From atmospheric water isotopes measurement to firn core interpretation in Adelie Land: A case study for isotope-enabled atmospheric models in Antarctica

ANSWER TO REVIEWERS:

We thank the reviewers for their very interesting comments. They motivated the clarification of synoptic events definition and the reinforcement of their potential impact on isotopic signals during winter. It is key for firn core interpretation and we rewrote and reordered the 3.1 section in order to improve the quality of this analysis. The isotopic data series correction will be clarified and the abstract will be re-written as explained below. The other comments will also be taken into account in the new version of the manuscript as explained below.

Reviewer #1:

1) **The authors show higher frequency of synoptic events in winter. Can the authors specify what the mean with "synoptic events". They state that synoptic events are seen as meridional exchanges, bringing warm moist air masses to the site, but are they always associated with precipitation and are they the only process causing on-site precipitation? What are the reasons leading to these synoptic events? Are they really less frequent in summer or just harder to identify in the meteorological record? It would be great if the authors could elaborate a bit more on these synoptic events, as it seems to be a major driver of d18O of vapor and precipitation and thus also relevant for the interpretation of ice core data.**

We agree that synoptic event definition and detection was unclear in the manuscript. Our study focuses on the main precipitation events, as they are ultimately responsible for the isotopic signal recorded in the firn cores. Firstly, we identify these main precipitation events and conclude that they must be linked to the intrusion of warm, moist air masses from the north, as they are often associated with increased humidity and temperature on a daily time scale, corresponding to synoptic-scale events. Secondly, we observe that the occurrence of these events does not depend on the season (comparison DJF vs JJA), but that their impact on local weather conditions or climatology does. The anomalies (temperature, d18Ov) caused by these intrusions are larger in winter and could have a significant impact on winter variability, as identified by (Servettaz et al., 2020) at ABN.

We will re-write the corresponding paragraph in 3.1 section with the arguments detailed in the previous paragraph in order to address those comments.

2) **The authors show seasonal variations in the relationship between d18OV, T and humidity. Can the authors further evaluate, which processes are responsible for these changes and its relevance for the interpretation of ice core data?**

At DDU, the impact of synoptic events in different weather dynamics (summer diurnal cycles versus winter synoptic variability) are sufficient to explain seasonality of relationships.

In winter the temperature range is larger than in summer: this can be seen in std in Table 1 (std for temperature is doubled in winter compared to summer) and in Figure S4. This is due to the different impact of the synoptic events regarding the season: the rapid switch of air origin in winter will drive the relationship between d18OV and temperature. In summer, the influence of the intrusion of a warm air mass is not very visible because the background climate is already warm.

As for the humidity to d18OV relationship, the higher slope found in winter compared to summer one is expected (see Table 1 and Figure S3) from the relationships between d18O and humidity (or mixing ratio) along distillation line or during mixing of 2 different air masses. We provide below a figure where we plot the relationship between d18O and humidity for 2 processes: 1) pure Rayleigh distillation and 2) mixing of two air masses

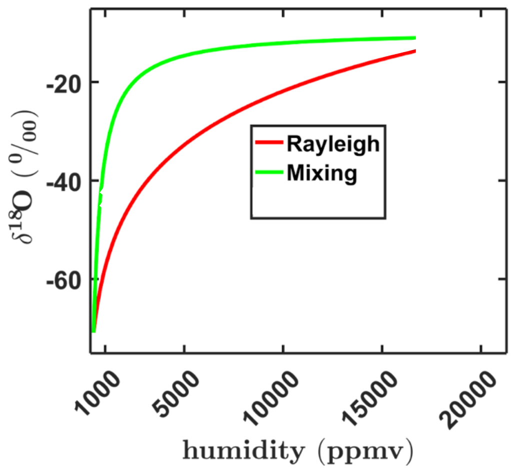


Figure 1: Relationship between the 18O and humidity for the vapor phase in a Rayleigh distillation (red), an air mass mixture (green)

It follows from this figure that the d18O vs humidity slope is larger at low humidity (winter) than at high humidity (summer) as observed in the data. We will give this explanation in the new version of the manuscript.

