We would like to thank the reviewer for his thorough reading of the paper and his useful comments and suggestions. We feel that the paper has considerably benefited from his remarks. We provide in the following detailed responses to his comments, and significant additions and modifications of the manuscript are highlighted in blue in the revised version.

1 Response to reviewer #1

1.1 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:

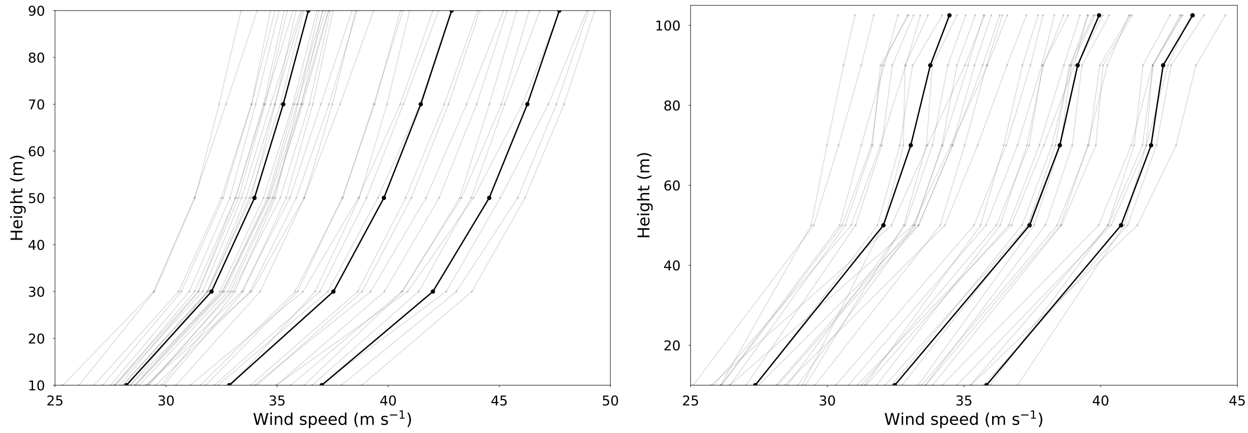
1. *It is understandable that only a limited number of tropical cyclones are analyzed due to the availability of measurements. However, particularly since the results are presented in terms of error metrics, the authors should include a discussion of the representativeness of both the small sample of 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.*

   We thank the reviewer for this comment. We agree that the representativeness of both the selected tropical cyclones and the chosen time steps deserves further clarification. The dataset used in this study originates from a collaborative research project, in which the industrial partners provided access to multi-height measurements of five tropical cyclones. These measurements were therefore used to ensure that the project outcomes would directly benefit ongoing industrial applications. As a consequence, the number of available TC cases is indeed small and cannot be considered representative of the full diversity of tropical cyclones. Nevertheless, the selected events cover a range of intensities, from relatively weak systems (e.g. Isaias) to more intense cases (e.g. Megi), which allows us to apply and compare the models under different conditions. The geographical locations of the measurement sites correspond to regions where offshore wind farm development is being considered by the project partners. In particular, sites in Japan and Taiwan are of strong interest because of the pronounced orographic effects, which are highly relevant when evaluating the applicability and limitations of parametric models which are designed and calibrated for open-ocean conditions. Regarding the horizontal wind field maps, the time steps shown were selected as follows: for each TC case, we identified the HWRF output time step closest to the moment of maximum wind speed at the measurement site, and when relevant, we also selected time steps where interaction with nearby islands or complex terrain was expected to influence the TC structure significantly. These moments allow for the most meaningful comparison between the parametric and numerical models or reanalyses that explicitly account for topographic effects. The representativeness of the dataset and the motivation behind the selected time steps have therefore been clarified in the revised manuscript (L.298-300 and L.339-341).

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.*

   We agree that vertical wind shear may be affected by upstream terrain and deviate from a logarithmic profile in coastal environments. To address this point, we examined averaged vertical wind profiles in Taiwan at sites 1 and 2 during strong wind events of Megi (2016). We applied the same filtering as in the main analysis and retained only periods with $U_{10} > 25\,\mathrm{m\ s^{-1}}$, focusing on conditions relevant for design (see Fig. 1 below). Under these conditions, the profiles generally follow a logarithmic shape. As illustrated by Fig. 18 of the revised manuscript, the wind direction indicates that the flow enters the Taiwan Strait from the northeast and remains largely unaffected by the orography.

