can cite WRF using Skamarock et al. (2019).
14. Lines 172–173: *"Therefore, for consistency with the averaging time of the observations used in this study, HWRF winds are assumed to represent 10-min averaged values."* Please note that this depends on grid resolution and model configurations.
15. Line 192 and others: Instead of *"previously,"* it is better to refer directly to *"Sect. x"*.
16. Line 195: *"(e.g., estimated from the pixel of maximum wind on a SAR image)"*, this may give the impression that the best-track data is entirely based on SAR; however, different datasets are incorporated in the best-track data.
17. Equation 15: Could you use consistent naming, i.e., is $Ma_{max} := Ma(r = R_{max})$?

18. Line 245: Could you repeat "according to the MASCOT software,". This also applies to the label of Fig. 3.

19. Line 238: Is this also a floating lidar?

20. Line 269: *"CFD cannot give reliable results in such cases."* This statement is perhaps too general; I suggest briefly elaborating why CFD was not chosen.

21. Sect. 5.1: Please specify if all measurement heights are used for the least-squares fit.

22. Tables 3 and 4 and their descriptions: Please specify that these values are calculated for the wind speed.

23. Lines 287–288: Add *"for the analyzed data"* or similar; As the dataset is small, the scatter in the data is large, and it may be affected by terrain effects.

24. Line 303: *"straight-line propagation"*, it is not clear what that means, you could write: *"the location of the tropical cyclone center from the best-track data is linearly interpolated in time."*

25. Lines 315–316: *"ERA5 (Fig. 9b) clearly underestimates the inner core but matches the outer core reasonably well due to its accurate representation of weak winds."* How do you reach this conclusion? There are no measurement data presented. Please clarify that this is in comparison to HWRF, and note that the accurate representation of weak winds may be coincidental. While HWRF likely performs better in the inner core, comparison to best-track data would be necessary to support this statement.

26. Lines 330–337: You could be more precise in wording, e.g., *"forward speed," "eyewall [..] is also much larger"* (you likely mean the radius of the eyewall), *"island blocking action," "configurations,"* etc.

27. Line 341: *"the proposed model"*, which model is meant?

References

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Powell, M. D., Vickery, P. J., and Reinhold, T. A.: Reduced drag coefficient for high wind speeds in tropical cyclones, Nature, 422, 279–283, https://doi.org/10.1038/nature01481, 2003.

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