

Review Leroy-Dos Santos et al “From atmospheric water isotopes measurement to firn core interpretation in Adelie Land: A case study for isotope-enabled atmospheric models in Antarctica”

The manuscript entitled “From atmospheric water isotopes measurement to firn core interpretation in Adelie Land: A case study for isotope-enabled atmospheric models in Antarctica” by Leroy-Dos Santos et al. presents the first 2-year continuous isotope record (d18O, dD) of atmospheric water vapor and daily precipitation at a coastal site in East Antarctica. The authors show that observed seasonal and interannual isotope variability of atmospheric water vapor and precipitation are well captured by an isotope-enabled atmospheric general circulation model (ECHAM6-wiso), considering the contribution of oceanic and continental influences. They demonstrate that the ECHAM6-wiso allows to better explain the isotope signal of ice cores at coastal sites, considering not only temperature, but also precipitation intermittency, diffusion and post-depositional effects. The results of this study have important implications for the interpretation of firn and ice core data in terms of paleotemperatures.

The methods and results are well presented, and the discussion is detailed, well-written and reasonable. I have two general comments, where I would like the authors to elaborate a bit more in detail:

- 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.
- 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?

Further, there are a number of passages that may be reformulated to be more precise and concise. Some of them are listed below. I would encourage the authors to take care of using concise language when revising the manuscript.

Minor comments / recommendations:

Abstract

Line 7: Can you specify the isotope composition of which compartment of the water cycle do you mean? Do you want to refer here to ice core records?

Line 9: You should specify that “humidity” refers here to the atmospheric water mixing ratio not to atmospheric relative humidity.

Line 11: You may want to state here if you also found a relationship to humidity. And, what does these relationships between d18O, T, and humidity imply? Is there a relation between the

d18O vs T relationship and the d18O vs humidity relationship (i.e. do T and humidity correlate)?

Line 17: Instead of giving an outlook can you specify how this link between vapor and precipitation helps to interpret short firn cores?

Introduction

Line 34: Remove “providing”.

Line 51: Remove “record”.

Line 53: I think there is no need to start a new paragraph here.

Line 69: In the named study, comparison of water vapor isotope data and back trajectory simulations was performed to ... ?

Line 71: suggested change to “the instrumental setup and the 2-year isotopic series”. Remove “analysis of a”.

Line 72: Suggested change to “Then, we show the ECHAM6-wiso outputs [...]”

Line 73: Remove “evaluated”.

Methods

Line 85: The start of this sentence sound strange to me. I suggest to avoid “in the following study we discuss” and reformulating to be more concise, e.g. “The laser spectrometer measures molecular water vapor mixing ratio and ...”

Line 92: To be more concise, I suggest: “The d18O and dD series were calibrated following ...”

Line 102: Please specify “isotope-humidity relationship”. You may also highlight that you do not observe a difference in this relationship in the field and at LSCE.

Line 116: Can you specify how many replicates per sample? (2-3?). You do not use an independent standard that you measure routinely in each sequence that you could use to estimate uncertainty?

Line 123-125: Can you specify for what these data are used? (Comparison to isotope data? / Identification of processes driving isotope variability of atm water vapor/precipitation / input for ECHAM6-wiso model). If you use the ERA5 reanalysis data solely for nudging the ECHAM6-wiso model, you may consider combining this section with the following.

Results

Table 1: Specify that the isotope composition of atmospheric water vapor is shown, not for precipitation. Verify throughout the paper if there is a need to specify if you mean atm water vapor or precipitation.

Table 1: Why there is no value for the slope between d18O vs Temp in DJF? I think something shifted in this table (cf. Fig. S4). Please check.

Line 147: According to Table 1, also d-excess shows higher values in summer (10.2) than in winter (7.8), but the difference is not significant?

Line 153: “[...] lead to higher variability [...] when there are larger meridional temperature and moisture gradients than in summer.”

Line 167: I can't find these numbers in Table 1. Please check if something shifted. You may also consider showing the plots for the full period in Fig S3 and S4. Also, I don't think that a slope of $0.5 \text{ ‰}^\circ\text{C}^{-1}$ is significantly different from $0.6 \text{ ‰}^\circ\text{C}^{-1}$. In contrast, the slope seems to be significantly higher in fall and winter than in spring and summer. Please verify.

Line 169: Is it the synoptic events that are not visible or rather the impact of the synoptic events on the meteo data?

Line 170: The observation of diurnal cycle only in summer is very interesting. However, std given in Table 1 is based on daily means. Hence, diurnal variability should not affect this value, shouldn't it?

Line 202: Remove “temperature at DDU”.

Line 237: You may consider highlighting the two precipitation peak events in figure 5.

Line 238: “The amplitude of these peaks in ECHAM is comparable to that of measurements.”

Table 3: Consider combining with Table 1, as half of the presented information are identical.

Discussion

Line 253: Suggested change to: “In the following, we explore how [...]”

Line 254: Suggested change to: “For this, we use [...]”

Conclusion

Line 319: I do not see the nice agreement (in absolute values) for all three parameters simultaneously. Approach c fits d18OV, but produces an offset of 400 ppmv in humidity, while temperature is always about 4-5°C underestimated by the model. Irrespective of the offset in absolute values, however, the observed trends are well reproduced by the model. You may want to specify this here. Also, you do not show model results for approach a and b, so that the reader remains unknown if these model approaches also allow to reproduce general trends. You may do so in the supplement.

Line 326: Typo. “Our results”, not “are results”.