We thank very much the editor and the two reviewers for their very detailed and helpful comments. We have addressed all comments below and are willing to submit a manuscript taking into account all comments as explained in the answers to comment below.

Many thanks again for your help.

Editor comments on egusphere-2023-617

General comments:

The quality of this draft is above standard and the subject is suitable for publication in ACP. However, I think it may not be easy for every reader to understand this manuscript. Especially for those who are not experts on water isotope processes. I recommend major revisions of the manuscript so that the contents and results of this study become better understandable for any potential reader. Further, the graphical presentation is rather of low quality. Most of the figures should be improved (increasing the size of the figure itself as well as the font size and the line thickness). Some of the rather important figures of this study are in the supplement and may be moved to the main text or in an appendix to the main text (so that these do not appear in an extra document).

>> Many thanks for this suggestion. Indeed, the idea of an appendix to present the figures is a very good one and we will use it preferentially than the supplementary material for some figures.

The usage of terms etc. should be done more consistently one way or the other. In some occasions the term “vapor mixing ratio”, “mixing ratio” , “water mixing ratio” or “water vapor mixing ratio” are used making reading this manuscript very confusing. Then the notation “v” as subscript is used in some occasions, but in many others not. I would suggest to use “v” and “p” to differentiate between vapor and precipitation water vapor and water vapor isotopes throughout the manuscript.

>> This is indeed again a good suggestion and we will do this systematically for the new version of the manuscript.

Specific comments

Abstract: The abstract is quite confusing and some transitional sentences, e.g. the first and second paragraph as well as a sentence stating that you use two models for comparison are missing. What is the main focus of your study ? The two paragraphs feel like two independent abstracts. One describing the measurements and the conclusions you derive from these and then the measurement-model comparison and the according results of this part of the study. The problem with the focus of study continues throughout the entire study. Additionally, several technical issues in the abstract need to be corrected (see below the list of technical corrections). Since there were so many issues I provide you here a corrected/improved version of your manuscript as a suggestion how it would read much better:

>> Many thanks for your suggestion. We slightly adapted it and propose the following abstract:

“In order to complement the picture of the atmospheric water cycle in the Southern Ocean, we have continuously monitored water vapor isotopes since January 2020 in Amsterdam Island (37.7983 °S, 77.5378 °E) in the Indian Ocean. We present here the first 2-year-long water vapor isotopic record on this site. We show that the vapor isotopic composition largely follows the water vapor mixing ratio, as expected in marine boundary layers. However, we evidence 11 periods of a few days where there is a strong loss of correlation between water vapor $\delta^{18}O$ and water vapor mixing ratio. These periods are associated with abrupt negative excursions of water vapor $\delta^{18}O$, often occurring toward the end

of precipitation events. Six of these events show a decrease in gaseous elemental mercury suggesting subsidence of air from higher altitude.

Our study aims at exploring the mechanism driving the variations of water vapor $\delta^{18}O$ and especially the 11 events identified in the water vapor $\delta^{18}O$. With the aim to understand the associated processes, the comparison of the data series with outputs of atmospheric components of Earth System models equipped with water isotopes is a very useful tool. We thus used two different models to provide a data-model comparison over this 2-year water vapor $\delta^{18}O$ record. While the European Centre Hamburg model (ECHAM6-wiso) was able to reproduce most of the sharp negative water vapor $\delta^{18}O$ excursions, the Laboratoire de Météorologie Dynamique Zoom model (LMDZ-iso) at 2° (3°) resolution was only able to reproduce 7 (1) of the negative excursions. Based on a detailed model-data comparison, we conclude that the most plausible explanations for such isotopic excursions are rain-vapor interactions associated with subsidence at the rear of a precipitation event."

P2, L51: Also in the introduction still the question remains what the purpose of your study is. Is it to confirm/better understand the measurements or to test the capability of the isotope enabled models to reproduce the isotopic processes?

