**wcd-2020-46**

**The role of air–sea fluxes for the water vapour isotope signals in the cold and warm sectors of extratropical cyclones over the Southern Ocean**

**Iris Thurnherr et al.**

**Replies to Review 2:**

We thank the reviewer for the constructive comments. We address below each comment point by point.

**Major comments**

1. *L. 221: Since the identification of the moisture uptake plays an important role in the analysis, more details should be provided here about the method to identify the moisture source. What should be explained in particular is the method to calculate the transport time from the moisture source to the measurement point. Since many back-trajectories are calculated by the Lagrangian model for each ending point, this time to the moisture source but be an averaged value for all trajectories. Furthermore, each trajectory might be filled in water at different stages of the atmospheric transport, so how is the moisture source estimated for each trajectory? Is it also an average time of all the stages at which the air mass is filled in moisture?*

   Answer:
   Yes, the moisture uptake time is a weighted mean of the uptake times over all trajectories. This weighted mean uptake time is obtained in two stages. In the first stage, all the uptakes along a given trajectory are identified by following the trajectory and identifying instances, when specific humidity increases. To each of these uptakes from a given trajectory we assign a weight according to its contribution to the final specific humidity at the arrival point of the trajectory. Whenever precipitation occurs (i.e. specific humidity decreases along the trajectory), the weights of the previous uptakes are discounted accordingly. As a result from the first stage, we obtain a weighted mean time of uptake for every trajectory arriving in the boundary layer. In the second stage, we weight the resulting mean uptake time from each trajectory according to its specific humidity at the arrival point. We realise that this was not clear enough in the manuscript and added the following short explanation in Section 2.3 at line 222:

   "In short, this method considers the mass budget of water vapour in an air parcel. Moisture uptakes are registered whenever the specific humidity along an air parcel trajectory increases. The weight of each uptake depends on its contribution to the specific humidity of the trajectory upon arrival. If precipitation occurs (i.e. a decrease of specific humidity along the trajectory happens) after one or several uptakes, the weight of all previous uptakes is reduced proportionally to their respective contribution to the loss. The moisture source conditions identified for each trajectory are subsequently weighted by the air parcel's specific humidity at the arrival in the boundary layer. This is done for different variables that are relevant for characterizing the moisture source conditions, such as the time of uptake, the latitude and the water vapour's isotopic composition. More details on the moisture source identification algorithm is provided in the supplement."

Furthermore, we add a more detailed description of the moisture source diagnostics in the Supplement (to avoid making the paper even longer as this was already a concern of the reviewer).

Concerning the uptake times discussed at lines 468 and 469 we now make clear that these are weighted mean times before arrival.

2. *L. 291-292: Maybe add in a few words what is exactly meant by the "occurrence frequency" and "associated air-sea moisture fluxes". Is it the frequency for each grid cell of each type of advection regime, and is the associated moisture fluxes would the mean values associated to each regime?*

   Answer:
   Yes, these are as you wrote. We add a few words, when we introduce these terms.

3. *Section 4,3 and Fig. 12 and 13: You are often discussing variations of the median values for trajectories experiencing no surface precipitation upon arrival and for those with surface precipitation upon arrival. On the figures, many of the differences you discuss appear to be rather small for some parameters. Contrary to the values for all trajectories, where the [25,75] percentiles are shown, these values are not presented for these two subsets. Would it be possible to include such parameters on Fig. 12 and 13? I believe this is missing when you want to legitimate the validity of the comparison of the medians when differences are very small.*

   Answer:
   By showing the two subgroups of trajectories with and without precipitation at arrival, we want to highlight that the differences in SWIs between cold and warm temperature advection are much larger than the effects due to precipitation. We find this an important and interesting result because many previous studies have shown that boundary layer water vapour isotope signals are strongly affected by precipitation (Risi et al. 2010; Aemisegger et al. 2015; Graf et al. 2019). Therefore, we show the median trajectories of these subgroups, even though the difference to all trajectories is mostly small.