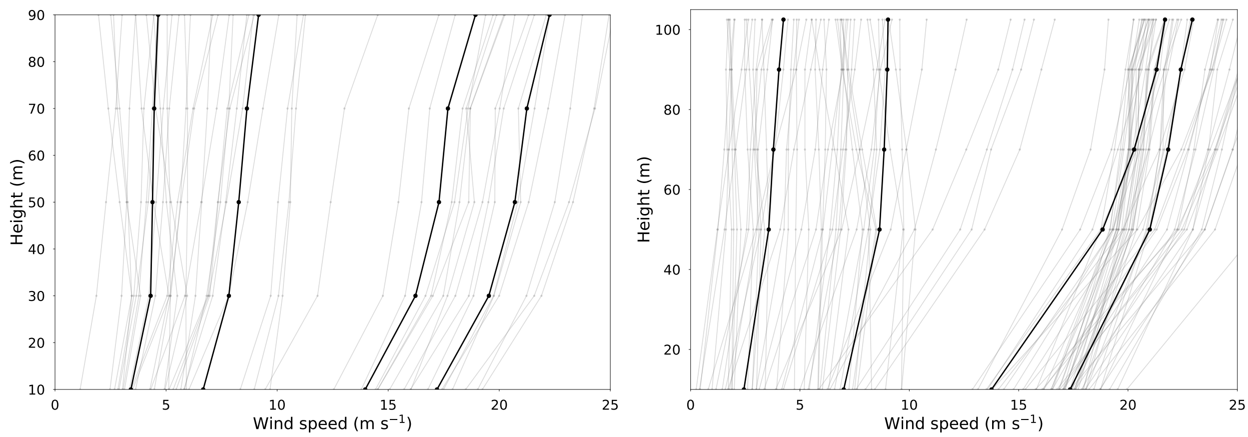
   Nevertheless, the vertical wind shear may deviate from the logarithmic profile when the TC is located over the Taiwan Strait and the sites are in the island's wake. (as illustated in Fig. 20 of the revised manuscript). In these cases, profiles can become nearly uniform or even show a decrease in wind speed with height for $U_{10} < 10\,\mathrm{m\ s^{-1}}$ (see Fig. 2 below). However, these situations correspond to low winds and are not the focus of this study. For conciseness, and in line with the reviewer's comment on the number of figures (3), we



(a) Site 1 (b) Site 2

Figure 1: Vertical wind speed profiles (grey lines) and mean profiles for 5 m s^{-1} wind speed bins (black lines) for U_{10}>25 m s^{-1} at site 1 (a) and 2 (b).

have not included these additional figures in the manuscript, but we provide them here for completeness.



(a) Site 1 (b) Site 2

Figure 2: Vertical wind speed profiles (grey lines) and mean profiles for 5 m s^{-1} wind speed bins (black lines) for U_{10}<20 m s^{-1} at site 1 (a) and 2 (b).

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 the revised manuscript, we have therefore explicitly acknowledged the limitations of the parametric models and better clarified the motivations in the introduction (L.70-74).

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).*

   We thank the reviewer for this comment. Vinour et al. (2026) focused exclusively on surface wind fields (10 m level) and on improving parametric models using SAR measurements over open-ocean conditions. Their analysis did not address hub-height winds, vertical extrapolation, or orographic influences. Our study aims at extrapolating surface wind to hub height and evaluating the performance of the model at different vertical levels in coastal regions. This has been better highlighted in the revised manuscript (L.77-79).

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.*

   We fully agree that Sections 2 and 3 contain a lot of information. In the revised manuscript, short introductions and transitions have been added to improve readability and maintain a clear flow (L.100–102, 125, 139–140, 161-162, 172–173, 202–203, 212-216, 227–231, 257–259).

   In addition, the section on orographic effects has been moved to Section 2.3, after the description of the parametric models, since both models account for the speed-up ratio.

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.*

   We agree that the manuscript contains a large number of figures and tables. Following the reviewer's comment 1.2.7, the comparisons of the wind intensity has been removed, which reduced the number of tables. On the other hand, each table of scores is essential to illustrate the discrepancy. Keeping only the table of the synthesis has been envisaged, but it does not allow for detailed comments on the errors at each site.

