Review of "Warm conveyor belts and future climate simulations. Part II: Role of potential vorticity production for cyclone intensification" by H. Binder, H. Joos, M. Sprenger & H. Wernli.

This study aims to investigates how climate change affects WCB strength and the relation between WCB strength and cyclone deepening rate by comparing two climate model simulations which have been re-run with the Community Earth System Model Large Ensemble. The main result is that in the SH both the WCB strength and the cyclone deepening rate increase whereas in the northern hemisphere the WCB strength increases slightly but there are no changes to cyclone deepening rates. The difference between the hemispheres is convincingly explained by the differing responses of low-level baroclinicity to climate change. Overall, the manuscript is interesting, well within the scope of the journal, and clearly written. I have two major comments (details below) that the authors need to carefully consider before this manuscript can be accepted. In addition, I have lots of minor comments which I hope the authors find useful and can be used to improve the manuscript.

Major Comments

1. **Metric to quantify the WCB strength.**

   (a) I appreciate that this metric has been used before but I am not convinced that the number of trajectories accurately tells us how "strong" a warm conveyor belt is for two main reasons: (1) I would expect that a WCB that ascends faster (larger vertical motion) is stronger (more mass flux through a given level), but the current metric does not account for this – trajectories are all assumed to be a WCB as long as they meet the threshold and (2) defining the WCB strength by counting the number of trajectories also means that the geographical size of the cyclone / warm sector can influence the "strength" of the WCB. Point (2) is worthy of careful consideration since some studies have suggested that extra-tropical cyclones, (at least their wind field) is likely to increase in size in the future. Even if these concerns have been considered in previous studies, they should also be included here – there are very few details of this metric presented in this study and adding more information could hopefully remove my concerns about the applicability of this metric.

   (b) The definition of a WCB trajectory – trajectories must ascend 600 hPa within 48 hours. Again, I appreciate that this diagnostic has been used extensively before but mainly in a weather / current climate setting. Now it is used in a future (warmer) climate, I am not sure using a fixed threshold (600 hPa) is valid as the tropopause will be higher in the warmer climate and as such a trajectory needs to ascend a smaller fraction of the troposphere in the future to be a WCB than in the current climate. Can you convince a reader the fixed threshold is still appropriate?

2. **Link between WCB strength and how diabatic a cyclone is**

   (a) In the abstract it is stated "cyclones will be more diabatic in a warmer climate" and again in lines 328-329 where it is stated that a stronger WCB means that cyclones are more diabatic. This is a very strong statement given the evidence presented in this manuscript as I don't think this is such a simple step to go from stronger WCB to more diabatic. For example, the ascent in the WCB is largely driven by warm air advection and usually stronger warm air advection occurs where there is a stronger north-south temperature gradient and more baroclinicity. Therefore, just because there is a stronger WCB does not mean that the relative forcing (between dry dynamics and diabatic processes) changes. To really claim robustly that cyclones are becoming more diabatic, the diabatic temperature tendencies would need to be computed.

Minor Comments

1. Section 2.3: It is not clear why a different approach for assigning WCB trajectories to the cyclones is taken in CESM-LE compared to in ERA-Interim. Please revised the manuscript to justify why this is the case.
2. Section 2.3, lines 176 - 177. It is not completely clear how ΔSLP is computed. Is it computed every 6 hours and over a 24-hour period or only computed every 24 hours, over a 24-hour period? Please clarify.
3. Section 3: Why compare to ERA-Interim which is now quite old? ERA5 has been available for a considerable time now and it would have more appropriate to compare to ERA5. Given the much higher resolution of ERA5 compared to ERA-Interim, I think the low-level PV anomaly associated with WCB in ERA5 is likely larger than in ERA-Interim which would mean the difference between ERA5 and CESM-LE might be very large.
4. Lines 211. “about half of the cyclones do not have a WCB in ERA-Interim and CESM-HIST”. This sentence really confused me to start with and as such I think it is misleading. Those cyclones probably do have a warm, ascending airstream (i.e. a weak or shallow WCB) associated with them but it does not meet the requirements to be defined as a WCB here. This sentence needs to be revised. This comment also applies to line 445.
5. Line 222. Why are the correlations between WCB strength and cyclone deepening rate smaller in the southern hemisphere compared to the northern hemisphere in both ERA-Interim and CESM? This is interesting – is it because SH cyclones are more driven by the low-level baroclinity and less by diabatic processes than in the NH?
6. Line 253, 376, and elsewhere. Units of precipitation. Here in the text it is written mm/h, but in Figure 3 it is mm / 6hr. Please check these. Also the precipitation rates (even if mm/6hr) seem to be a little larger than I would expect in a composite as usually on weather maps of day-to-day maximum values of precipitation in one mid-latitude cyclone are around ~5 mm / h. I’d expect the compositing to average / smooth things out leading to smaller values.
7. Line 269 and line 433 “remarkably well”. This is too strong a statement – it would be more accurate to say there is reasonable agreement. The difference of 0.5 PVU in the intensity of the low-level PV anomaly is, in percentage terms, quite large.
8. Line 278. Is the difference in the number of cyclones statistically significant? This could be tested using the numer of cyclone each winter and comparing the two populations.
9. Line 347. Suggest change “higher” to “larger” as I first though that the WCB peak was moving upwards in the atmosphere.
10. Line 382 – 395, Figure 9. Does the vertical tilt change between CESM-HIST and CESM-RCP85? I think this analysis would be a small but valuable addition to this manuscript since some studies (that you cite in the introduction) have shown that the low-level PV anomaly

moves downstream which limits the coupling and interaction between the low level and upper level anomaly.

Figures and Tables

1. Figure 2. Suggest moving the C1 and C2 labels to a part of the "box" where there is no data as they are hard to see. Also the arrow pointing to the C3 part of the phase space is very easy to miss – can it be made more obvious?
2. Figure 3. Check the units of precipitation (see minor comment #6 above). Add how many cyclones are included in these composites.
3. Figure 4. When printed, it is very difficult to see the grey shading. Can it be made darker or the edges shown by dashed lines?
4. Figure 5. This shows the mean number of each type of cyclone per winter. Could some range of the variability be added to this figure (related to minor comment #8)
5. Figure 6. Add to the caption what t=0 corrasponds to.
6. Figure 7 & 8. Add how many cyclones are included in these composites either to the title (after CESM-HIST etc.) or in the caption.