>> In this manuscript, we mainly look for controls on negative isotopic excursions. This is done by using models to unravel processes and while suggesting a mechanism, we could also point on limitations of the performances of some of them, which may support future use of mesoscale isotope models to go further. We propose to add a sentence making the link between atmospheric components of Earth System Models and water isotopes and introducing in l. 61 the importance of the implementation of water isotopes in the atmospheric components of Earth System Models and to confront them with the models as:

"They can also be used as additional tools to test the performance of atmospheric components of some Earth System Models in which water isotopes have been added (Risi et al., 2010; Schmidt et al., 2005; Werner et al., 2011). «

P5, L113-114: The Dumont d'Urville and Concordia stations in Antarctica are not really in the Indian sector, but rather in the Pacific sector. Thus, how these are suitable for understanding the atmospheric water cycle over the Indian basin of the Southern Ocean does not become clear.

>> Many thanks for this question. We will make a more broader statement about South Indian Ocean being a significant moisture source for Antarctic precipitation, notably in the region encompassing Dumont d'Urville and Concordia stations. We will add references to this statement, including Wang et al. (2020) and Jullien et al. (2020) (see figures R1 and R2 below),

References :

Wang, H., Fyke, J. G., Lenaerts, J. T. M., Nusbaumer, J. M., Singh, H., Noone, D., Rasch, P. J., and Zhang, R.: Influence of sea-ice anomalies on Antarctic precipitation using source attribution in the Community Earth System Model, The Cryosphere, 14, 429–444, https://doi.org/10.5194/tc-14-429-2020, 2020.

Jullien, N., Vignon, É., Sprenger, M., Aemisegger, F., and Berne, A.: Synoptic conditions and atmospheric moisture pathways associated with virga and precipitation over coastal Adélie Land in Antarctica, The Cryosphere, 14, 1685–1702, https://doi.org/10.5194/tc-14-1685-2020, 2020.

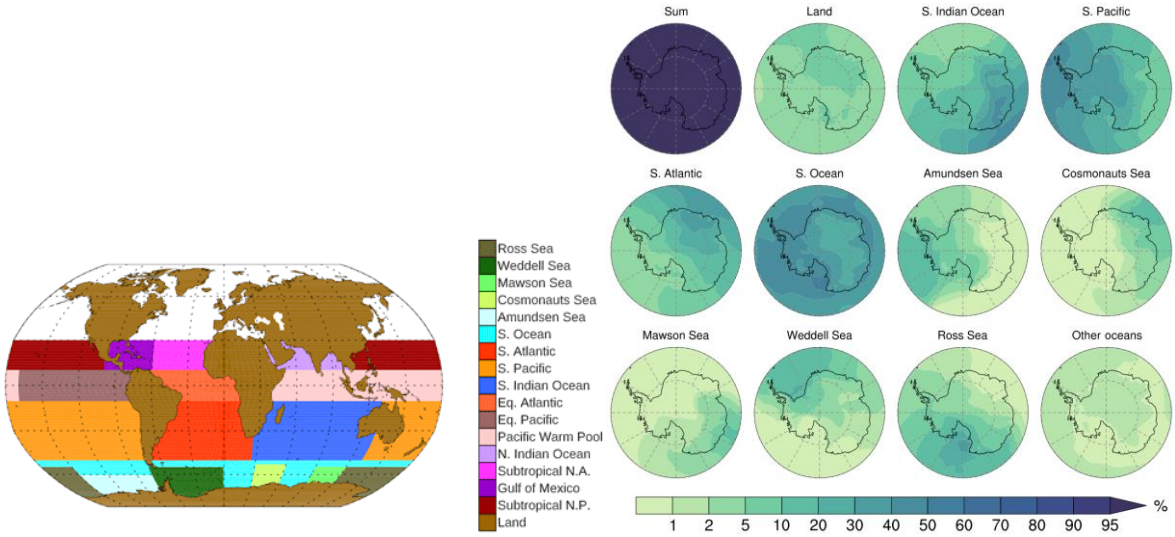

Figure R1 : Spatial distribution of fractional contribution (%) to annual mean precipitation at the surface (right) from individual source regions in the mean case (left). Left panel is taken from Wang et al. 2020, Fig. 2 ; right panel is taken from Wang et al. 2020, Fig. 6