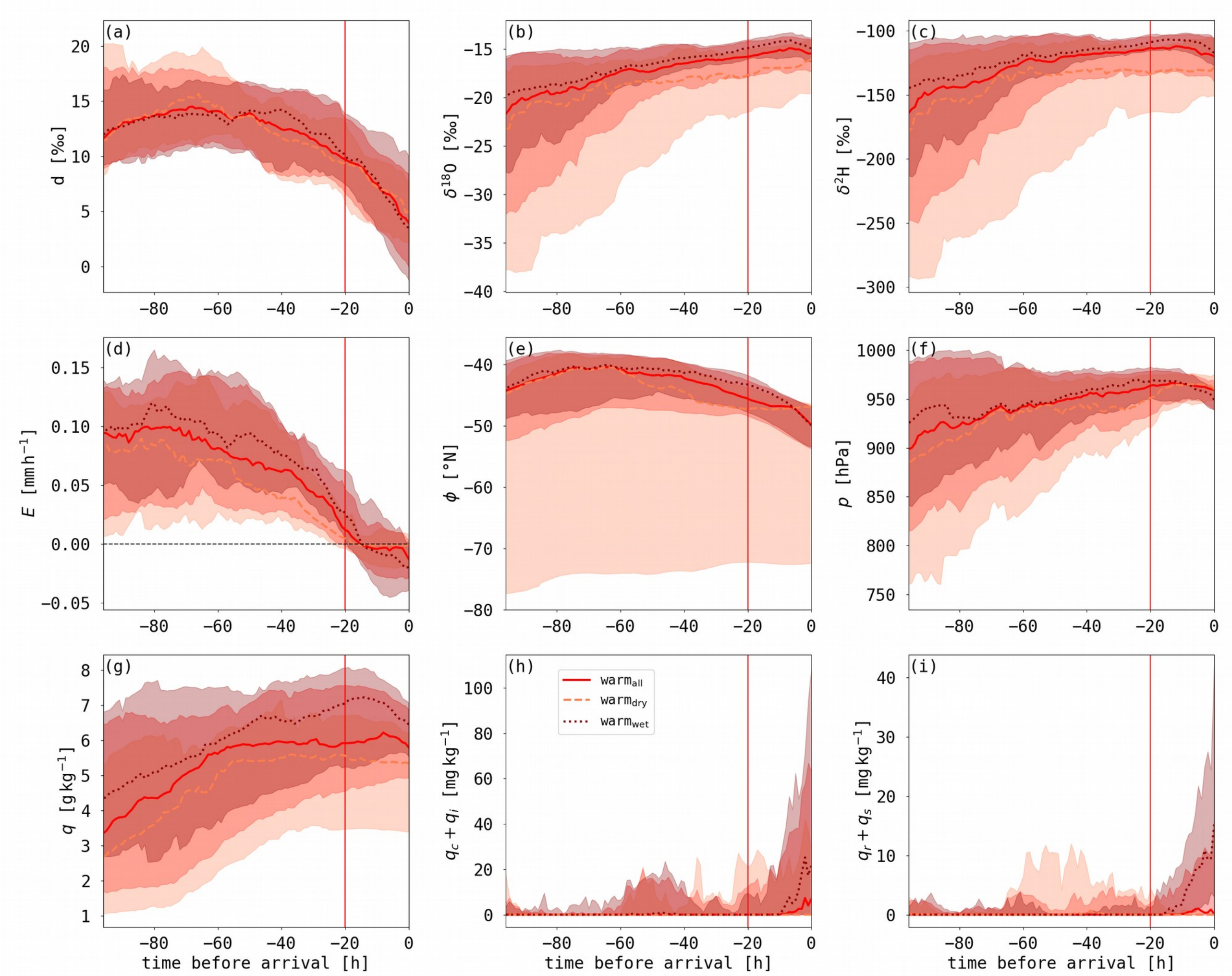


*Figure 1: Same as Fig. 12 in the manuscript, but only showing the composites for warm temperature advection. Additionally, the [25,75] percentile range is shown for the precipitating and non-precipitating trajectories.*

We think that the figures will be overloaded if we also show the [25,75] percentiles for the non-precipitating (dry) and precipitating (wet) trajectories (see Figure 1 above, only showing the warm temperature advection trajectories). Furthermore, the [25,75] percentiles are similar for all trajectory groups (for wet, dry and all trajectories during cold and warm advection). Only for warm advection under dry conditions, there is a large difference in percentiles for the trajectory subgroups. The large percentile range for dry conditions are caused by trajectories, which have low *q* and δ-values and arrive during leg 2 at high latitude. These are two groups of trajectories, which originate from Antarctica (see Fig. 4 in the manuscript) and are most likely warmer than the ocean surface due to large-scale subsidence. Even though these trajectories show a different dynamical history, they show the same *d* and *E* evolution as expected in an oversaturated environment (see Fig. 12a,d). Because (i) the [25,75] percentiles are similar for the different subgroups, (ii) the large differences in percentile range for dry warm advection trajectories are not caused by precipitation-related processes, which we are investigating here, and (iii) the figure will be difficult to read, we will not show these ranges in Fig. 12 and 13.