In the new version of the manuscript, we will reorder part of the 3.1 section in order to reinforce the link between the different weather regimes in summer and winter and the relationships observed between d18Ov variations and climatic parameters. We will discuss both winter and summer weather regimes before we conclude on the different slopes. Then we will argue on the rapid air origin switch to argue on the seasonal differences of the d18O to temperature relationships. In addition, we will add a comment in the manuscript on the impact of slope seasonality on the interpretation of temperature proxies in ice cores: we need to investigate the link between isotopic composition of vapor and precipitation to study the impact on ice-core interpretation as we classically use a unique slope (site dependent) to convert isotopic signal into temperature.

Reviewer #2:

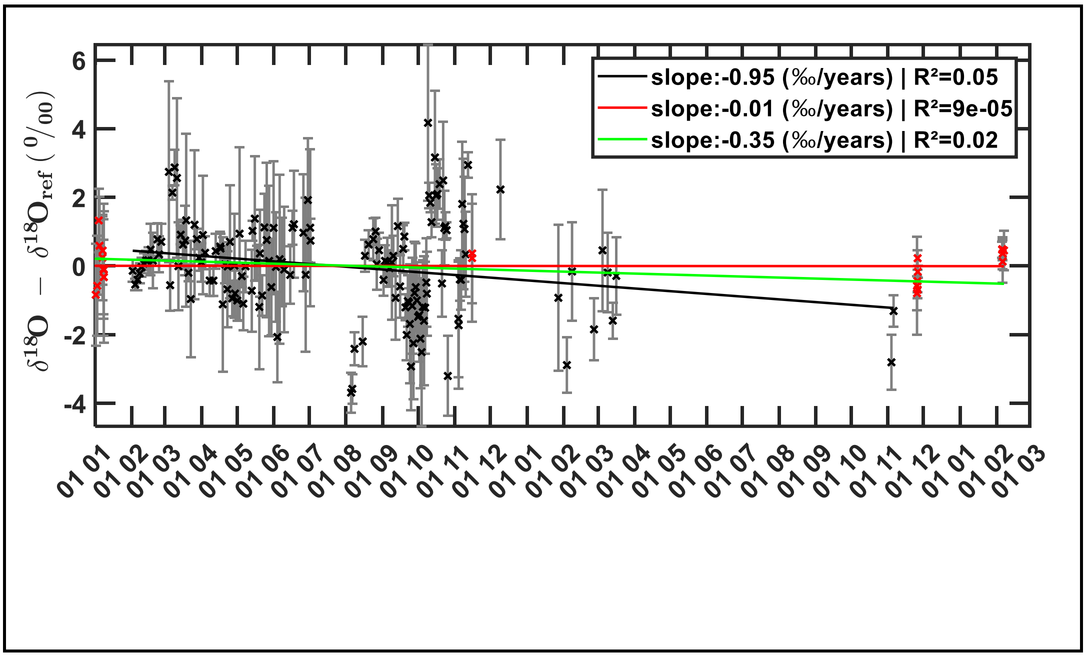
1) **The abstract is a bit cursory without concision, and lacks some important conclusions. I would suggest the authors rewrite it.**

