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Interactive comment on "Vertical cloud structure of warm conveyor belts – a comparison and evaluation of ERA5 reanalyses, CloudSat and CALIPSO data" *by* Hanin Binder et al.

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Review of Vertical cloud structure of warm conveyor belts - a comparison and evaluation of ERA5 reanalyses, CloudSat, and CALIPSO data

by H. Binder, M. Boettcher, H. Joos, M. Sprenger, and H. Wernli

Summary: This paper uses retrievals of ice water content from CloudSat and CALIPSO to study the properties of warm conveyor belts, and to evaluate the clouds produced by the ERA5 reanalysis. The authors analyze a representative WCB case, then extend their analysis to multiple years of observations, producing composites of retrieved ice

water content for all WCBs and for the 5% corresponding to the largest radar reflectivity values. They find interesting signals in the observations that indicate a connection between WCB properties and the vertical distribution of ice cloud content. They further connect the ice cloud properties to dynamics and thermodynamic properties by analyzing the static stability and PV in the reanalysis data. The paper is well conceived and well written, and I have only minor comments and suggestions for the authors. I begin with a few general comments, then itemize a number of specific comments after.

General Comments:

1. One must be careful, when comparing model output to satellite retrievals, to note where there may be overlap between the sources of data. In the case of DARDAR ice water content retrievals, the ice cloud estimates depend on temperature profiles that are obtained from ECMWF. As such, the IWC information in the retrievals is not entirely independent of the model that is being evaluated. This does not mean that the comparison is not valid, but I would suggest that the authors note the fact that there is information from ECMWF in the DARDAR product and perhaps discuss how this might affect the conclusions drawn in the study.

2. There appears to be a convective (vs stratiform) signal present for strong WCBs with heights above 12 km (see my specific comments below for details). I wonder if it would be of interest to the authors to add a brief discussion of this?

3. One question that comes to mind in any observation-based study (and in an evaluation of model output in particular) is the degree to which observations are able to characterize processes. The authors have done a nice job inferring the connection between satellite retrievals and processes via the analysis of PV and the thermodynamic environment. However, the A-Train sees only snapshots of cloud fields and (as the authors point out) rarely revisits a given storm more than once in its life cycle. Looking toward the future, I wonder what is missing from the observations that would enable a more specific process-based analysis and model evaluation? What are the most

critical observational needs for improving the understanding of the time evolution of storms? Will the addition of vertical motion estimates (e.g., from the doppler radar on EarthCARE) be helpful, or are time resolved measurements of the interior of the WCB clouds necessary? The paper is complete with or without this discussion, but I wonder if the authors would like to comment (given their expertise in this area) on what would be most useful in their future analyses?

Specific Comments:

1. P6, line 173. Is it true that the satellite observations are available more often than hourly? Even over the poles, the data is available only approximately once every 100 minutes, with longer delays with decreasing latitude. I wonder if you meant that the satellite intersections of WCBs often happen between the 1-hour analysis intervals?

2. P8, lines 227-229. What is the uncertainty in the DARDAR IWC retrieval? Is there a known issue with retrieval of ice in mixed phase regions in DARDAR? I wonder whether the differences between ERA-5 and DARDAR are within the 1-sigma uncertainty of the observations? The same question would apply to the composite comparisons in section 4.

3. P8, lines 249-250. In Figs 3a and 3b, I noticed an apparent gap in the concentration of WCBs (in the full set and also the "strong" subset) around 170 W longitude and extending through all latitudes from SE to NW. It looks like there may be data missing along a satellite swath? I am curious as to whether this is a real (geophysical) feature or an artifact in the data?

4. P11, lines 354-355. It is interesting that there are large reflectivities (and relatively warm columns) where the "strong" WCB heights are greater than 12 km. As the authors point out, it is not possible from Figs. 4c and 4d to determine trajectories; however, I wonder if the fact that the mean profiles extend through the depth of the troposphere with high reflectivities and warm temperatures indicate convective (vs stratiform) profiles?

5. P12, lines 377-378. I have the same question here as I did in for the case study - how do the obs-model differences compare with the observational uncertainty? In this case, would one expect the observation uncertainty to be smaller than for the individual case (since the results consist of an average over a large number of profiles)?

6. P14, lines 425ff. As noted above in my comment on the deep protions of strong WCBs in Fig. 4c, the thermodynamic analysis appears to support the presence of convective clouds for WCB trajectory heights above 11 km. There is a strong uptick in precipitation rate (including convective) (Fig. 6) as well as high RH through the depth of the troposphere (Fig. 7c), high theta-e values (Fig 7d), and weak stratification (Fig. 7e).

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