Review of “The role of air-sea fluxes for the water vapour isotope signals in the cold and warm sectors of extra-tropical cyclones over the Southern Ocean” by Thurnherr et al.

This study nicely combines observations and numerical modelling experiments and diagnostics to determine how the isotopic composition of water vapour is affected by air-sea fluxes induced by either cold and warm advection associated with extra-tropical cyclones. The main finding is that the cold and warm sectors have notably different stable water isotope composition. Overall, the manuscript is well written, employs appropriate methods and the conclusions are largely supported by the evidence shown in the figures. My detailed comments are below.

Major comments

1. This manuscript is currently quite long and covers many different data sets and time scales. Consequently, it is challenging to read. In particular, it is not clear why the climatology of cold and warm air advection from ERA-Interim needs to be included in this manuscript. This is particular the case as the climatology presented only covers December to March and therefore is not a complete climatology and as such I argue that the authors do not address their first objective “What is the occurrence frequency of cold and warm temperature advection over the Southern Ocean”. Furthermore, this climatological analysis is not mentioned at all in the abstract suggesting it is not key to this manuscript. It is my opinion that this material could be removed from this manuscript.
2. Section 3 is long and the purpose of this section is not clear. This is because as well as defining the diagnostic used to identify warm and cold air advection, this section also contains an attempt to compare the COSMO simulations and the ship observations. This comparison is hard to follow and is not complete. This section would be clearer if it only covered the diagnostic and if the comparison was moved elsewhere.
3. The choice of threshold for defining cold or warm advection is not clearly explain. There is a reference to Hartmuth (2019) but some details should be included here. In particular, it is not clear why the threshold should be the same (e.g. +1K and 1K). Related to this, are these “symmetric thresholds” the reason why cold air advection is notably more common (39%) than warm air advection (12%)? Or is this difference in occurrence due to cold sectors generally being larger than warm sectors?
4. Section 2.3. More details are required here in this manuscript about how the moisture uptake is calculated. This becomes evident when a reader reaches lines 468 and 489. In both lines it is stated “the moisture uptake took place XX h before the air parcel arrived at the measurement site”. This is unclear and potentially mis-leading as surely the moisture uptake does not take place at one instant in time but is an accumulation along the trajectory? Please can this be clarified / relevent detailed added to section 2.3. Furthermore, at line 463, it is not clear what is meant by “The weighted mean of the moisture source properties...” It needs to be explained how / what weighting is done. Again, please add details to section 2.3.

Minor comments

1. Lines 86 - 87. Please define the terms moisture sink and moisture reservoir more clearly. I find this confusing currently as in the case of evaporation from the ocean to the atmosphere, the ocean is the moisure sink (it is loosing moisture) but also could be the moisture reservoir.
2. Line 137. Related to major point 1, when it is mentioned here that ERA-Interim data will be used, it is confusing to a reader as to why.
3. Line168-169. Here the text suggests that all months are considered from ERA-Interim but elsewhere it is stated that only December-March are considered. Please clarify.

4. Line 175 - 179 and Figure 5. How does the cyclone tracking, with mean sea level pressure, work very close to or above the Antarctic coastline? In Figure 5 there are very tightly packed contours of the storm occurrence on the coastlines. Is this realistic? Please add text noting the limitations of the cyclone identification method in areas of steep terrain.
5. Line 187. The reasoning for how the boundary conditions for the COSMO simulation were created is not clear and quite confusing. It is not clear why the boundary conditions could not be taken straight from ERA-Interim at T255 resolution. It does not make sense to use ERA-Interim to drive a coarser resolution ECHAM simulations and then use that to drive the COSMO simulations. Please clarify in this manuscript why the ECHAM simulation was chosen.
6. Line 190. I believe that T106 has an equivalent resolution of 125 km, not 88 km.
7. Line 209-210. I understand why multiple limited domain simulations have been performed (computational cost) but I am concerned about how often trajectories leave the domain and also that this will not be a systematic bias i.e. there will be some times when the ship track is closer to the edge of the model domain and thus it is more likely that trajectories leave the domain. For each simulation listed in Table 1, could the number of released trajectories and the number of trajectories still in the domain after 7 (or 4?) days be added to this table? This would also give a reader a quantitative value of the number of trajectories released.
8. Line 218. How are the trajectories starting in the MBL identified? i.e how is the MBL defined? Is the BL depth taken directly from the COSMO simulations? Furthermore, why are trajectories released up to 500 hPa if only those arriving in the BL are considered?
9. Line 274. “the difference between 10 m and 24 m a.s.l air temperature is fairly small” Can this be more quantitative? I also expect this is a more valid statement for regions of cold-air advection where the MBL is well mixed, but less valid for stable MBLs. Could you utilise the data shown in Figure 8 to make this more quantatitive?
10. Line 292. If you can justify keeping the climatological aspect (and these results in section 4.1a), it needs to be more clearly explained at the start of this section why the climatology only covers December – March.
11. Line 417. Are these cases with the very high and very low observed ΔT_{ao} included in the trajectory analysis or not? Please clarify in the text.
12. Line 465. “driest point”. Is this in terms of specific humidity (or relative humidity) along the trajectory? Please clarify this in the manuscript.
13. Line 521-522 and Figure 12. Are the trajectories split based on observed precipitation or the precipitation from COSMO? And more importantly, does COSMO precipitation agree with the observed precipitation? Could this panel be added to Figure 10? (ACE precipitation vs. COSMO precipitation).

Figures Comments

1. Figure 3: Could the sea-ice edge be made clearer in this figure? e.g. as solid contour or by hatching? Also in Figure 3b, could the warm and cold fronts be shown differently?
2. Figure 4. Can you add the dates / time period of the ship track to the caption here? It would help a reader to have this information about the time-scale at hand.
3. Figure 5. The cyclone frequency contours are hard to see (especially in Fig. 5e) – could this be improved? Maybe making one contour e.g. 30% darker?
4. Figure 12 and 13 (and in the text). The units of precipitation, and cloud water and ice are given in strange units, mg $kg^{-1.}$ Either can these be written as mm for precipitation or g kg^{-1} for the cloud variables, or can mg kg^{-1} be defined in the caption / text.