

Dear editor,

We would like to thank the two reviewers for their comments. We have been working toward a new version of the manuscript taking their respective comments into account. We include and number the reviewers' comments in **black**. The referees' comments have been addressed individually, as requested by the journal.

Our responses are in **blue**, and the modifications to the manuscript in **red** in this response file.

On behalf of all the co-authors,
Agnese Petteni

Reviewer #1:

Review of “Air Mass Origin Effects on Antarctic Snow Isotopic Composition: An Observation and Modelling Study” by Petteni et al.

The authors present a study analyzing the stable water isotopic composition of snow samples collected in East Antarctica. These measurements are complemented with back-trajectory calculations, ERA5 reanalysis data, and output from two models (LMDZ6iso and a snow metamorphism model). Together, these datasets are used to address the key question of what climatic information is preserved in the stable water isotopic composition of firn and ice cores, particularly in low-accumulation regions such as the East Antarctic Plateau. This question has been addressed by a number of studies in the previous years highlighting the relevance of the topic presented in this manuscript. The manuscript fits into the aims and objectives of the EGU sphere, but I suggest revisions before publication.

Overall Comments

- 1) The study addresses two major aspects: i) the analysis of different air mass origins, and ii) the influence of post-depositional effects, particularly sublimation, on the isotopic signal of surface snow. However, only the first aspect is reflected in the current title. I recommend revising the title so that both themes are represented equally.

We agree with the referee, and we adapt the title as follow:

“Air mass origin and local impacts on Antarctic snow isotopic composition: an observation and modelling study”

- 2) Overall, the manuscript is generally well written but would benefit from thorough proofreading before final submission. Figure labelling is inconsistent: in several cases, panels are labelled “left” and “right” in the captions but referred to as “a” and “b” in the text. Please adopt a consistent style throughout the manuscript (e.g., “a, b, c...”).

We have corrected the text and accordingly adjusted the figures, as suggested by the referee.

- 3) In addition, several figure captions lack sufficient detail. For example, Figure 1 does not explain the abbreviations for the locations shown on the left panel, and the right panel is missing proper axis labels (is the x-axis in kilometres?). Please also cite any mapping software used (e.g., Quantarctica).

We have corrected the figure as suggested by the referee. We included a statement at the end of the manuscript in the acknowledgement to state that the maps were plotted with Matlab using the mapping toolbox.

- 4) In the Results section, some passages read more like discussion, while in the Discussion section new results and figures are introduced. I recommend ensuring that results and discussion are clearly separated.

We agree with the referee’s comment. We have moved sections that were previously in the Discussion into the Results, and transferred all paragraphs related to the discussion of the results into the two Discussion subsections concerning air mass origins and sublimation. This has improved the clarity of the two parts.

- 5) At multiple points, the manuscript states that “significant” differences or impacts are observed, but no explanation is given as to how significance was determined. Please clarify the methods used to assess significance and discuss the findings in relation to uncertainties. Similarly, model uncertainty is not considered when comparing observations with model output (e.g., Fig. 11).

The referee is correct. We have revised the sentences where we previously mentioned a “significant” difference or impact, indicating the p values < 0.05 (Pearson correlation).

Regarding model uncertainty, we have addressed this point by adding a new Fig. 10 in the revised manuscript, which provides an improved comparison between observations and model output. In this figure, we explicitly include the LMDZ6iso model uncertainty in predicted values, as reported by Dutrievoz et al. (2025).

When sublimation is included, the mean absolute difference between modelled and observed values decreases for both $\delta^{18}\text{O}$ and d-excess, demonstrating an improved agreement with observations when accounting for this process.

We have implemented the results with Fig. 10 and presented it in the Results section:

Line 350: “The difference between observed versus modelled final values are shown in Figure 10. Red symbols represent Section I sampling sites, characterized by high precipitation (> 1 mm w.e.) between outbound and return samplings, for which freshly precipitation represents ~25% of the sampled snow. Blue symbols represent Section II sites with low precipitation (< 1 mm w.e.), characterised by negligible precipitation. The modelled values are presented considering either only the precipitation input or both precipitation and sublimation effects. Including sublimation in the computation reduces the discrepancy for all Section II sites and for the majority of Section I sites. The mean absolute difference decreases from 1.9 to 1.3 ‰ for $\delta^{18}\text{O}$, and from 6.6 to 2.9 ‰ for d-excess.”