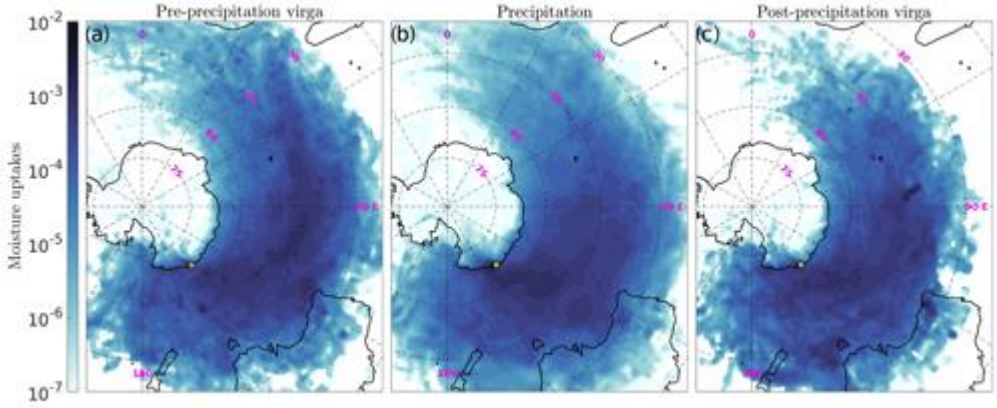

Figure R2 : Composite maps of moisture uptakes for precipitating air parcels over Dumont d'Urville from Jullien et al. (2020) Fig. 10.

We suggest to add the following information in the revised version:

"Amsterdam Island is the only oceanic observatories dedicated to atmospheric studies in the southern hemisphere. This regions in the Southern Indian Ocean is a significant moisture source for Antarctic precipitation, notably in the region encompassing Dumont d'Urville and Concordia stations (Wang et al., 2020; Jullien et al., 2020) "

Figure 1 caption: What is meant with Magand? Add a link or reference?

>> We explain better with "photo taken by O. Magand"

P8, L223: What do you mean here with “low altitude”? That the observatory is located at low altitude? Or that the air from higher altitude is transported down to lower altitudes?

>> Indeed, this sentence if not clear. We propose to modify as:

“The identification of such observational processes (lower GEM concentrations in high-altitude air masses versus marine boundary layer ones) is used here to help indentifying possible intrusions of high altitude air masses down to the surface in Pointe Benedicte Observatory.”

P8, L228: Here now subscripts “v” used, but before not.

>> Actually, when we were writing either “water vapor $\delta^{18}O$” or simply “ $\delta^{18}O_v$” all along the manuscript but we will simplify and put systematic $\delta^{18}O_v$, q_v and δD_v everywhere.

P9, Figure 2 caption: Why is here the anomaly used?

>> This is the standard way to correct the Picarro $\delta^{18}O_v$ and δD_v measurements as detailed in the references given in the manuscript « (Tremoy et al., 2011; Leroy-Dos Santos et al., 2020)”.

P9, L247: What exactly are these standards and how are these derived? Not clear! Are these typical relationships between these species? Are these documented somewhere else?

>> This is very classical when using isotopic composition of water vapor but we agree that it is difficult to follow when not working with this tool. We thus propose to explain better with the modified sentences:

“Here, we introduced two different water standards, EPB-AMS obtained from tap water at LSCE and GREEN-AMS obtained from melting Greenland surface snow. The water isotopic composition of these two standards lie on the global meteoric water line (Craig, 1961): they have respective values of (-5.66 ‰, -47.31 ‰) and (-32.65 ‰, -263.76 ‰) for the couple ($\delta^{18}O$, δD) which also encompass the range of isotopic values of $\delta^{18}O_v$ and δD_v in Amsterdam Island. “

P9, L247: What exactly is denoted by these numbers? The data range?

>> These are the $\delta^{18}O$ and δD values has explained in the sentence above (answer to previous comment)

P9, L254ff: I could not follow you. Why does the data need to be corrected? What did you find here in the relationship that is not as it should be?

>> From Figure 2 of the manuscript, you can see that the value of $\delta^{18}O_{measured}$ - $\delta^{18}O_{standard}$ is not flat as it should be when we inject the same standard at different humidity. This is a well known artefact of the water vapor analyzer which needs to be corrected as in all previous studies dealing with such kind of measurements.