   Regarding the figures, we believe that each of them provides important information to support the comparison between the models and to highlight their respective strengths and limitations. Therefore, they all have been retained in the revised manuscript. Nevertheless, if the reviewer considers that some figures are not essential, we would consider their removal.

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

   We thank the reviewer for this comment. We therefore have had more detailed descriptions in the revised manuscript, especially in Section 6 and 7.

1.2 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.*

   Following this suggestion, we have better clarified the extension of the model of Vinour et al. (2026), which described the surface wind only. The vertical wind extrapolation is introduced in the present paper (L.77-79).

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.*

   Indeed, Section 2.2 on vertical extrapolation is linked to the OROWSHI model, which is now Section 2.1.3 in the revised manuscript, since it is an extension of the surface wind model of Vinour et al. (2026). Also, section 2.3 on topographic effects has been moved after the description of the Ishihara and Yamaguchi (2015) model, as the two parametric models use the speed-up ratio.

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 this is not considered in the OROWSHI model.*

   In Yasui et al. (2002), it is argued that the wind speed from the simulation model is a 3-hour average since it has smooth variation, similar to the measured 3-hour average. Since they used best-track data at a 3-hour time step without interpolation, it is expected that wind speed fluctuations at 10-min interval can not be represented. Therefore, the 3-hour wind speed U_{3h} is converted to 10-min wind speeds using random wind speeds following a normal distribution with a standard deviation of $0.1U_{3h}$ to replicate wind speed variations in a short period of time. However, it does not make sense to apply the same method for a wind field at a fixed time step. Therefore, to obtain a consistent order of magnitude of the maximum 10-min wind speed at a given instant, and for comparing the maximum wind speed with the other models at these instants, the formulation from Yamaguchi et al. (2011) is used for the conversion. The 3-hour average surface wind field is computed, and converted to 10-min average using the coefficient derived from Eq. 8 and applied to the entire wind field. This has been better clarified in the revised manuscript (L.150-155).

The OROWSHI model does not use this method since one of the governing parameters of the model is the maximum wind speed V_m from best-track data. In the study, the parameters from the US Agency are considered. V_m is therefore the 1-min average maximum sustained wind speed. Thus, without any processing and using the raw V_m, the wind speed computed with the OROWSHI model would be a 1-minute average. We therefore used a standard gust factor (0.93 according the WMO), widely used in other studies to convert the wind speed to a 10-min average. This is specified in the revised manuscript (L.119-124).

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.*

   Results from HWRF were provided by the NOAA through a scientific partnership with Ifremer. Unfortunately, there is no specific reference for this dataset. We have specified the origin of the data the revised manuscript (L.180).

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.*

   All available heights are used for deriving the empirical coefficients. It is now specified in the revised manuscript in Section 5.2 (L.303-304).

   The first two vertical levels of the HWRF results are at 10 m and 220 m, no data are available between these two levels. This reason motivates the extrapolation of the wind speed at the altitude of interest using a power law. Also, for TCs Hinnamnor, Nanmadol and Isaias, the wind time series comparison is made on the two levels close to the output of ERA5 (100 m) and HWRF (220 m), to minimise the error of the extrapolation. For the met masts in Taiwan, the highest vertical level is about 100 m.

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

   We add some details about the mast measurements at the beginning of subsections 4.2 and 4.3 (L.282-283; L.287-288).

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 represent 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.*

   The dates corresponding to each time step, and the choice of these time steps, which are used to illustrate the difference in the wind fields and most importantly the interactions with obstacles, are explicitly stated in the revised manuscript (L.348-351; L.366-367; L.383-384; L.403-405; L.424-425). Vertical dashed lines have been added to the figures to mark the instant at which the surface wind field is assessed.

   We agree that presenting scores of intensity on these specific time steps is not relevant and not statistically representative. These comparisons have therefore been removed in the revised manuscript.

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.*

   We have clarified the sentence by stating that the output time step of HWRF is too large to effectively capture the peak wind speed (L.347-348; L.380-381).

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 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 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."

Following the reviewer's suggestion, additional clarifications have been added to Sections 6 and 7. In particular, the errors (MBE, RMSE, PD_{max}) are specified in the text and more discussions on these errors have been added.