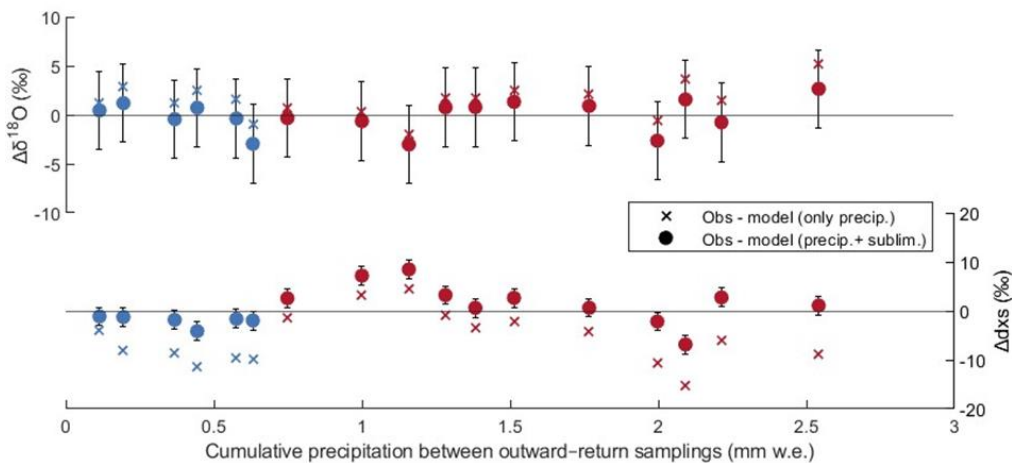


Figure 10. Difference between return-snow observations and modelled $\delta^{18}\text{O}$ and d-excess values, for Section I (red) and Section II (blue). Modelled values are calculated considering either precipitation only (crosses) or both precipitation and sublimation effects (circles). Error bars represent the uncertainty of the LMDZ6iso model in simulating the isotopic precipitation at Concordia (Dutrievoz et al., 2025). The x-axis indicates the cumulative precipitation between the outward and return samplings.

- 6) Figure 11 underpins many of the conclusions, but several issues remain:
- o Model uncertainty is not addressed.

This aspect has been addressed in point 5).

- o ERA5 precipitation uncertainty, which is well known, is not discussed.

We agree with the referee on the importance of discussing the uncertainty in ERA5, and we have added this point in Section 4.3.

Line 439: “Including this sublimation effect in the modelled isotopic predictions substantially improved the agreement with observations, reducing the discrepancy for d-excess compared to simulations considering precipitation input alone (Fig. 10).

This improvement is particularly evident at sites where cumulative precipitation between samplings is negligible (< 1 mm w.e.). The remaining differences and variability between observed and modelled values can be partially attributed to uncertainties in ERA5 precipitation and LMDZ6iso model. Previous studies have shown ERA5 overestimates precipitation over the East Antarctic Plateau, with biases reaching up to 50% relative to satellite-based measurements (Roussel et al., 2020). As a result, our modelling likely represents the maximum contribution of precipitation, implying that the metamorphism would be even greater if actual precipitation were lower. We emphasize that the aim of this study is not to quantify the ability of the combined ERA5-LMDZ6iso in reproduce the absolute isotopic values, but rather to evaluate whether accounting sublimation improved the qualitative representation of surface snow isotopic composition compared to precipitation-only scenarios. These results further reinforced the key role of post-depositional processes in shaping the isotopic composition of surface snow.”

- o The “better agreement” between the return-sample variability and the LMDZ6iso output is claimed but not quantified; moreover, the modelled $\delta^{18}\text{O}$ values show no variability.

This aspect has been addressed in point 5).

- o Wind-driven redistribution, which is a key process on the East Antarctic Plateau, is not mentioned. This process could substantially influence accumulation patterns and isotopic signatures and should be discussed alongside sublimation.

This aspect has been addressed in point 7).

