Weather Clim. Dynam. Discuss., referee comment RC1
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Comment on wcd-2022-38

Anonymous Referee #1

Referee comment on "Warm conveyor belts in present-day and future climate simulations – Part 1: Climatology and impacts" by Hanna Joos et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2022-38-RC1, 2022

Synopsis:
The present study by Joos et al. is the first to investigate changes of warm conveyor belts in a future climate. To do so, the authors have identified WCBs via trajectory calculations in 50 years of CESM simulations. Key findings are that CESM simulations reasonably well represent WCBs in present climate, that changes in WCB occurrence frequencies largely coincide with changes in midlatitude storm tracks, and that WCB related precipitation will increase due to an overall higher moisture content. Further, a poleward and upward shift of diabatic heating will allow WCBs to more favorably interact with the upper-level Rossby waveguide. The study is well written, the methods are established and the results are certainly of interest to the readership of WCD. I therefore recommend to accept this paper pending on some minor revisions.

Minor:
p.1, l.2: What is meant by "strong cloud formation"? Perhaps simply remove "strong"?

p.1, l.6: Should it be 1990-1999 instead of 1991-2000?

p.1., l.13: Could you be more specific how the characteristics change, e.g., increase or decrease?

p.1, l.16/17: Does "strong" mean that the increases are significant? Perhaps choose a more quantitative description by providing the relative or absolute increase.

p.2, l.5: The authors may want to reconsider the structure of the sentence. Better write "...deep convective clouds associated with the Hadley-Walker circulation dominate in the tropics...."?

p.2, l.24: WCBs do not necessarily ascend poleward. Perhaps adding "most often"?

p. 4, l.5: To better understand possible differences between ERAI and CESM the reader may need some more information concerning the CESM setup. Could you therefore include information on the native grid spacing of the CESM simulation and how this compares to ERAI? Also, it could be worthwhile to mention that the number of vertical levels is considerably higher in ERAI than in CESM.

p. 4, l.12: I understand that the identification of WCBs in CESM is nearly identical to that in ERAI. Still, could you clearly mention possible differences (e.g., number of vertical levels).

p. 5, l.14: If I recall correctly, ERAI WCB trajectories of Sprenger et al. (2017) were mapped to 1° grid spacing. What is the motivation for choosing 0.5° in CESM? Were the ERAI data remapped to the same grid spacing prior to comparing the two data sets. If not, could the different resolution explain some of the differences seen in Fig. 2?

p. 6, l.2: I fully understand that the authors chose a period of 37 years from ERAI to have the largest possible sample size. Still I am wondering, if the authors calculated the ERAI WCB frequencies for the same period as with CESM (1990-1999) would the differences between the two datasets become even smaller?

p.6, l.28/29: I assume it should be Northwest Pacific instead of Northeast Pacific.

p. 6, l.32: The similarity between ERAI and CESM is indeed striking. Still, it seems to me that some differences may exist which are currently not seen due to the choice of figures. For example, the DJF WCB frequencies seem to be generally higher in ERAI than in CESM (Figs. 2a,b) and differences at t=0h and t=48h are hardly visible since only the 1% isoline is shown. Have the authors considered to include difference maps between the two? I would strongly encourage the authors to either include such difference maps in the main paper or at least in the supplemental material. For example, it would be interesting to see whether biases in WCB frequency at t=48h correspond to biases in blocking frequency reported in previous studies (e.g., Woolings et al. 2018).

p. 8, l. 1: What exactly is shown in the intensity maps of the supplementary material? I would understand if the unit of the intensity maps was "number of trajectories/6h". Currently it is in %. Is this correct? If correct please explain how to understand the intensity in %.

p. 9, l.5/6: I guess it is 1990-1999 instead of 1990-2000.

p. 9, l.23: The decrease of WCB frequency at t=0,24, and 48h east of Madagascar is a rather stationary signal, i.e., the decrease in WCB outflow does not occur far downstream of WCB inflow as one would expect in midlatitudes. So, are the signals seen here really WCBs or is it rather the signature of recurving tropical cyclones that are expected to become less frequent in a warmer climate (Fig. 3 in Roberts et al. 2020)? This decrease of tropical cyclone frequency likely corresponds to the decrease in cyclone frequency seen in Fig. 7 of the present manuscript. Since the authors already tracked cyclones in CESM: would it be possible to determine the genesis region of cyclones around Madagascar? In my view, this would aid the interpretation of the results. Depending on the outcome of this analysis please consider to change the abstract and conclusion accordingly.

