Reviewer 1

Review of Ribberink et al. (2025)

I would like to thank the authors for performing a thorough revision of the manuscript and replying in detail to the many points raised in the previous review. The paper has made good progress, and it is now closer to being ready for publication.

Despite these improvements, there are still several points that should be addressed before publication.

1) Use of the term "minimum MSLP": While this is technically not incorrect, it can be redundant or potentially misleading. Since the authors are referring to the storm center, which implies a single-point minimum, the term "mean" is unnecessary. Simply using "minimum sea level pressure (MSLP)" or "central pressure" would be clearer and more appropriate in this context.

We thank the reviewer for their input and have changed the wording to "central pressure" to match the wording already present in many figure captions.

2) Lines 235-243: Although the physical mechanisms behind this behavior warrant further investigation (and may be beyond the scope of this manuscript), the significant drop in MSLP does not correspond to a proportional increase in wind speed. I still don't believe the resolution is the whole story. Given that surface wind speed is largely governed by both the pressure gradient and surface properties, one possible explanation is the representation of surface roughness length over the ocean. Assuming RACMO uses the Charnock formulation (as is common in many atmospheric models), the roughness length is tied solely to wind speed. This can lead to unrealistically large roughness values as the storm intensifies. A recent study (Jung et al., 2025) showed that coupling an atmospheric model with an ocean wave model can yield more reasonable surface roughness estimates—specifically, reduced roughness values (see their Figs. 11e–f)—which significantly impacted hurricane wind speed and structure. From this perspective, the lack of air–sea interaction in the current setup may provide a more plausible explanation for the observed wind–pressure inconsistency.

RACMO does use the Charnock formulation, however after a comparison study found that RACMO underestimated 10m wind speeds at sea, the Charnock formulation was edited slightly to have a constant surface roughness length at higher wind speed values (van Meijgaard et al., 2008). This was found to increase the 10m wind speeds by about 5-10 % for wind speeds above 20 m/s.

We definitely acknowledge that coupling an ocean model to our atmosphere model would give more accurate results than the slab ocean incorporated in RACMO currently. This applies for more than just windspeed; when Ophelia first forms it moves little, which would likely generate a “cold pool” under the storm, hampering some of its growth. It would be an interesting extension to see how such an edge case storm develops with this feedback, and what that might mean for other similar storms studied in other atmosphere-only models.

3) Regarding the response to Comment 6):
Thanks for the explanation. I understand that stronger storms in warmer conditions can modify their environment more effectively. However, since the warming is applied uniformly across the domain, the environmental temperature gradient (and thus the large-scale thickness asymmetry) remains the same.
From my understanding, the authors suggest that internal storm processes can reshape the surrounding geopotential structure in such a way that it delays the development of asymmetries captured by the B parameter. Could you clarify whether there is physical finding that storm-induced heating asymmetrically alters the geopotential field at a sufficient scale to influence the timing of B onset?

We apologize for the confusion. We were referring to the fact that in strong, mature warm-core systems, the storm creates a larger and more robust warm centre. This is more resistant to the effects of the jet stream, such as wind shear, which can tear into the vertically stacked, axially symmetric core and initiate the thermal asymmetry development. As such it is less that the storm asymmetrically alters the geopotential field than it insulates itself from the factors that would alter the field around itself.

4) Regarding the response to Comment 8):
The response emphasizes that beta drift is calculated using Rmax and largely discusses variations in storm size across experiments. However, beta drift is also sensitive to storm intensity and overall circulation strength. Based on Figure A2, storms in warmer conditions (e.g., October 14) appear to exhibit noticeably greater intensity. In that case, could the authors clarify whether beta drift in these simulations is primarily driven by storm size, or if storm intensity also plays a significant role?

To demonstrate the relative importance of r_{max} and V_{max} in the beta drift, we use the beta drift equation given as

$$BD = 0.72B^{-0.54}r_{max}^2\beta$$

$$\text{where} \quad B = \frac{r_{max}^2\beta}{V_{max}}$$

If we substitute and simplify:

$$BD = 0.72\left(\frac{r_{max}^2\beta}{V_{max}}\right)^{-0.54} r_{max}^2\beta$$

$$BD = 0.72\left(\frac{V_{max}}{r_{max}^2\beta}\right)^{0.54} r_{max}^2\beta$$

$$BD = 0.72(V_{max})^{0.54}(r_{max}^2\beta)^{0.46}$$

$$BD = 0.72\,V_{max}^{0.54}\,r_{max}^{0.92}\,\beta^{0.46}$$

Due to the greater power on r_{max}, we expect that storm size has a greater effect on Beta drift than V_{max}.

We have added this to the Appendix (see B3), and a reference in Appendix A, lines 543-545.

However, based on the beta drift equation, we expect that R_{max} has a greater effect on the beta drift than V_{max} which may be a contributing factor to the relative lack of difference between beta drift values across simulations despite large values of V_{max} (see Appendix B3 for the derivation).

Reviewer 2

Thank you to the authors for their work addressing comments/concerns raised by myself and another reviewer. I have just one lingering question and one suggestion to address prior to publication.

L118–119: Can you comment on this choice to keep CO2 constant for the warmed simulations (perhaps RAMCO doesn't allow for changing concentrations?) and how it might affect your results? I think this information is important to add to the text.

RACMO does allow for changing concentrations (as was done in e.g. Dullaart et al. (2024)). We use the simple delta T to simplify and constrain the experiment, ensuring that Ophelia is picked up by the jet stream. Adjusting the CO_2 levels as well as the delta T would double up and create unrealistic results, and make it difficult to extricate the relative effects of the change in temperature. Changing just CO_2 levels would be similar to a PGW approach.

Fig. 4 and related discussion: Michaelis et al. (2019; https://gmd.copernicus.org/articles/12/3725/2019/) and included references might be useful for other examples of misrepresenting TC intensity (especially regarding max winds).

We thank you for the suggestion and have inserted references into that section, specifically lines 245-247.

Dullaart, J. C. M., de Vries, H., Bloemendaal, N., Aerts, J. C. J. H., & Muis, S. (2024). Improving our understanding of future tropical cyclone intensities in the Caribbean using a high-resolution regional climate model. *Scientific Reports*, *14*(1), 6108. https://doi.org/10.1038/s41598-023-49685-y

van Meijgaard, E., van Ulft, L. H., van de Berg, W. J., Bosveld, F. C., van den Hurk, B. J. J. M., Lenderink, G., & Siebesma, A. P. (2008). *The KNMI regional atmospheric climate model RACMO version 2.* (No. TR-302).