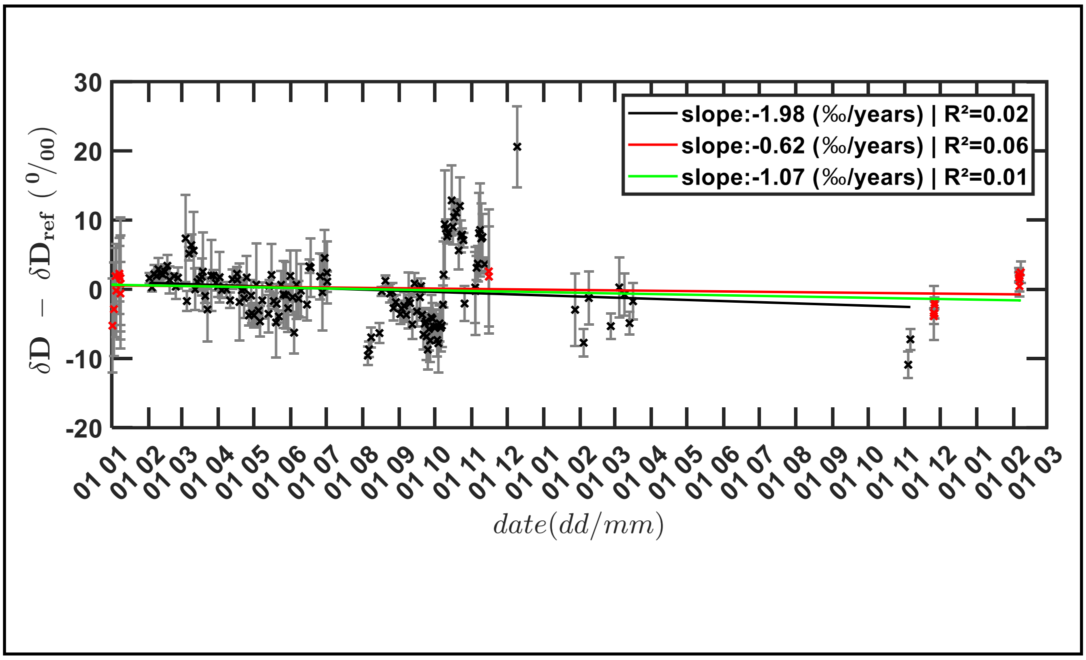
We agree with the reviewer #2 that the abstract could be significantly improved. We will rewrite it and add the following conclusions: 1) ECHAM6 wiso is able to reproduce isotopic signal at DDU at different time scales in water vapour (synoptic and seasonal scales) 2) VFCs built from ECHAM6-wiso data show lower frequency variability than observed (S1C1 firn core): deposition and post deposition effects contribute significantly to the isotopic signal recorded in coastal sites, as well as probably an ECHAM underestimated variability at interannual to decadal scales of precipitation amount and/or isotopic composition.

2) **The description of the isotopic calibration is lacking detail and uncertainties. The plots of drift correction should be provided in SI. Delta 18O-humidity calibration data from SDM are missed in Fig. S2.**

Indeed, there's a problem on figure S2, the data made with SDM are plotted but they are not plotted in the right color (dark gray circles), which explains the confusion, Figures have been replotted and replaced in the Supplementary material.

Regarding the drift correction, in the next manuscript version (in supplementary material), we will add the Figure displayed below showing drift estimation through 48h period routine automatic measurements in comparison with the drift estimated through 4 different humidity calibration sessions. Also, we will comment those results in a paragraph (in the supplementary online material) in order to explain why we do not consider drift correction in regard to the humidity correction applied to the data series. A first proposition for this explanation is given below.





Text SXX: Mean linear drift estimation from different sets of data. Black crosses are routine measurements and red crosses are measurements performed during humidity calibration sessions. Grey bars are the standard deviation associated with each measurement. Black (red) line is the linear drift estimated from routine measurements (humidity calibration sessions measurement). The green line is the linear drift estimated from both data series.

Standard measurements (black crosses) was performed every 2 days with the humidity generator (humidity was set at 1140 ppmv on average, 40 minutes' duration for each measurement level). Some technical issues led us to select only 150 calibrations over the 2 years' period. The results show a drift with decreasing d18O and dD values with time. Unfortunately, the data are very scattered and even sparse after the first year of installation of the instrument. The reason for this scattering is a problem with the humidity generator (bad drying procedure in the instrument) when it was working without human intervention. However, we could perform proper calibrations each year during the field summer seasons (red crosses). Because we are more confident with these measurements, we have only kept these series for the drift estimation.

Mean drift over the two-year period is estimated to 0.01 ‰/years and 0.6 ‰/years for $\delta^{18}O$ and δD, respectively. Because the drift is very small but associated with a high uncertainty, we decided not to correct our data series from mean annual drifts but to associate a large uncertainty with d18O and dD. The uncertainty associated with δ18O and δD measurements in water vapor is calculated as the 70th percentile of the distribution of the 4 annual calibration during the summer season and results in 0.8 ‰ and 3.2‰, respectively. Note that the new version of the LHLG (low-humidity level generator), installed in January 2022 at DDU, does not show any more the scattered patters in the routine calibrations performed on a 48h periodicity and that there is a good agreement between the drift inferred from this routine calibration and the drift calculated from the calibrations performed during the summer season. We will provide these explanations as well as the figures in the supplementary material of the new manuscript.

3) **The influence of synoptic events does not address very well. Some descriptions are not clear at seasonal or events scales. For instance, what is the difference of such events between winter and summer, and what is the reason for those distinct influences in L150-155? How could get the conclusion in L315-317?**

Thanks for this comment which echoes that of reviewer #1. Therefore, we copy below the answer to the similar comment.

