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Comment on wcd-2022-62

Anonymous Referee #1

Referee comment on "The relationship between extra-tropical cyclone intensity and precipitation in idealised current and future climates" by Victoria A. Sinclair and Jennifer L. Catto, Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2022-62-RC1, 2023

Review of WCD-2022-62: “The relationship between extra-tropical cyclone intensity and precipitation in idealised current and future climates” by V.A. Sinclair and J. L. Catto

Summary

In this study, the authors perform global aquaplanet simulations of extratropical cyclones (ETCs) in current and warmed climates. They use a feature-tracking algorithm to diagnose the ETC life cycles and a clustering algorithm to group the ETCs according to their precipitation structures. Through this approach, they evaluate differences between four clusters in both current and future climate, particularly in terms of the relationship between relative vorticity and precipitation amount. They conclude that the types of ETCs for which this relationship is the strongest are the ones where most of the precipitation is concentrated along the warm front. They also argue that stronger diabatic heating in warmer climates, owing to increased atmospheric moisture content, does not feed back onto the dynamical intensity of the storm.

Altogether, I found this manuscript to contain interesting analyses that make excellent use of various meteorological tools (GCMs, cyclone tracking, and clustering) to quantify ETC

variability in current and future climates. For the most part, the results are presented clearly and supported by figures and tables. Given that this study is mostly diagnostic and descriptive, with little dynamical interpretation of the results, such interpretation would be needed to translate this result into improved dynamical understanding. But I can accept that a proper interpretation can wait for future work.

For the present work, I believe that there is scope for some improvement, to address lack of clarity or conclusions that, at least in my view, are not justified based on the data presented. In the following, I highlight those areas in an effort to stimulate improvements to the manuscript.

Major comment

- As mentioned above, the authors conclude that "increased precipitation in the warmer simulations does not feed back, via diabatic heating and potential vorticity anomalies, onto the dynamical intensity of the ETCs." This is a rather provocative conclusion that isn't based on a very rigorous analysis. They argue on P. 9 that, if diabatic heating did feed back on the dynamics, that "similar slopes would be found in all experiments."

Although I agree that the slope would be shallower (and more like the control) if there was a positive feedback of diabatic heating on cyclone intensity, how can you conclude that the slope would be the same? This would seem to imply that diabatic heating is the only factor affecting cyclone strength. Is it possible that other factors could also influence this relationship, and that these factors may also change from the control to the SST4 cases? Because the only input that changes is the SST, one may conclude that the only dynamical difference is the diabatic heating. That may be the case, but without a deeper analysis I'm not entirely sure of that. Therefore, I think the authors should reconsider the strength of this conclusion, or find a stronger justification for it.

- Another possible overstatement is in the abstract on L. 14, where the authors conclude that "precipitation patterns associated with ETCs are unlikely to change in the future". The results do suggest that the mean of the different composites will not change in the future, but you haven't looked at higher-order aspects of the distribution (variance, skewness, etc.). Again, while this statement may ultimately be true, I think the level of certainty of the conclusion is a bit greater than the justification provided by the data.

Minor comments

- P. 1, L. 11: it is unclear what the term "slopes" is referring to.

- P. 2, L. 45-46: "can potentially feedback onto the intensity of the ETC by enhancing the low-level circulation and thus the intensity of the ETCs.". This is clearly redundant, so delete "and thus the intensity of the ETCs".

Also, feedback should be "feed back"

- L. 139: It would have been a bit cleaner to increase the SSTs at higher latitudes by 4C,

for consistency with SST4. I'm not saying this needs to be changed, but it makes comparison between SST4 and AA a bit more complicated than it needs to be.

4 L. 143-144: What is meant by the vague statement "did not modify the surface pressure"? A bit more specificity is needed. I'm assuming you are referring to accounting for topography, but it would help to clarify that.

- L. 160-161: "Previous studies have often selected…". You say that previous studies have done something, but then you don't cite any actual studies. Your point would be more convincing if you cited a couple papers to support your claim.

- L. 172-174: Another way to evaluate this is to look at the variability of parameters within each composite (i.e., higher-order moments of the distributions).

