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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

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Replies to Review 1:

We thank the reviewer for the constructive comments. We address below each comment point by point. Section, Figure and line numbers correspond to the first submission.

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.*

   Answer:

   We agree that the manuscript is quite long and have shortened some lengthy parts as a consequence (e.g. in Sec. 4.1a). We however think that a short discussion of a few carefully selected climatological aspects of cold and warm advection in the Southern Ocean is needed to justify the the method and to provide a climatological context to the measurements. To keep the manuscript as short as possible, we only discuss the climatology for Dec-March, which represents the months of the measurement period. We will additionally change the following:

   - Fig. 5 (1979-2018 climatology of Dec-March) will be moved into a supplement document.
   - The ocean evaporation and precipitation composites for warm and cold advection during Dec 16 – March 17 will be added to Fig. 7 and discussed for the measurement time period.
   - We will remove the first objective as this is not a main focus of the 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.*

Answer:

We understand, that the comparison of $COSMO_{iso}$ simulations with the ship observations is not well placed in this section. We will move the comparison of measurements and model output to Sec. 4.1b and only mention it in Sec. 3, where we explain our choice of the used vertical level for air temperature.

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?*

Answer:

In this study, we don't want to describe extreme events, but situations, in which the atmosphere and the ocean are not in thermodynamic equilibrium. Therefore, we decided to define symmetric thresholds around an isothermal near-surface stratification and do not choose the thresholds depending on the probability distribution of ΔT. We added a short justification to the text in Section 3 at line 240.

But the reviewer's remark is a valid one and most probably points towards the fact that one does not a priori expect the distribution of ΔT to have a median or mode equal to 0 K. Due to the difference in heat capacity of ocean and atmosphere one may expect the mode of the ΔT distribution to be negative (see Figure 1 in this reply document). This indicates that the ocean and the atmosphere are generally not in thermodynamic equilibrium and the ocean on average loses heat to the atmosphere. As shown in Figure 1, our chosen threshold for cold advection corresponds to approximately the 50^{th} percentile and the one for warm advection to the 75^{th} percentile of the ΔT distribution. The reason for the different occurrence frequencies of ΔT*>1.0°C (warm temperature advection) and* ΔT*<-1.0°C (cold temperature advection)* mainly results from this observed shift of the ΔT distribution towards negative values. Accordingly, the size of the cold temperature advection regions tends to be much larger than warm temperature advection regions.