We agree that the difference between the precipitating and non-precipitating trajectories is sometimes discussed in too much detail in the manuscript, without highlighting the main conclusion, which is that the largest variability in SWIs is seen due to the different temperature advection regimes. We will adjust the text in section 4.3 to highlight the main message better.

**Minor comments**

1. *L. 196: "realistic"*

   Answer:
   Thank you, we will adjust it.

2. *L. 246 and 254: "around 60° E" and "the cyclone at 60 ° E": I believe the description of the localization of the described cyclone is too vague, which makes it difficult to compare with Fig.3. Based on the Figure, I wouldn't say that the cyclone is located at 60° E but rather between 60 and 90° E. Maybe you should mark the cyclone on Fig. 3 with a different color.*

   Answer:
   We will adjust the description to be clearer.

3. *L. 250: Similar remark for the 40° W cyclone, which extends over a large range of longitudes. Furthermore, it would be easier to identify it if the 40° W meridian was shown on Fig. 3. As for the previous comment, a color indicator or an arrow/letter on Fig. 3 would help identifying this specific cyclone you are describing.*

   Answer:
   Also here, we will adjust the description to be clearer.

4. *L. 275: "compare orange trajectories in Fig. 4 and black dots in Fig.5" > I don't understand what should be compared here. I think you are actually refering to Fig. 7 instead of 5.*

   *Answer:*
   Thank you for pointing this out. Yes we are referring to Fig. 7 and will adjust this.

5. *L. 361: "Fig. 10d". Check the order in which Figures are referred to and placed in the manuscript.*

   Answer:
   We will check the order of the Figures.

6. *Fig. 11: I think it would be more readable if the xlabels where not centered on the ticks, but right-aligned, as they are rotated and they would finish at the tick.*

   *Answer:*
   We will adjust this.

7. *L. 505-507: Maybe you could make a similar introduction for part c) at the end of part a). It would also be possible to modify the order of the paragraphs into a), c), b), d), in order to stay focused on the same type of events.*

   Answer:
   Yes, a rearrangement of the paragraphs might help to stay focused. We will change the order and adjust the ending of paragraph a).

8. *L. 519: "influenced"*

   Answer:
   Thank you, we will adjust it.

9. *In section 4.3: I find this section extremely interesting and this shows the capacity of the Lagrangian backtrajectory simulations and the regional modeling of water isotopic composition in explaining the processes affecting the water vapour within air masses during atmospheric transport. However, I think what is missing in this section is a comparison with the measurements performed on the ship. Would it be possible to include on the graphics of Fig 12 and 13 the distribution of all isotopic and meteorological values for the selected periods at the end point of the trajectories or at the surface, to see if it compares well with the COMOiso simulation and discuss such comparison in the manuscript?*

   Answer:
   We cannot validate the Lagrangian results with our measurements as we only have measurements along the ACE track (at time=0 along the trajectories). This comparison is done in Fig. 9. As we mention in the discussion, further measurements are needed to validate our results from the trajectory analysis, for example with profile measurements, measurements in clouds or even a set of measurements with a series of instruments positioned along an air mass' pathway (which is challenging to do). We will add a sentence

at the end of the discussion to come back to the comparison of measurements and model simulations.

10. *Fig. 13: A legend similar to Fig. 12,h should be added to this figure.*

    Answer:
    Thank you, we will add this.

11. *L. 577-578: "This is indicated by the vertical d profile of non-precipitation trajectorie": I am not sure this sentence is necessary here, since it is followed by a discussion on the trajectories and the discussion on vertical profiles comes afterwards.*

    Answer:
    We will remove this sentence (and adjust the following one slightly to "The non-precipitation trajectories show a higher *d*...") as we don't want to refer to the vertical profiles at this stage.

12. *L. 591-593: Maybe rather place this paragraph in the discussion-conclusion, as this is not only a conclusion for 4.3.d but for the complete 4.3 section.*

    Answer:
    Yes, this is a conclusion for the entire Sec. 4.3. We will keep it here, but will make clear that it refers to the entire section.

References:

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Risi, C., Bony, S., Vimeux, F., Chong, M. and Descroix, L.: Evolution of the stable water isotopic composition of the rain sampled along Sahelian squall lines. Q.J.R. Meteorol. Soc., 136: 227-242. https://doi.org/10.1002/qj.485, 2010.

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