

# Response to reviewers - The relationship between extra-tropical cyclone intensity and precipitation in idealised current and future climates

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## 1 Editor's comments

Thanks for the detailed responses and comprehensive revisions of your manuscript. Your paper has been assessed once again by the two reviewers and both recommend publication in WCD. One of the reviewers has a few minor suggestions that I'd ask you to take into account when preparing the final version. In addition, please also consider the following very minor points:

1. Line 46 ("This diabatic heating..."): I think it would be nice to support this sentence with a reference, potentially to one of the earlier papers on this linkage, such as Stoelinga (1996). [We have added a reference to Stoelinga \(1996\)](#)
2. Line 67: Held and Soden didn't really look at extremes; maybe add another reference, such as Allen and Ingram (2002). [We have changed this reference as suggested](#)
3. Line 76 ("large ensemble of climate model simulations"): It would be more precise to call this a "single model initial condition large ensemble", just to make clear that it is not a CMIP-type analysis. [This is now revised to read "analysed a 30-member initial condition climate model ensemble"](#)
4. Line 135: "extrapolated" instead of "interpolated"? [Yes, this is more accurate and we have changed it.](#)
5. Line 166: Is there any reason for only looking at the Northern Hemisphere? Taking into account both hemispheres would have been a cheap way to increase your sample size. [No, there is no major reason for only considering one hemisphere. One very small reason was that one of the metrics we used to determine if the simulations had spun up into balance was the difference in the total precipitation between each hemisphere \(which should be close to zero\). Furthermore, we felt our sample size of >10,000 cyclones in each experiment was large enough and it was just computationally simpler to run the simulations for 10 years rather than just 5 and consider two hemispheres.](#)
6. Line 250: I'd find it helpful to specify that you're referring to the "horizontal" temperature gradient. [We now write the "meridional temperature gradient" here.](#)

7. Line 320, Figure 4: Are the units really correct, or is there a factor of  $10^5$  missing?. [Yes, you are correct. We have corrected the units here and elsewhere throughout the manuscript.](#)
8. Line 528: "significant" instead of "significantly different". [Revised as suggested.](#)
9. Fig 3: Consider to use different symbols or line styles to make the figure more accessible for color-blind readers. [We have updated Figure 3 so that each experiment has a different line marker in addition to be a different colour.](#)

## Reviewer 2

### General comments

I thank you for considering and carefully replying to all the reviewers' comments. I think the (very interesting!) additional analysis of the changes in the mean atmospheric state now greatly "round off" the manuscript and make many results regarding the linear relationship between vorticity and precipitation better understandable/interpretable. Therefore, I suggest to accept the revised manuscript after considering the following few very minor comments that all refer to how things are phrased (the line numbers below refer to the line numbers in your tracked-changes document). Note that particularly in the new text passages there are still quite a few typos, of which some I mention below.

### Minor comments

1. L5-8: I think it's great that you now discuss this aspect (i.e. similar vs. changing slope, and the potential role of changes in the background state) so clearly throughout the manuscript, but I wonder whether this "hypothesis" sentence is really at the right location here in the abstract? Could you mention this hypothesis later, after the finding that the slopes slightly change and what this might mean (see next comment)? I'm not sure what is better, but in the current form I find the sentence a bit isolated, because later you don't really refer back to this hypothesis... [We have now moved this hypothesis sentence to the start of section 5 and also refer to it later on in section 5.](#)
2. L13-14: This sounds very technical now for an abstract ("slope of the linear regression line is statistically larger"), and, moreover, not unambiguous, as it's not clear which slope you actually refer to. Why not writing something like: "The amount of precipitation for ETCs with a specific vorticity is higher in the uniform warming and polar amplification simulations than in the control simulation (i.e., the slope of the linear regression between vorticity and precipitation is larger), indicating that changes...". Furthermore, could you add the explanation for this conclusion, i.e. the hypothesis mentioned earlier, right here instead (see previous comment), and then mention the potentially additional processes that might compete with diabatic heating (i.e., reduction in baroclinicity, which you currently don't mention at all in the abstract)? I think this would make it easier to follow the line of argumentation what a change in slope could mean. I hope you understand my suggestion... [We have edited this part of the abstract to include the mention of competing processes, and to make it less technical sounding, as suggested. However, the suggested line "The amount of precipitation for ETCs with a specific vorticity is higher in the](#)

uniform warming and polar amplification simulations than in the control simulation” is not quite what we meant here, since this does not necessarily mean that the slope will be larger.

