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Interactive comment on "The characteristics and structure of extra-tropical cyclones in a warmer climate" *by* Victoria A. Sinclair et al.

Anonymous Referee #1

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This paper uses Lagrangian tracking to explore extratropical cyclones structure in an aquaplanet: for an SST distribution that gives a global climate similar to present day and then a second time with a 4K addition to all SST. The study finds that storms in the modeled warmer climate have a larger contribution to the generation of their dynamical strength from processes associated latent heating. This is a result that has been shown before, but not with this type of model. That being said, this manuscript can still be judged to advance the field, because of the detailed analysis of the cyclone circulation.

Overall, I think the work is well written. I also think it could be improved if the placed their results in the context of existing literature. I also have multiple minor recommendations that I hope will be addressed.

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Discussion paper

Minor #1: I think the authors need discuss the signal-to-noise issue in greater detail. The signal being the change in storms with warming, and the noise being interannual and intraseasonal variability (as partially discussed in: Harvey et al. GRL 2012).

My sense is that a +4 SST warming represents something like how the midlatitudes will look in 150 years or so, but perhaps that is optimistic? From my eye-ball comparison of your zonal mean changes, with that of Lu et al. 2008 (because I had it on hand, and I did not want to page through the entire report that you cited – Collins et al. 2013) – it looks like your model is warming in the troposphere by 2 - 3 more degrees than theirs. My point being, it would be good to express some idea of what your models warming signal is akin to timing wise – rather than just saying that the zonal mean changes look similar to the CMIP5 models. Or, another way to express my comment is: what time horizon do you expect that we will be able to observe changes like those shown in the paper? I think information like this would make the paper more applicable.

Also relevant: for some of the results, the signal you are finding is in the top 200 events. What do things look like if you use a larger set? I think it is fine to report on the changes in the extremes, but then in the discussion, or elsewhere, I would like you to contextualize this, in terms of your analysis method.

Lu, J., G. Chen, and D.M. Frierson, 2008: Response of the Zonal Mean Atmospheric Circulation to El Niño versus Global Warming. J. Climate, 21, 5835–5851.

Harvey, B. J., Shaffrey, L. C., Woollings, T. J., Zappa, G., and Hodges, K. I. (2012), How large are projected 21st century storm track changes?, Geophys. Res. Lett., 39, L18707, doi:10.1029/2012GL052873.

Minor #2: Things that might need to be mentioned in the conclusions, or considered for further study: 1) With global warming projections, it seems likely that differential changes in land-sea temperatures (or local cooling of SST in certain regions) will have important impacts on the dry dynamical forcing of the cyclones. This cannot be captured with an aquaplanet. Thus, perhaps, these results are mainly relevant to cyclones

in the middle of the ocean. Which is fine, but it needs to be mentioned. 2) Climate models are known to have biases in the representation of clouds and precipitation (raining too frequently at weak rain rates and not frequently enough at strong rates), however, they capture the bulk impact of precipitation. So how confident are you in the model results, especially those related to precipitation and vertical velocity.

Relevant to comment #2 and Section 6.5: One step you can take, to provide more context on your model result relative to existing studies, is to examine the asymmetry (e.g., Tamarin-Brodsky and Hadas, 2019, and citations therein), in the vertical motion in the model's current climate and SST+4K climate.

Tamarin-Brodsky T, Hadas O. The Asymmetry of Vertical Velocity in Current and Future Climate. Geophys Res Lett. 2019 Jan 16;46(1):374–82.

Minor #3: Are you sure that you are not finding some tropical cyclones? Your procedure for excluding them does not seem too robust. I know from experience with SLP trackers, that such an approach would not exclude all tropical cyclones. Perhaps it is more straight forward with relative vorticity trackers?

Line-by-line

Line 43: There are also studies of the cloud and precipitation structure taken from satellite retrievals. Given that reanalysis is still reliant on a model for precipitation and cloud physics, I think it is important to keep studies like these on people's minds:

Naud, C. M., A. D. Del Genio, M. Bauer and W. Kovari, 2010: Cloud vertical distribution across warm and cold fronts in CloudSat-CALIPSO data and a general circulation model. J. Climate, 23, 3397-3415.

Naud, C. M., J. F. Booth, Lebsock, M. and M Grecu, 2018: Observational Constraint for Precipitation in Extratropical Cyclones: sensitivity to data sources. Journal of Applied Meteorology and Climatology, 57, 991–1009, https://doi.org/10.1175/JAMC-D-17-0289.1

Line 55: Some other highly relevant paper on this topic. These could be useful both in the introduction and, perhaps, in the conclusions.

Champion AJ, Hodges KI, Bengtsson LO, Keenlyside NS, Esch M (2011) Impact of increasing resolution and a warmer climate on extreme weather from Northern Hemisphere extratropical cyclones. Tellus 63A:893-906

Yettella, V., & Kay, J. E. (2017). How will precipitation change in extratropical cyclones as the planet warms? Insights from a large initial condition climate model ensemble. Climate Dynamics, 49(5-6), 1765–1781. https://doi.org/10.1007/s00382-016-3410-2

Michaelis, A. C., Willison, J., Lackmann, G. M., & Robinson, W. A. (2017). Changes in winter North Atlantic extratropical cyclones in high- resolution regional pseudo–global warming simulations. Journal of Climate, 30(17), 6905–6925. https://doi.org/10.1175/JCLI-D-16-0697.1

Line 185: Can you briefly discuss the level of agreement between the omega that you estimate and that which is calculated by the model? I am curious to know how far off they are. Related to this, in Section 365, are your discussing the model-produced omega or that which you estimate from this equation?

Line 223: From the figure, it is difficult to see what is occurring for low-level baroclinicity (e.g., below 900 hPa), because of the contours. Is it getting stronger or weaker near the surface?

Line 225: Be careful in how about how you state this. I agree that stratospheric baroclinicity is likely to have a small role. But near the tropopause things are less clear, see for instance: Yuval, J. and Y. Kaspi, 2016: Eddy Activity Sensitivity to Changes in the Vertical Structure of Baroclinicity. J. Atmos. Sci., 73, 1709–1726

Line 270: This latitude shift is going to have a big impact on the precipitation (e.g., Booth et al. 2018). It seems a bit confusing that the storm track as a whole shifts polewards, but the strongest events initialize closer to the equator. This likely relates to

the added role of moisture in driving the strength?

Booth, J. F., Naud, C. M., & J. Jeyaratnam, 2018: Extratropical cyclone precipitation life cycles: A satellite-based analysis. Geophysical Research Letters, 45, 8647-8654.

Line 501: Tierney et al. (2018) also documented this shift. My question: can you speculate on what impact this for people, either on the hazards created by the cyclones or the interactions between the cyclone mid-level and upper-level circulation. Can we speculate that in a warmer world the storms mid-level disconnects from the upper-level – which could have a big impact on storm behavior? Isn't this what Tierney et al. and Kirshbaum et al find with their baroclinic wave studies? Or are is this change in the structure of the cyclones in warming simulations just a curiosity that is of interest to the dynamics community? I think it is the former, but things like this should be stated and discussed a bit so if you want the paper to reach outside the cyclones community.

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