

Weather Clim. Dynam. Discuss., referee comment RC2
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Comment on wcd-2022-37

Anonymous Referee #2

Referee comment on "Warm conveyor belts in present-day and future climate simulations – Part 2: Role of potential vorticity production for cyclone intensification" by Hanin Binder et al., Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2022-37-RC2>, 2022

Brief Summary: This manuscript analyzes the ability of CESM-LE to represent warm conveyor belts (WCB) as compared to reanalysis. Then the paper examines how WCBs are forecast to change in the future in the CESM-LE.

Overall impression: the paper is well-written in terms of clarity, grammar and intent. I appreciate that the authors lay out their research questions in the introduction and then return to them in the conclusion. I think the methodology is sound and do not see a need to a significant amount of additional work. However, I have some comments about the interpretation of the results and a few questions that will require some extra analysis.

Major Comment

The authors conclude that there is a clear signal of an increase in WCB strength and enhanced WCB-related diabatic heating the SH. I agree with this to some extent, but I would be a bit more cautious in how I would state the results – both in the abstract and in the conclusion section. The reason I say this is because, based on Figure 3e vs Fig 3f, the difference in low-level PV in the core of the ETC composite for ERA-Interim vs CESM-Hist is of a similar magnitude as the difference between CESM-Hist and CESM-RCP85 (i.e., Fig. 7g,h and Fig 8 g,h). This means, if we accept reanalysis as truth, the bias in the model for present day is of a similar magnitude as the projected change in low-level PV.

While I agree that some of the other variables analyzed do not show the same sort of issue – e.g., the climate change signal is larger than the model bias for precipitation, if it were me writing the paper I would be more cautious in how I deliver the take home message about the modeled projections of the WCB and associated diabatically generated PV. Thus, I expect the authors to either add more explanation as to why such caveats are unnecessary, or adjust the language in the abstract and the conclusions to illustrate the amount of uncertainty that the figures appear to show.

Minor Comments

L 55 -80: Somewhere in the introduction, perhaps in this section (L 55 – 80), I think it would make sense to refer some of the studies that have focused on GCM evaluation of extratropical cyclones for the processes and mechanisms that the authors are focused on. For instance, the Catto et al. 2010 work on ETCs in general; the Hawcroft et al. 2015 and Booth et al. 2018 work for ETC precip, and the Riviere et al. 2021 work that focuses directly on the WCB in a global model. My feeling is: discussing these works in the introduction sets context for the GCM evaluation analysis that you will do and shows some of the successes of the models. At the same time, we can't trust the models completely, especially in the southern hemisphere, see for instance, Chemke et al., 2022

Catto, J. L., L. C. Shaffrey and K. I. Hodges, 2010: Can Climate Models Capture the Structure of Extratropical Cyclones?. *J Climate*, **23**, 1621–1635.

Hawcroft M. K., Shaffrey L. C., Hodges K. I., Dacre H. F., 2015: Can climate models represent the precipitation associated with extratropical cyclones? *Climate Dynamics*. 1–17. doi:10.1007/s00382-015-2863-z

Booth J. F., C. M. Naud, J. Willison, 2018: Evaluation of Extratropical Cyclone Precipitation in the North Atlantic Basin: An analysis of ERA-Interim, WRF, and two CMIP5 models. *J Climate*, 31:6, 2345-2360.

Rivière, G., Wimmer, M., Arbogast, P., Piriou, J.-M., Delanoë, J., Labadie, C., Cazenave, Q., and Pelon, J.: The impact of deep convection representation in a global atmospheric model on the warm conveyor belt and jet stream during NAWDEX IOP6, *Weather Clim. Dynam.*, 2, 1011–1031, <https://doi.org/10.5194/wcd-2-1011-2021>, 2021

Chemke, R., Ming, Y. & Yuval, J. The intensification of winter mid-latitude storm tracks in the Southern Hemisphere. *Nature Climate Change* **12**, 553–557 (2022). <https://doi.org/10.1038/s41558-022-01368-8>

L138: You write: "In total, this yields 50 simulated years for each time period."

I interpret this to mean that you are not looking at the data from the different ensemble members collectively, is that correct? I am used to ensembles being used for inter-comparison, but here you use them simply to have more data. That is fine, but a sentence clarifying that would be helpful.

L142: Using 1979 – 2014 for reanalysis as compared to 1990-1999 for the GCM might be an issue, but perhaps not? There are differences in ENSO variability for the two time periods which might influence midlatitude mean state and thereby influence the WCBs and ETCs. Or perhaps not. You discuss this later on, in the section where you discuss the figure, but I suggest moving or adding something explanation right here, where you introduce the models as having been analyzed for different epochs.

L165-166: Why are different methods used for assigning WCB trajectories in ERAi vs CESM? This seems like a possibly crucial issue given how closely you are comparing the results garnered from this analysis. I think the reader would benefit from some explanation of this choice, and some reassurance as to why it should not be an issue.

Line 229: Table 1: It is a bit of a surprise to me that there are more cyclones in the NH than in the SH. Are the SH events longer-lived? There is more storminess in the SH than the NH isn't there? A comparison of the Hoskins and Hodges 2002 and 2005 papers suggests that there is more in the SH. The lack of land masses down there also seems to suggest that there would be more cyclone activity over the southern ocean as compared to the NH. Why do you think you've found a different result?

Hoskins, B. and K. Hodges, 2002: New Perspectives on the Northern Hemisphere Winter Storm Tracks. *J. of Atmos. Sci.*, **59**, 1041-1061.

Hoskins B. and K. I. Hodges,, 2005: A new perspective on Southern Hemisphere storm tracks. *J. Climate*, **18**, 4108-4129.

Figure 2: In panel b, the maximum on the y-axis is different from the other panels. Also, the number values shown in the y-axis are a bit non-traditional. Is there a reason for that?

Figure 2 suggests that the probability of a cyclone reaching bomb strength without much WCB air mass is large. Why do you think that is?

Figure 5: The text on the x-axis looks to have been cut-off at the bottom, e.g., the word Bombs is cropped too much.

Figure 5: In terms of the changes in the cyclone characteristics that relate to SLP. I just wonder if the normalization to a fixed latitude doesn't do enough for the southern hemisphere, where the gradient of the zonal mean of SLP with respect to latitude is very large in some locations. A plot that I would like to see is this:

- A histogram of the latitudes of the cyclone centers in the HIST and the RCP8.5 run on the same plot.
- A plot of the zonal mean of the climatology of the SLP for HIST and RCP8.5

My question on this is because I wonder how much a latitudinal shift in the location of the cyclones, or a change in the SLP climatology impacts a metric like the Bergeron.

L309: In the introduction, and throughout the paper there are multiple references to a projected increase in baroclinicity in the SH, and all of the references point to a single paper. Given the data at the authors disposal, I wonder if it would make the paper stronger if the authors also calculate the change in baroclinicity and include that figure in the manuscript?

End of Comments