3. L19: You both use “dependence” and “dependency” in the abstract, so I would consistently use only one. [We have revised this to only use “dependency”](#)
4. L21: Typo “voricity” [Corrected](#)
5. L26: Comma before “whereas” [Added](#)
6. Figure 2 captions: Maybe write “difference between SST4 and control” (i.e., not just in brackets) [We already write something very similiar: ”Shading shows the difference in the zonal wind speed between the control and SST4 simulation”](#) and therefore we have not made any revisions here.
7. L256: Change to “and causes a decrease in the Eady...” [Sorry this was a typo and is now revised as suggested.](#)
8. L258-259: Maybe change to “rather than a decrease in the meridional temperature gradient, which barely changes”. [Revised as suggested.](#)
9. L259-261: I think you should help the reader to see where (and if) the lifting of the tropopause can be seen in these figures. I assume you could kind of see it in the changes in the Brunt Väisälä frequency, right? But how exactly? Is the tropopause basically going up by the vertical extent of the negative Brunt Väisälä frequency anomaly? At least, this lift is not obvious to me at first glance...[We have now indicated this is the previous line when mentioning the lifting of the jet.](#)
10. L261: “the jet to move equatorwards” [Revised as suggested.](#)
11. L264: “related to a decrease” [Revised as suggested.](#)
12. L321: “largest slopes and correlation coefficients occur”; furthermore, I guess you refer to Figure 4 when you write “The same as Figure 3, but...”? [Occurs has now been changed to occur and the reference to the figure has been corrected.](#)
13. L345 and L347: Change back to “feedback” as it’s a noun there. [Revised as suggested.](#)
14. L346-349: It’s nice that you now include this sentence here, but it is a bit “heavy” to read and not very specific. In fact I liked the wording you used in your response document more, when you said something like “the increasing slope might still indicate an increased diabatic feedback on vorticity, which, however, might be masked by the counteracting reduction in Eady growth rate” – > I think the use of “mask” or something similar might be helpful here. . . [Thank you for the suggestions. We have now rephrased this sentence to make is clearer and easier to read.](#)
15. Figures 5 and 6: Did you leave the range rings here on purpose, although you removed them in Fig. 4? I think you could just remove them everywhere. [We think it is good to give an idea of the spatial scale on these figures and since Figures 5 and 6 are not too crowded we decided to keep them here.](#)
16. L495: “in the number of weak ETCs” [Corrected](#)

17. L509: “compared to cold front ETCs” We have left this as is as we think it is correct.
18. L542: Something is off with “... as with uniform uniform these ETCs are less...” This has been revised to read ”as with uniform warming”
19. L555: “means that precipitation” Corrected
20. L557-558: “do not act to intensify warm front ETCs” Corrected
21. L560-562: I don’t understand this sentence here, particularly the second part of the sentence “..., the weak ETCs in this cluster see...” What we meant here is that if you compare the best fit linear regression lines shown in Figure 14a and 14c, the left side (small vorticity values) increases (moves to higher precipitation values) more than the right hand side - although this is quite subtle and hard to see from the figures. It can also be computed using the slope and intercepts of both best fit lines. For the cold front cluster in the control simulation the equation of the best fit line is  $TP = 0.176Vo + 2.391$  whereas in the AA experiment the best fit lines is  $TP = 0.172Vo + 2.614$ . These equations show that for a ETC with a maximum vorticity of  $1 \times 10^{-5} \text{s}^{-1}$  the 6 hr precipitation in the AA experiment is 0.215mm larger than in the control simulation. In contrast, for an ETC with a maximum vorticity of  $1 \times 10^{-5} \text{s}^{-1}$  the corresponding value is 0.162 mm.
22. Conclusion: Make sure you stay with one tense, because you start with past tense but then, at least partly, fall into present tense (for instance at L577). We have revised the conclusions section to ensure that we use the past tense throughout.
23. L578: “uniform warming” Corrected
24. L595: “feedback” instead of “feed back” as it’s a noun here, right? Yes, this is a noun so changed to feedback

## References