It seems that our definition and detection of synoptic events is, indeed, unclear and this makes our result and interpretation confusing. Here we are interested in main precipitation events, as in definitive, they are responsible for isotopic signals recorded in firn cores. First, we identified those main precipitation events (using a daily precipitation rate as described in the manuscript) and then we conclude that they must be related to warm and moist air masses intrusion coming from North (as they are really often associated to humidity and temperature increase on a daily time scale), corresponding to synoptic scale events. Then, we observe that occurrences of such events aren't seasonally dependent (DJF vs JJA comparison) but their impact on local weather or climatology is. Indeed, as pointed by reviewer #1, the anomalies (temperature, d18) caused by those intrusions are more important in winter and could significantly impact winter variability, as identified by (Servettaz et al., 2020) at ABN.

We will re-order and partially re-write the 3.1 section in order to address those comments and the comment #1 from reviewer #1.

Also, this impact asymmetry of synoptic events in regard to season is reflected in relationship between d18 and temperature for example. This will be addressed in comment #2 of reviewer #1. In the modified manuscript, we comment on the rapid air origin switch to argue on the seasonal differences of the d18O to temperature relationships.

Regarding the last part of this comment, we do believe that our new analyses, thanks to the reviewers comments are clearer and that they are not contradictory with what is written in the conclusion: "The warm and wet synoptic events occurring in winter and associated with strong precipitation are clearly imprinted in the water vapor isotopic signal while our precipitation water isotopic signal only captures the strong seasonal cycle.". While winter synoptic events are clearly observable in water vapour signal, the seasonal signal is observable in both vapour and precipitation isotopic signal (see mean values in Table 1).

4) **The evaluation of d-excess from ECHAM6-wiso is missing in section 3.2. The d-excess variability is well established from observations, but does not show any related analysis to combine with simulations. Why?**

We thank the reviewer for this comment. Indeed, there is a lack of comment on the comparison between measured d-excess and d-excess issued from the ECHAM6-wiso model even though comparison is made on Table 3 and Figure S7. From those data, we observe that ECHAM6-wiso is not able to reproduce the second-order parameter d-excess. In fact, apart from the comparable mean values over the two-year data series (8.4 ‰ and 7.8 ‰ respectively for measurement and model output), the model fails in reproducing the seasonality of observed d-excess. Seasonality is actually inverted in the model: while d-excess reaches its maximum in summer (JJA) in observations (10.2‰), its value is minimal in ECHAM6-wiso combination (6.5‰).

We will add this short analysis to the revised manuscript in section 3.2.

5) **I can not get the point clearly in L243-246. It seems controversial with the conclusion.**

   "In particular, the seasonal cycle is well captured both by the observations and model outputs with lower mean δ18O values during winter and higher mean δ18O during summer in both modeled and measured precipitation (Fig. S9). The daily precipitation δ18O samples are however strongly scattered and it is not possible to observe in the precipitation δ18O record (hereafter, δ18Op) an equivalent to the strong peaks observed in the water vapor δ18O during the two strong mid-winter synoptic events (Fig. S9)."

   We understand that this paragraph was not clear enough. We thus propose to add the following explanations at its end.

   "Because the sampling of precipitation was limited to one sample per day and only for the days with precipitation, it is expected that we cannot observe the same d18O signal in the precipitation record than in the continuous water vapor at an hourly resolution."

   Moreover, we do believe that this paragraph is not contradictory with what is written in the conclusion: "The warm and wet synoptic events occurring in winter and associated with strong precipitation are clearly imprinted in the water vapor isotopic signal while our precipitation water isotopic signal only captures the strong seasonal cycle."

   1) **Increase all font sizes in figures and figures are too small to see details.**

We have increased the font size in all figures in the manuscript.

   2) **It is hard to compare the vapor data and precipitation data when they are plotted on separate panels with distinct axis scales in Fig. S9.**

Indeed, the figure S9 does not permit one to easily make this comparison. While comparing signal imprinted in vapor in comparison to precipitation, we should refer to Figure S12. This figure is a scatter plot comparing isotopic signal in vapor versus isotopic signal in precipitation for both model and observation. We will change the referencing in the modified manuscript.