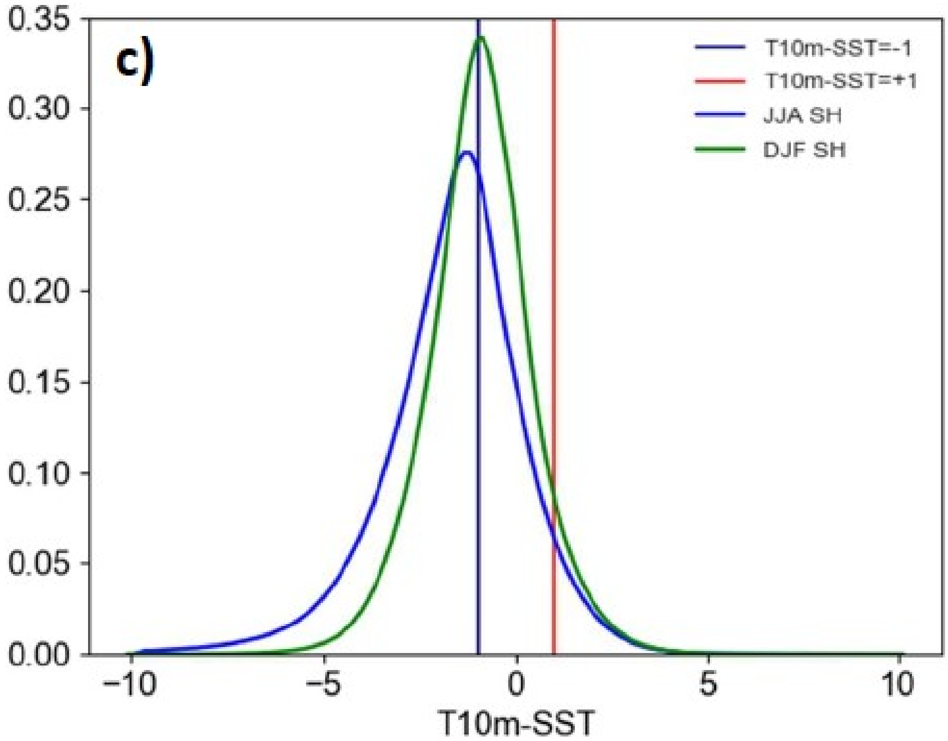

Figure 1: Distribution of T_{10m}-SST over the ocean on the Southern hemisphere for ERA-Interim between 1979-2016 for DJF (green) and JJA (blue). The red and blue lines indicate the +/-1 °C threshold values for the warm and cold sector definition.

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.*

   Answer:

   The moisture source diagnostic by Sodemann et al. (2008) is a well-established method and has been applied in many studies so far (see, e.g., Pfahl and Wernli 2008; Bonne et al. 2014; Aemisegger 2018). In section 2.3 at line 222, we add a short summary of the method as follows, including an explanation of the weighting along the trajectories:

   "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.

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 moisture sink (it is losing moisture) but also could be the moisture reservoir.*

   Answer:
   We understand the reviewers concern and removed the word moisture sink. We changed the text highlighted in red: "During a diffusive process, a positive anomaly in *d* develops in the phase towards which the flux is directed (e.g. the atmosphere during ocean evaporation), while a negative *d* anomaly can be observed in the other, reservoir phase (e.g. rain droplets during below-cloud evaporation). If the moisture reservoir is large and well-mixed, which can ~~e.g.~~ be assumed for the ocean during ocean evaporation, the impact of isotopic fractionation on the isotopic composition of the reservoir can be neglected."

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.*

   Answer:
   We use ERA-interim because of its global and temporal coverage that allows to provide a climatological context to and classification of the studied summer period and to analyse the general properties of cold and warm temperature advection in terms of their associated surface evaporation flux and precipitation.

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.*

   Answer:

We will add that only Dec-March are considered in this study.

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.*

   Answer:
   For the identification of cyclones, pressure minima are removed where the topography exceeds 1500 m. Therefore, a strong gradient in cyclone frequency is seen along the Antarctic coastline. We will add this information in section 2.2.1.

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.*

   Answer:
   Since COSMOiso is an isotope-enabled regional numerical weather prediction model, it needs global isotope-enabled data for its initialization and for the boundary conditions. For consistency, ECHAM5-wiso is used for all fields. Please note that the ECHAM5-wiso simulation was nudged to ERA-Interim. We will add this information in section 2.2.2

6. *Line 190. I believe that T106 has an equivalent resolution of 125 km, not 88 km.*

   Answer:
   T106 corresponds to 1.125° horizontal resolution. This corresponds to 125 km in the meridional direction and 88 km at 45°N in the zonal direction. We will change the value to 125 km, which is independent of the latitude.

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.*

   Answer:
   We understand the reviewer's concern, that the limited domains of the simulation might impact the weighted mean properties along the analysed trajectories. However, we do not think that this is a major limitation of our study, because we only consider trajectories for which a major part of the moisture at the arrival points is explained by the moisture source diagnostic (referred to as explained moisture fraction). Trajectories, which are included in this study, have an explained moisture fraction of at least 75%. We tested the sensitivity of

our results to this threshold by changing it to values between 70% and 90% and found, that the results are not sensitive to the choice of the threshold. Furthermore, trajectories, which leave the domain after a few days are not systematically neglected in this analysis (see Fig. 2 below). The excluded trajectories for a threshold of 75% stay in the model domain for 4-7 days. For the trajectories considered in the analysis, there is a larger spread of exit times from the model domain of 1-7 days. Based on this analysis, we decided to change the threshold to 70%, because with a 75% threshold most of trajectories in the simulation *leg2_run3* are excluded. We will add the number of released trajectories during warm and cold temperature advection and the percentage of these trajectories included in the analysis for each simulation in Table 1.