p. 9, l.32: To my understanding of Fig. 4f WCB outflow is less often located around 45°N rather than 30°N.

p. 10, l.1: The increase mostly occurs north of 60°N. Please correct.

p. 10, l.15: Also here, please double-check the units of Figures S1 and S2.

p. 10, l.33: Please double-check the latitude. I would rather say that the increase is found around 60°N.

p. 11, l.1: As mentioned above: The decrease of cyclone frequency around Madagascar could be a signal of changed tropical cyclone frequencies. Perhaps the authors can refer to the corresponding literature (e.g., Roberts et al. 2018) at this occasion.

p. 13, footnote: "WCB air parcels no longer increase in altitude, i.e., the pressure increases again" might be slightly confusing. Better write "...no longer gain altitude..."?

p. 14, l.2: Is the increase in specific humidity what one would expect based on the Clausius-Clapeyron relation?

p. 14, l.8: I assume that the distinction between large-scale and convective precipitation is based on the classification by CESM (parametrized vs resolved). Please clarify in the text.

p. 17, l.6: I guess it is 5.0mm/48h instead of 50mm/48h.

p. 18, l.31: I understand that the maximum anomaly is located within the lower stratosphere. But is this also true when considering the absolute frequencies? Further, I am wondering would the increase in outflow frequency mean that transport across the (dynamic) tropopause increases? Have you considered to quantify the fraction of WCBs that eventually end up in the lower stratosphere based on their PV at t=48h. An increase of WCB outflow in the lower stratosphere would raise interesting questions concerning their contribution to stratospheric water vapour concentrations in a warming climate - a discussion which focuses often on deep convection (e.g., Smith et al. 2022).

p. 20, l.2: Is the diabatic heating rate calculated explicitly or is it based on the potential temperature change along the trajectories? Please explain.

p. 20, l.6: I agree that the anomaly suggests a poleward shift by 5°, but is this also true when considering absolute values (i.e., the sum of HIST and anomalies)?

p. 21, l.30: Please include a brief description on how the percentage of total precipitation linked to WCBs is quantified (matching of WCB masks and precipitation fields I assume).

p. 22, l.3: Is it really certain that the microphysical processes are reasonably captured in CESM? Perhaps this statement could be weakened by stating that the integrated effect of microphysical processes is reasonably captured.

p. 24, l.28: To my understanding it is not only the changes in WCB outflow which have the potential to disturb the jet stream but also the changes in WCB ascent. A poleward shift of the ascent regions will lead to an irrotational outflow closer to the upper-level jet. If the authors agree this aspect could be included in the discussion.

p. 25, l.6: Remove colon after "in".

p. 25, l.22: In a recent study Steinfeld et al. (2022) showed that the frequency of WCBs in blocking anticyclones is expected to increase by 15%. Perhaps the authors could refer to their study.

Figures:
Figs. 2, 4, 5, 7, 11, 12, 13: Please increase the font size of the axis labels.

Figs. 11, 12: Please indicate that PV in the southern hemisphere has been multiplied by -1.

References:
Roberts, M.J., Camp, J., Seddon, J., Vidale, P.L., Hodges, K., Vannière, B., Mecking, J., Haarsma, R., Bellucci, A., Scoccimarro, E., Caron, L.-P., Chauvin, F., Terray, L., Valcke, S., Moine, M.-P., Putrasahan, D., Roberts, C.D., Senan, R., Zarzycki, C., Ullrich, P., Yamada, Y., Mizuta, R., Kodama, C., Fu, D., Zhang, Q., Danabasoglu, G., Rosenbloom, N., Wang, H. and Wu, L. (2020), Projected Future Changes in Tropical Cyclones Using the CMIP6 HighResMIP Multimodel Ensemble. Geophys. Res. Lett., 47: e2020GL088662. https://doi.org/10.1029/2020GL088662

Smith, J. W., Bushell, A. C., Butchart, N., Haynes, P. H., & Maycock, A. C. (2022). The effect of convective injection of ice on stratospheric water vapor in a changing climate. Geophysical Research Letters, 49, e2021GL097386. https://doi.org/10.1029/2021GL097386

Steinfeld, D., Sprenger, M., Beyerle, U., & Pfahl, S. (2022). Response of moist and dry processes in atmospheric blocking to climate change. Environmental Research Letters, 17(8), 084020. https://doi.org/10.1088/1748-9326/ac81af

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