- L. 184: I think you should clarify that your interest in grouping ETCs based on precipitation structure (rather than intensity) is limited to the clustering analysis. You are actually very interested in the absolute amounts in later sections, so this can be misconstrued.

- L. 214-215: This comparison is too vague. You say that the global mean precipitation rate in your simulations is larger than the one on the "real Earth". But how much larger? An order of magnitude, 10%, or something in-between? This is useful

information for assessing the level of realism of these simulations.

- L. 224: 10 samples is a very small number, and it likely downplays the statistical significance of these results. Since there's no annual cycle, why is it necessary to make the sampling interval one year? You could choose seasonal means and get many more samples. It would be interesting to see if the significance changes if you change the sampling rate.

Also, doesn't the t-test require specification of a confidence level (usually 95%), beyond which the results are considered significant? If so, what is that level?

- L. 242-243: I don't follow this text: "The control simulation has more ETCs with weak to moderate precipitation (2 - 3 mm / 6 hr) than either the SST4 or AA simulations, potentially as, on average, the control is the coldest simulation.". The "potentially" part is confusing.

- L. 252-253: The authors discuss a certain analysis here (called ANCOVA) and report the results. But no results are shown. Is this intentional? This is one place where the reader is unable to verify the result. Is there more information available that can make this a bit more convincing?

- L. 257-258: To help the reader, can you clarify which one is more efficient at producing

precipitation for a given relative vorticity? You say there is a difference, but you don't indicate which one is larger.

- L. 287, "This is likely due to the warmer atmosphere (Table 1) and more diabatic heating in this case than the control or AA simulations." I don't understand the authors' logic here. Why does more diabatic heating weaken the relationship between vorticity and precipitation in SST4? Earlier it was noted that diabatic heating does not seem to feed back much on vorticity, but that is an empirical finding that doesn't physically explain the result. Now the authors seem to be attempting to pose a physical explanation, but I'm not sure what that is.

- L. 327-328 and Fig. 4: I can't verify the authors' conclusion here, because the MSLP contours are very difficult to see in Fig. 4. The figure is too busy with numerous contours on it. Are the range rings really necessary? If not, I strongly suggest removing them.

- L. 353-354, "The distributions of latitude of the maximum vorticity also differ in a

statistically significant way." It is unclear if you are referring to all ETCs, or just the weak category that you were referring to in the previous sentence.

- L. 368, "The minimum MSLP is 1-3 hPa deeper". Technically, the word "deep" is used to describe the cyclone centre. But here you are referring to the "minimum MSLP", for which the correct comparison word is "smaller" or "larger".

- L. 371: I don’t think the term “dry dynamics” is relevant when discussing moist midlatitude cyclones. The dynamics are a mixture of dry and moist, and I’m not sure if you can be certain about one or the other at this

- L. 372: Here a “weaker jet streak” is mentioned in a paragraph where no figures are cited. At times, the authors do not cite relevant figures and force the reader to do some detective work in attempting to verify their claims.

- L. 397: This is the first I’ve heard of a “small” cluster. Which of the four clusters are you referring to here?

- Figure 11: Throughout this paper, the cluster discussions have always referred to the four clusters in a certain order. Here, the order has been changed, as warm-front now goes before, rather than after, the cold-front cluster. Unless there is a need to do this, it would make sense to keep the ordering consistent throughout.

- L. 412: It may also be worth noting here that the increase in weak ETCs does not come at the expense of stronger ones. There are just more cyclones, but most of the extra ones are weak.

22: L. 614: This reference to Owen et al. does not provide sufficient information. To what journal is this “in prep” paper being submitted. If you haven’t even decided upon that, it’s probably premature to be citing it. A conference presentation may work better here.

Technical corrections

- L. 95: Delete “will be on”

- L. 108: set up -> setup

- L. 109: analysis -> analyse

- L. 343-345: This sentence would fit better in the subsequent paragraph. In terms of the logical flow, it is currently out of place.

- L. 345-349 and Figs. 7 and 8: the panel labelling seems off in these figures and in the citations to them in the text, making it difficult for me to follow the authors' argument.

- L. 389: then -> them