Review of "The relationship between extra-tropical cyclone intensity and precipitation in idealised current and future climates" by Sinclair and Catto submitted to Weather and Climate Dynamics

General comments

This study investigates the relationship between extratropical cyclone intensity and precipitation in (aquaplanet simulations of) current and future climates – a topic that is still not fully understood and thus important to be investigated. The main novelty is that the authors carefully look at this relationship in four interesting groups of cyclones, which have been obtained by clustering the precipitation fields of the cyclones, and for all these different idealized climates. The manuscript is very well written over large parts, the figures are mostly very clear and illustrative, and the use of the different methods and tools are well introduced and motivated. While I found many findings and sensitivities of these cyclone types interesting and inspiring, I think the study gives too much the impression that cyclone intensity is only driven by latent heating, because it hardly mentions all the other dynamical drivers, which, however, are crucial to be included when considering cyclones in a future climate. Therefore, I suggest to give more insight into these aspects, which elaborate more in first two major comments. Note that I did not read the first review, which has been posted a week ago, until finishing my own review below.

Major comments

In my opinion, the manuscript lacks a discussion on how the three-dimensional background state in the control experiment is characterized and how it changes in the SST4 and AA experiments. Although the focus of this study is on changes in ETC precipitation, some rather strong conclusions are drawn with respect to the influence of precipitation and thus diabatic heating on ETC intensity by only considering the linear relationship between the two variables but without consulting the potential role of all other (dry-)dynamical drivers (apart from the upper-level jet). For instance, O'Gorman and Schneider (2008) showed how eddy kinetic energy (and thus ETC intensity) in aquaplanet climates scales linearly with mean available potential energy (MAPE), which is proportional to the vertically integrated meridional temperature gradient, static stability, and the depth of the extratropical troposphere. I assume that in your climate change experiments these individual measures change in different ways and thereby influence MAPE and thus ETC intensity in various, partly counteracting ways – as it is the case in full GCM projections. I think, knowing about these changes, at least qualitatively, would be important to understand the changes in ETC intensity in your study more holistically. For instance, one of your conclusions is (this appears at several locations of the manuscripts): "In our warmer experiments, precipitation strongly increases but there is not any large increase in the intensity of ETCs i.e. the slope is larger in SST4 than in the control. This implies that the increased diabatic heating does not feedback strongly onto the intensity of the ETCs in these experiments." However, other studies have shown that increased latent heating / precipitation itself actually does or would lead to a further increase in ETC intensity via the well-known mechanisms, but this is balanced by the counteracting effect of reduced MAPE (e.g., Büeler and Pfahl, 2019). So, couldn't it be that your increase in precipitation in SST4 itself actually does increase the vorticity strongly linearly, but the increase is partly dampened by other counteracting factors not seen from this linear relationship, which is why the slope changes?

I am not very familiar with the full-GCM scenarios regarding Arctic amplification. Therefore, I wonder how realistic the prescribed SST setup in your AA experiment is. When I make up the meridional gradient in my mind from your SST curves in Fig. 1, it seems that in your AA experiment the lower-tropospheric meridional temperature gradient does not really decrease but the zone of the strong gradient is just narrowed and shifted equatorward. Is that correct? And, if yes, don't the full-GCM polar amplification scenarios rather show a flattening of the curve and thus really a reduction of the gradient? I am not saying that your AA setup should be as realistic as possible, but I think you should better discuss how your setup influences the background state (because change of ETC intensity seem to be quite sensitive to how exactly baroclinicity, both in the lower and upper troposphere, and temperature/moisture change; e.g., Tierney et al. 2018) and how it compares to the "real" polar amplification.

Did you also investigate the whole cyclone life cycle characteristics in the three experiments in more detail? For instance, did you look at potential changes in cyclone lifetime, intensification rates, propagation speed, or accumulated precipitation over their lifetime? Thinking of impacts, particularly the latter two measures might be interesting, considering the fact that extreme precipitation is often caused by very stationary cyclones. Of course, this is often related to topography, but it might still be interesting to see whether these characteristics differ even in aquaplanets. This comment is more meant as a nice-to-have and is not crucial for the paper.

Minor comments

L11: What do you mean by "differing slopes"?

L79-80: Do you refer to the frequency distribution of ETC intensities here? In other words, you say that the relative frequencies of different intensities do not change, right? You could write that more specifically.

L136: What exactly does "QObs" mean?

L150: Out of curiosity, are there many ETCs that do not at all propagate in your aquaplanet simulations (i.e. the ones you filter out)? I would assume, given the fact that no topography is there to generate standing waves and no land is there to produce, for instance, heat lows, most ETCs propagate, right?

L213: I assume it's global mean surface temperature, right?

L224-225: I think it's interesting that you find most ETCs in the AA experiment, in which, considering its background state, less eddies would be needed to transport heat poleward. Do you think the higher number of ETCs is just (over-)compensated by their weaker intensity (which would result in an equal or even weaker poleward heat transport)?

L236-237: What about the latitude distribution during maximum intensity? Is there also an equatorward shift in AA, as for genesis and lysis? Does this shift result from the equatorward shift of the baroclinic zone in this experiment? And how does this response compare to polar amplification scenarios of full GCMs? (see also my second major comment)

L258: The last part of this long sentence ("ETCs with the same intensity...") is not clear to me – can you rephrase?

L274: What does the "s" mean in "(mm / 6 hr) s"?