We suggest the modified text:

"While we expect a constant null value for $\delta^{18}O_{measured}$- $\delta^{18}O_{standard}$ on Figure 2 because we always inject the same water standards at different humidity, the $\delta^{18}O$ measurements of both EPB-AMS and GREEN-AMS standards decrease with increasing humidity with the same amplitude. $\delta D_{measured}$-$\delta D_{standard}$ displayed on Figure 2 also shows variations but in contrast to the relative evolution of $\delta^{18}O$ with respect to water vapor mixing ratio, the δD measurements of both EPB-AMS and GREEN-AMS standards exhibit different behavior: δD of EPB-AMS increases by 1.5‰ and δD of GREEN-AMS decreases by 2.5 ‰ over the same 6,000-24,000 ppmv range for water vapor mixing ratio q_v. "

P15, L402: How do you know that these peaks occurred during a cold front? No analyses ofmeteorological parameters indicating a front passage are shown or discussed here.

>> Indeed, we simply wanted to say that these $\delta^{18}O_v$ excursions occurred near the presence of a cold front as observed on weather synoptic maps for each excursion (see answer to reviewer 2, Figure R3). But as explained in the answer of reviewer 2, many cold fronts are also not associated with $\delta^{18}O_v$ excursions and we will refrain from referring to much to the cold fronts in the revised version. We agree that this was confusing and should be improved in the next version.

P14, L403-404: You only picked the 11 excursions with low correlation coefficient between d18Ov and qv. However, according to Fig. 3, there are other negative excursions of d18O besides these 11 cases. If the goal of this study is to show the isotopic features associated with the passage of the cold fronts, the authors should rather pick up the events from the weather chart showing a cold front passage, not from the low correlations alone.

>> As mentioned above, we concentrate on water vapor $\delta^{18}O_v$ excursions and not cold fronts. So we will still pick the events using low correlation coefficient between water vapor $\delta^{18}O_v$ and q_v and the occurrence of $\delta^{18}O_v$ excursions. As explained in our answer to reviewer 1, we will thus precise in the revised manuscript that the $\delta^{18}O_v$ excursions are associated with water vapor $\delta^{18}O_v$ negative excursion larger than 2.5‰ (at 6h resolution) on a total length smaller than 24h (definition of the length of the event is given in the caption of Table 1). We will also precise in the text that the average water vapor $\delta^{18}O_v$ 24h before and 24h after the event should not be larger than 1/4th of the amplitude of the $\delta^{18}O_v$ excursion. Note that some excursions were also discarded because of a too large interruption in the water isotopic record (21st March 2020).

P16, Figure 4: Add a legend so that we can understand which color indicates which data just by looking at it.

>> This has been added, see answers to comments to reviewers 1 and 2.

P17, L454-455: Sentence not clear since it is grammatically incorrect. Please rephrase.

>> We propose this new sentence "Such mismatch makes the understanding of the processes at play during these events particularly important to test to further improve the performances of atmospheric general circulation models equipped with water isotopes."

P18, L475: I still have not seen how you can be sure that there was a cold front passage. How have the cold fronts been detected?

>> You are right. This sentence leads to confusion since we wanted to focus on excursions only. We will better add in table 1 if there is a presence of a cold front during the event (or a few hours before or after) but not state that cold fronts are associated with $\delta^{18}O_v$ excursions which was actually not our aim but it was indeed badly phrased. We will modify this sentence as:

"Several hypotheses can be proposed to explain the negative excursions of water vapor $\delta^{18}O_v$."

Line 504-506: Rain evaporation occurs under the cloud base, moistening the boundary layer. So, the authors should not underestimate the role of rain evaporation because of the high relative humidity near the surface.

>> This is true, we will precise this with this modified sentence making the distinction between what is occurring at the surface and the rain -vapor interaction (including rain evaporation) which can occur higher in the atmosphere:

"Since relative humidity at the surface stays relatively high during these events (values given in Table 1 compared to a mean value of 77 %), it more likely reflects rain-vapor exchanges in the atmosphere than rain evaporation under a transition to dry conditions at the surface."