1.3 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).*

   We explicitly write wage-age stress-dependent parameterisation in the abstract (L.5-6). The abbreviation is later defined in the text (L.69).

2. *The term "High-fidelity models" is not very accurate, particularly since ERA5 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."*

   Indeed, ERA5 can not be considered as a high-fidelity model. Following the reviewer's suggestion, we have therefore changed this denomination in the text by replacing 'high-fidelity models' by 'global reanalysis' and 'mesoscale model' to refer to ERA5 and HWRF (L.9-10, L.81-82, L.175, L.468).

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.*

   Following the reviewer's suggestion, we wrote 'According to their analysis" (L.26).

4. *Line 43: "IEC", please write it out once as "International Electrotechnical Commission's standard."*

   IEC is now explicitly written in the revised manuscript (L.43)

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)?*

   Ishihara and Yamaguchi (2015) considered a shear exponent of 0.1 for offshore conditions in their model. This has been better clarified (L.45).

6. *Line 47: "Vertical wind distribution" — "vertical wind shear" would be more accurate.*

   This has been changed in the revised manuscript (L.48)

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."*

   We have reformulated the sentence in the revised manuscript (L.55-56).

8. *Unit "m.s-1", why use a point between units? (You can add spaces using the tilde in LaTeX.)*

   This has been corrected in the text as well as in all figures and tables.

9. *Equation on Line 91: You can help the reader by explicitly stating that p stands for the adjustable parameters.*

   This has been added in the revised manuscript (L.97 and 111).

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)?*

    We explicitly stated that these values come from the study of Vinour et al. (2026) in the legend of table 2 of the revised manuscript.

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.*

    The SWAN model is now referred to in the revised manuscript (L.135). Also, we agree that Ma et al. (2021) might not be the best reference here. We therefore replace it by the COARE 3.0 formulation (Fairall et al., 2003), which is a well-known formulation based on the Charnock parameter (L.136-137).

12. *Line 128: "MASCOT", the software can be cited using a proper citation entry.*

    We have removed the footnote and have kept the reference Ishihara and Hibi (2002), as done in Ishihara and Yamaguchi (2015) (L.165).

13. *Line 160: You can cite WRF using Skamarock et al. (2019).*

    This has been modified in the manuscript (L.182).

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.*

We agree that considering 10-min average wind speeds depends on the model configuration. We have specified this point in the revised manuscript (L.191). Note that we used the data provided by the NOAA and therefore have not run HWRF by ourselves.

15. *Line 192 and others: Instead of "previously," it is better to refer directly to "Sect. x".*

    This has been modified in the revised manuscript (L.211).

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.*

    We thank the reviewer for this comment. Indeed, best-track data can be obtained with other measurement techniques. We have therefore added other sources of measures (L.214-215).

17. *Equation 15: Could you use consistent naming, i.e., is M amax := M a(r = Rmax)?*

    This has been taken into account in the revised manuscript in Eqs. 14 and 15.

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

    We specify that the speed-up ratio is computed using the MASCOT software in the text (L.268) and in the legend of Fig. 3.

19. *Line 238: Is this also a floating lidar?* The site is actually on the small island of Tairajima. The lidar is therefore fixed. We specified it in the revised manuscript (L.261).

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.*

    We have reformulated this sentence to better explain why MASCOT is not appropriate for Taiwan (L.294-295).

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

    Following the reviewer's suggestion, the use of all measurement heights is specified in the revised manuscript (L.303-304).

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

    This has been specified in the revised manuscript.

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.*

    This has been taken into account in the revised manuscript (L.310-311; L.319).

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."*

    In the Monte Carlo approach of Ishihara and Yamaguchi (2015), synthetic TC parameters are generated at the closest distance from the site of interest. To generate time series of wind speed, it is assumed that the TC propagates in a straight line from this location. We have better explained the straight-line propagation assumption in the Monte Carlo approach (L.335-336).

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.*

    Indeed, there are no measurements in this area. HWRF results are used as a reference for this conclusion. We therefore have better explained this comparison and nuanced the statement (L.350-354).

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.*

    Following the reviewer's suggestion, we have reformulated the text in the revised manuscript (L.365-372).

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

    We meant the OROWSHI model. This has been specified in the revised manuscript (L.378).