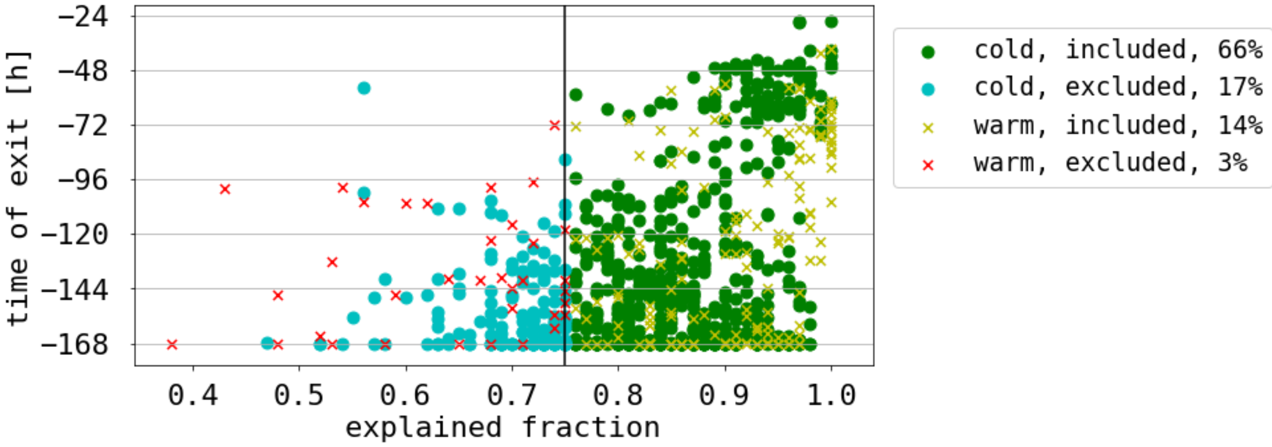

Figure 2: Scatterplots of 1-hourly explained moisture fraction upon arrival in the MBL and the time of exit from the model domain for trajectories arriving during cold (dots) and warm (crosses) temperature advection. The colors denote the warm and cold temperature advection trajectories which are, based on a threshold of 75% for the explained moisture fraction upon arrival, included (yellow and green, respectively) and excluded (red and blue, respectively) from the analysis. The percentages in the legend denote the fraction of the total number of trajectories.

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?*

   Answer:

   Yes, the diagnosed boundary layer height from COSMO is used as boundary layer depth. In COSMO this is done using the bulk Richardson number. The trajectories were released up to 500 hPa, but the trajectories above the MBL are not used in this study. We will adjust the information to avoid confusion.

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 quantitative?*

Answer:

Yes, we will add a more quantitative answer. Radiosonde profiles are not very precise close to the surface due to launch effects with the unwinding and oscillations of the sonde. Therefore we cannot use them for these near-surface temperature gradients. But we can give an estimate based on the simulated temperature profiles.

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.*

Answer:

Please, see our answer to major comment 1. We will state more clearly, that we focus on Dec– March to provide a climatological context to the measurement period from Dec 2016 to March 2017.