P19, Figure 6: This figure is not clear at all and needs more explanation. The plot represents the observations in the boundary layer, but the theoretical curves are the isotopic changes in the free troposphere? At least Noone (2012) used them to investigate processes in the tropical mid troposphere. What exactly is meant with "inspired" by Noone (2012)? How have these curves been derived? Do you take these from the Noone (2012) paper? Have you calculated/estimated these yourself?

>> The aim of figure 6 is to look at simple relationships between water vapor $\delta^{18}O_v$ and q_v using simple relationships. Calculation of the curves has been done using the formulas given in the Noone (2012) paper. These formulas are quite simple and could be used for different applications. Of course, it is better adapted to studies in the free troposphere but it has also been used in the past for studies of the isotopic composition of water vapor near the surface (e.g. Guilpart et al., JGR, 2017). We will explain in the revised manuscript that this simple representation is a first order approach for discussing the mechanisms. But because it is not enough, we then use general circulation models equipped with water isotopes in a second part of the discussion.

We hence propose to add the following explanation to introduce our analysis:

"First, to test the hypothesis of vapor-droplet interactions, we looked at the water vapor $\delta^{18}O_v$ and q_v distribution following the approach already used by Guilpart et al. (2017) (Figure 6). We acknowledge that our approach is simple and should be taken as first order approach since we can only look at the water vapor $\delta^{18}O_v$ and q_v distribution in the surface layer while it would be more adapted to look at this relationship in the free troposphere. "

>> We also changed the word "inspired" in the caption of Figure 6 and propose:

"The solid lines are theoretical lines whose equations are detailed in Noone (2012) "

Reference :

Guilpart, E., Vimeux, F., Evan, S., Brioude, J., Metzger, J. M., Barthe, C., ... & Cattani, O. (2017). The isotopic composition of near-surface water vapor at the Maïdo observatory (Reunion Island, southwestern Indian Ocean) documents the controls of the humidity of the subtropical troposphere. *Journal of Geophysical Research: Atmospheres*, *122*(18), 9628-9650.

P20, L523ff: Since subsidence is an important aspect of this study and you use Figure S1-S3 for the discussion, I don't understand why these figures are in the supplement instead of in the main part of the manuscript.

>> You are right and the modified figures according to the corresponding answer to reviewer 2 (figure R5) will be added in the main text.

P21, L623: I still haven't seen any proof that there has been a cold front passage.

>> As mentioned in other parts, this will be modified. Not all cold fronts are associated with $\delta^{18}O_v$ excursions (even if all water vapor $\delta^{18}O_v$ excursions occur in the vicinity of a cold front, Figure R4). We propose to remove the expression "associated with cold fronts" here.

P25, L640: It is still not clear what the function of the models are. Are these only used to be evaluated or are these also used to understand the processes behind the peaks in the d18O time series?

>> Indeed, in this conclusion, we explain that we used the models for the two applications. In the second paragraph (l. 629 - 634), we say that the model-data comparison permits to explain the mechanism at play to explain the $\delta^{18}O_v$ excursion. And in the last paragraph (l. 635 - 640), we say that the kind of water isotopic records provided can be useful to evaluate model equipped with water isotopes.

We propose a new version of the last paragraph to improve clarity:

"We have especially shown that the isotopic composition of water vapor measured at the surface can be a powerful tool to identify aspects to be improved in the atmospheric component of the general circulation models. In our case, we compared our data tomodel outputs with different horizontal resolutions. We showed that, as expected, resolution influences the representativity of the vertical dynamics and have important implication in the simulation of surface variations of water vapor $\delta^{18}O_v$ which can support the future use of mesoscale atmospheric models equipped with isotopes to better explain such abrupt isotopic excursions."

P26, L682: What do you mean with "of the s"?

>> to be changed to "realized most of the simulations"

Supplement: The supplement contains too many figures. I think not all of them are really necessary and the number could be reduced. Further, the formatting should be the same as for the ACP paper, that means no underlined headers and the same style for the figures (no underline of the figure caption title and no italic text for the caption text.)

>> We will indeed reduce the numbers of figures and put some of them in the appendix.

Technical corrections

>> All technical corrections will be taken into account. Many thanks again for your careful reading