L277-279: "Thus, we can conclude that there is a stronger relationship between maximum vorticity and precipitation in the SST4 experiment than in both the control or the AA experiment and also that the AA experiment has a stronger relation that the control." I think this statement is misleading, because a larger slope does not imply a stronger linear relationship, right? What determines the strength of the linear relationship is the r-value, which is actually smallest in SST4, largest in AA, and in between in Control. Hence, strictly speaking, your sentence above is not correct. However, as you explain in the subsequent sentence, it is about how much precipitation changes with a given vorticity change. So I would just rephrase the sentence above somehow (plus at a few other locations in the manuscript, where you write about the “strength of the relationship” referring to the slope).

L279-282: “This means that for the same increase in maximum vorticity, precipitation increases more in the SST4 and AA experiments compared to the control. Since the SST4 and AA experiments are both warmer than the control experiment this is to be expected based on the Clausius-Clapeyron relationship between temperature and vapour pressure.” Is this really that obvious / to be expected? If only the C-C relationship would matter, then also the weak cyclones (low relative vorticity) would need to have equally enhanced precipitation, which would imply that the slope should stay the same, but the intersect would change, right? So, maybe one could also interpret the increased slope such that the stronger cyclones are better able to convert the enhanced moisture content into enhanced precipitation than the weaker cyclones (likely due to the stronger dynamically induced forcing). Does that make sense?

Fig. 4 (and related figures): Could you change the color of the SLP contours? It’s very hard to distinguish them from the black TH@850 contours.

L288: Did you consider just showing equivalent potential temperature instead of potential temperature and total column water? It might be the better variable to show temperature distribution, the sharpness of the fronts, and the moisture availability at once. But it’s a matter of taste, I guess.

L293: It’s interesting to see these four clusters. Do you know about similar clusters in reanalysis? If yes, do they look similar and could you refer to these studies?

Fig. 6: I very much like to see the upper-level structure of the different clusters. Just for my understanding, could it be that some jet maxima we see in this figure are related to the subtropical jet? This also relates a bit to my first major comment regarding how the background state looks like on average.

L305-306: You might also add here that the upper-level PV values downstream are lowest in this cluster, probably because of the strongest diabatic erosion of PV downstream due to the WCB ascent.

L348-349: Do you know why the cold front ETCs (in the control) are more south than the warm front ETCs? Is this also the case when comparing climatologies of Norwegian cyclones with Shapiro-Keyser cyclones? I assume thanks to this fact, the cold front ETCs can draw more from the subtropical moisture reservoir and thus become more intense with more precipitation than the warm front ETCs, right?

L352: I think there is something off with the sentence “The mean latitude that ETCs in the centre ETC cluster reach their maximum vorticity…”

L367-368: Regarding the fact that the tropopause level might change in the different simulations (see also my first major comment), did you also look at other vertical levels when investigating upper-level PV?

L397: Which one is the “small cluster” you mention here?

L415-419: I find the strongest correlation between vorticity and precipitation in the warm front cluster interesting, but I think I don't understand the reason. I would have expected it to be stronger in the cold front cluster. Do you have an explanation for this?

L507-510: "The cold front ETC sees an increase in its maximum vorticity with uniform warming whereas both the warm front and cyclone centre ETCs show no change in the maximum vorticity and the maximum vorticity of the weak ETC decreases." Isn't this somehow contradictory to the aforementioned fact that the vorticity-precipitation relationship is strongest and with the largest slope in the warm front ETCs, which get much more precipitation in the warm experiments? Or does this just show that we cannot understand everything from just looking at these two variables (see also my first major comment)?

Typos / suggestions for rephrasing

L32: Replace "Even" with "Already"?

L33: Comma after "ascending"

L40: Replace the second "using" with "based on"

L70: Rephrase to "that the number of ETCs associated with extreme precipitation may triple"

L72: Comma or semicolon after "simulation"

L95: Delete "will be on"

L108: "setup" rather than "set up"?

L126: Write consistently either "aquaplanet" or "aqua-planet".

L141: "those previous simulations"

L167: "due east" -> "to the east"?

L238: Comma after "Lastly"

L265: Rephrase to "The same as Figure 3 but for precipitation 24 and 0 hours before the time of maximum vorticity are shown in the Supplementary material."

L279: "… stronger relation than the control."

L286: "… than in other simulations."

L314-315: Delete "associated with them"

L327: Delete "associated with it"

L342: "whereas the weak ETCs have very different shaped distributions"

L354: Delete "associated with them"

L389: Delete “associated with then”

L396: “)” after “Fig. 10”

L397: “with the weak ETC cluster”

L425: Delete “associated with them”

L493: Typo in “vorticity”

References

Büeler, D., and Pfahl, S. (2019). Potential Vorticity Diagnostics to Quantify Effects of Latent Heating in Extratropical Cyclones. Part II: Application to Idealized Climate Change Simulations. Journal of the Atmospheric Sciences 76, 1885-1902, https://doi.org/10.1175/JAS-D-18-0342.1

O’Gorman, P. A., and Schneider, T. (2008). Energy of Midlatitude Transient Eddies in Idealized Simulations of Changed Climates. Journal of Climate 21, 5797-5806, https://doi.org/10.1175/2008JCLI2099.1

Tierney, G., Posselt, D.J. and Booth, J.F. (2018). An examination of extratropical cyclone response to changes in baroclinicity and temperature in an idealized environment. Climate Dynamics 51, 3829–3846,. https://doi.org/10.1007/s00382-018-4115-5