11. *Line 417. Are these cases with the very high and very low observed ΔTao included in the trajectory analysis or not? Please clarify in the text.*

Answer:

Yes, these events of very high and very low observed ΔT_{ao} are included in the current analysis as the identification of cold or warm temperature advection is the same in the model and measurements. We make this clearer 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.*

Answer:

Yes, the driest point is calculated using the specific humidity. We will 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).*

Answer:

The precipitating and non-precipitating trajectories are selected based on the simulated precipitation in COSMOiso because in this section we focus on simulated SWI variables. We show the simulated and measured precipitation in Fig. 3 below, which shows the distribution of precipitation during cold and warm temperature advection for all instances when total precipitation $>$ 0mm/h along the ACE track. While the distribution of the measured precipitation shows a larger spread compared than the simulated precipitation and there is a difference in the mean and median values, as well as the 75^{th} and 95^{th} quartiles, overall the simulated precipitation is within the variability (and the uncertainty) of the observed values. The evaluation of precipitation in regional climate models and reanalysis

using ACE observations is part of another paper currently in preparation and is beyond the scope of this study. The reasons for the mismatch between the observed and modeled precipitation amounts include (i) needs to improve cloud/precipitation microphysical schemes in numerical weather and climate models, specifically for mixed-phase cloud representation, (ii) uncertainties in the micro-rain radar derived precipitation amounts, and (iii) discrepancies between point measurements (as provided by the radar) and the model's grid-averages. Nevertheless, overall we assume that the simulated precipitation statistics are fairly realistic. Figure 3 below shows that the simulations and measurements qualitatively agree, with clearly more precipitation during warm advection compared to cold advection. We therefore think that the analysis of the precipitating and not-precipitating trajectories is meaningful and can provide valuable information about the potential influence of precipitation-related processes on the isotopic composition of water vapour. In the future, measurements of vertical SWI profiles, especially in cloudy and precipitating situations would be most welcome to confirm the results of our study.
To keep the manuscript as short as possible, we will not add Fig. 3 to the paper. We will however make clear that we are using the simulated precipitation to create the subgroups of trajectories and we will add a statement about the comparison to the measured precipitation during ACE.

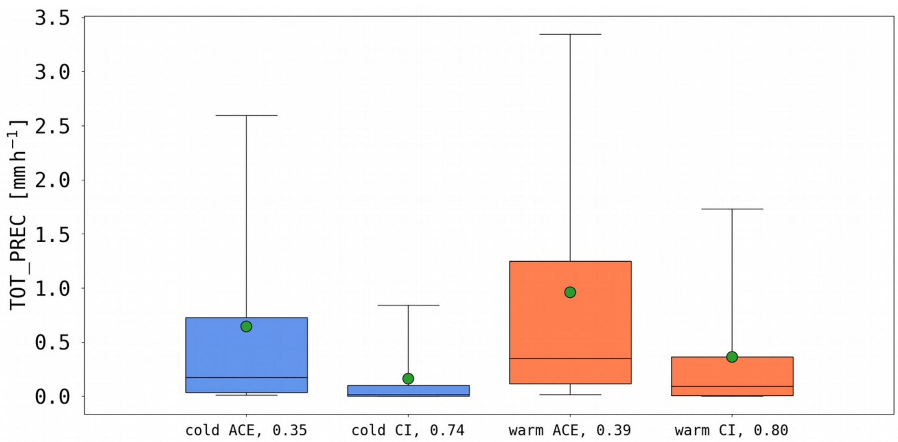

Figure 3: Distributions of modeled (CI) and measured (ACE) surface precipitation during cold (blue) and warm (orange) temperature advection. The green dots show the mean values, the horizontal black lines the median values, the boxes the interquartile range and the whiskers the [5,95] percentile range of the distributions. The numbers in the x-axis labels denote the fraction of time, when it was precipitating for the corresponding advection regime and data set.

Figure 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?*

Answer:

Yes, we will adjust Fig. 3 to make the sea ice edge clearer and to distinguish warm and cold fronts.

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.*

   Answer:
   Yes, this will be added.

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?*

   Answer:
   Yes, we will adjust this.

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.

   Answer:
   We use mg $kg^{-1}=10^{-3}$ g kg^{-1} for readability reasons of the tick labels. We will change the unit to 10^{-3} g kg^{-1} to avoid confusion.

References:

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