

Interactive comment on “How Well do Models Represent the Development of Extra-Tropical Cyclones? Evaluation of Two General Circulation Models Against NAWDEX IOP 6 Observations” by David L. A. Flack et al.

Anonymous Referee #1

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I have more expertise in the dynamics of extratropical cyclones than in the cloud microphysics. Thus, my comments mostly deal with the general presentation of the study and that part of the content which is related to the dynamics of the system.

General comments: In this study, the authors evaluate the performance of two climate models simulating the evolution of one North Atlantic extra-tropical cyclone (so called Stalactite Cyclone). The authors compare the models with two horizontal resolutions against ECMWF operative analyses. Quasi-geostrophic omega equation is applied to distinguish the dynamic and thermodynamic (diabatic) processes within the cyclone.

Furthermore, airborne measurements of microphysical properties of the clouds are compared to the model simulations. I found the topic of this paper interesting and suitable for the scope of WCD journal. The language is clear and understandable. However, I think that the paper suffers from some flaws in its organization, which are mostly due to its length. Therefore, more effort is needed to make the presentation of the paper and its message clearer for the readers. I hope that my comments will help the authors in this work.

Recommendation: major revision

Major comments, which are partly connected to each other:

1. In general, I think that the authors are trying to put slightly too much content into one paper and thus the manuscript was partially hard to read and lacking a clear and consistent storyline. In my opinion, answering comprehensively to the objectives 1 and 2 listed in the introduction would suffice perfectly to the topic of this paper. In its current form the point 3 feels somewhat disjoint and was, to my opinion, particularly difficult to understand. I think that the main results from points 1 and 2 (e.g. the fact that there was no change in the relative importance of diabatic processes with increased resolution) are very interesting and worth publishing alone without focusing on the comparison of microphysical properties with the models and observations, which in turn would be suitable as its own manuscript if investigated properly. Moreover, there is room for improvements in the organisation of the paper. For instance, between the introduction and Section 3, the authors present the overview of the Stalactite Cyclone. I think that this kind of section would better belong right before Section 4, where the representation of the cyclone by the models is presented. Now, after reading all the lengthy details related to the models, observations and the equations, the reader has already forgotten the whole overview of the cyclone itself.

2. Related to comment 1, the study as a whole is a very long read. The paper in its current form has > 11 000 words (from Abstract to Acknowledgements), 13 figures

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and altogether 38 pages (+ additional seven pages in the supplementary material). Although to my knowledge WCD does not explicitly limit their manuscript lengths, the longevity of the paper did make me lose my interest in reading through it at the first time. For reference, AGU journals (<https://www.agu.org/Publish-with-AGU/Publish/Author-Resources/Text-requirements>) are recommending up to 25 publication units (PU) for their manuscripts, where 1 PU is 500 words or 1 display element (figure or table). With the 13 figures and 2 tables, the current manuscript would be left with 15 PUs for the main text, which would correspond to 7500 words. The current length is now > 50 % longer. Please consider condensing the paper. I think the best way to do this would be to focus clearly only on objectives 1 and 2 (as suggested in comment 1), or expressing really the main results from points 1, 2 and 3 in a much more condensed way.

3. The manuscript is full of acronyms which are mostly related to model names. I think that the abundant use of acronyms was one of the reasons which made my reading less enjoyable. In the abstract, you call the models with names CNRM-CM6-1 and IPSL-CM6A. In the main text, these models are referred mainly to with their atmospheric components (if I understood correctly) called LMDZ and ARPEGE. Again in the summary you change to CNRM-CM6-1 and IPSL-CM6A. This was very confusing for a reader who is not familiar with these models. The inclusion of LR or HR in some places makes the names even longer (e.g. IPSL-CM6A-LR(-HR)). I strongly suggest to use short and consistent names throughout the whole manuscript, e.g. CNRM and IPSL, with possible -LR and -HR suffixes.

Minor comments

1. Title: I have a feeling that the part of the title “How Well do Models Represent the Development of Extra-Tropical Cyclones?” is a bit too vague, given that you have investigated only two models and one cyclone. Please consider having a more specific title.

2. ‘L4 and thereafter: CMIP5 and CMIP6 should be written together, and not “CMIP 5”

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or “CMIP 6” (see e.g. <https://pcmdi.llnl.gov/CMIP6/>).

3. L4: Can you write down both resolutions explicitly, or leave both away? Writing only one resolution with its numerical value (0.5° , HR) left me missing the other one, because at least I am not aware of the CMIP6 native resolution (LR).

4. L84: Section 2 title “NAWDEX IOP 6: The Stalactite Cyclone” could be more descriptive, e.g. The development of the Stalactite Cyclone, or The life cycle of the Stalactite Cyclone. 5. L89: I was confused about the word Diabatic Rossby Vortex (DRV), because Boettcher and Wernli (2013) talk about Diabatic Rossby Waves (DRW). If they mean identical phenomena, please consider adding a clarification where you indicate that they mean the same.

6. L124: Why do you use ECMWF operational analyses for comparison, and not ERA5 reanalysis? Isn't ERA5 considered more reliable because more observations have been assimilated into it? However, if you have strong rationale to use ECMWF analyses, please include it in the paper.

7. L243: It was a bit unclear for me why you split the diabatic heating into components here, because in the results (Sect. 4.4.1) you only talk about the omega/baroclinic conversion due to diabatics and not these single components. 8. L245: Isn't latent heating including also the freezing and melting of ice droplets in the clouds, but you mention only condensation and evaporation. Why?

9. L248: You mention that on average most of the modelled vertical motion is recovered using QG method. However, in Figure 7, there are some quite large discrepancies between Model and Inverted baroclinic conversion. How confident are you with the contributions of dynamic and diabatic processes if their sum (as indicated by Eq. 5) does not match well with the modelled baroclinic conversion? Did you verify how well the diagnosed omega (from QG equation) matches with the omega from the climate models? If yes, please consider adding a couple of sentences about the verification.

10. L252: Why do you express the omega equation twice? I think this is unnecessary and removing Eq. 2 would shorten your manuscript.

11. L416: How can you say that the larger ascents mainly arise from diabatic processes? For me it's very difficult to conclude that based on only the figure. Do you have some quantitative analysis behind your statement? In any case, it would be interesting to see some quantitative values related to how much the increased resolution increases the omega due to diabatic heating and the omega due to dynamics. For example, something similar as in Table 2 in Sinclair et al. (2020) (<https://wcd.copernicus.org/articles/1/1/2020/>)

12. Fig 1a, 2a and 7: please could you add horizontal (and vertical) grid lines so that the days are easier distinguishable from each other.

13. Fig 1a: in addition to the grid lines perhaps you could add shadings and small labels to the graph so that initiation and deepening phases are easier visible? And to my eye, the deepest phase of the cyclone seems to be about 6 hours earlier than what is indicated with the dotted line.

14. Fig 1d: The labels in the thickest black contours are not seen properly. Furthermore, there seems to be some gaps in the contours in the middle of the domain.

15. Fig 1e: The colorbar and its labels are too small.

16. Fig 9: Can you add the titles (Total, omega_diab and omega_dyn) to the plots. It would be easier to interpret the plot when you don't need to check from the caption what the different rows express.

Finally, it would help the reviewers if the figures were included within the main text and not at the end of the manuscript.

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