- 7) I agree with the authors that sublimation may have a strong effect on the isotopic composition of snow. However, other depositional and post-depositional processes, especially wind redistribution, should not be neglected. I recommend expanding the discussion to include these processes and their potential impact on the results.

We agree with the referee, we included in the revised version of the manuscript:

Line 426: “On the Antarctic plateau, post-depositional effects mainly include wind-driven snow redistribution, and sublimation.

Snow transport by wind is a relatively local process, typically mixing snow from the surrounding areas and one of the main contributions to stratigraphic noise (Hirsch et al., 2023). Studies suggest that the snow shuffled by wind redistribution can reach distances up to ~100 km (Sarchilli et al., 2010; Frezzotti et al., 2007). Such mixing generates stratigraphic effect between precipitation events, leading to $\delta^{18}\text{O}$ variability of up to 4.4 ‰ within the uppermost 6 cm at Kohnen Station in the plateau interior (Münch et al., 2016). In our dataset, this effect is reduced because we mixed surface snow collected over an extended area at each sampling site and by integrating 1 m of snow depth for bulk samples.

To quantify sublimation, mainly impacting the snow metamorphism in summer, ...”

Specific comments (minor)

- 8) on the specific location in Antarctica with East Antarctica or low accumulation regions having a much larger uncertainty than high accumulation areas in West Antarctica for instance.

Please be more specific here.

We agree with the referee’s comment, and we have implemented the manuscript as follow:

Line 17: “The magnitude of these uncertainties strongly depends on site location, with larger impacts in low-accumulation regions of East Antarctic Plateau.”

- 9) • Secondly, when mentioning that LMDZ6iso captures the spatial variations accurately, it would be nice to see a number showing how well the model captures the variability. It will strengthen the statement.

We agree with the reviewer on the importance of clarifying how the model predicts isotopic composition. In the spatial analysis, we now explicitly state that the LMDZ6iso model distinguishes two different $\delta^{18}\text{O}$ –temperature relationships for the Indian and Pacific sectors. Then, for the modelled values that also include sublimation impact, we provide the differences between observations and modelled values.

Line 26: “Comparison with LMDZ6iso simulations indicates that the model successfully captures the spatial variability of $\delta^{18}\text{O}$ –temperature relationship between different basins, with statistically significant correlations ($p < 0.05$) when the analysis is extended to the Antarctic dataset. This agreement further suggests the model’s ability to predict the temporal slope required for calibrating isotopic ice-core records used for temperature reconstructions, even in regions influenced by multiple moisture sources. Temporal slopes based on monthly precipitation values range from 0.4 to 0.5 ‰ °C⁻¹ for the EAIIST drilling sites. Finally, we quantify the impact of sublimation on isotopic composition of surface snow. Including sublimation in the modelling of surface snow reduces the discrepancy between observed and modelled values, compared to simulations accounting precipitation, from 1.9 to 1.3 ‰ for $\delta^{18}\text{O}$ and from 6.6 to 2.9 ‰ for d-excess. These results highlight the key role of this post-depositional process on the Antarctic Plateau.”

- 10) • L. 42f.: One sentence is not a full paragraph. Please incorporate this sentence into the following paragraph.

Taken into account

- 11) • L. 49: please add an s → distillation pathways

Taken into account

12) • L. 56f.: What is the difference between sublimation, water vapor and vapor diffusion? The referee pointed out that the sentence was ambiguous. We have revised it to improve clarity: Line 60: “Key post-depositional mechanisms include wind-driven snow redistribution and vapor exchange with the atmosphere, such as sublimation-condensation processes, and vapor diffusion within the snowpack driven by forced ventilation (Steen-Larsen et al., 2014; Casado et al., 2021, 2018; Wahl et al., 2022; Ollivier, 2025).”

13) • L. 66: I don’t understand “that fell over the past decade”. What do you mean here? We have adjusted the text following this comment. In the revised version, this sentence has been removed from the introduction, and the corresponding results are now presented directly in the Results section.

- 14) • L. 70: are you using all data from Masson-Delmotte (2008) or only a subset (e.g. the East Antarctic Plateau)?

Taken into account and implemented:

Line 128: “To provide a broader spatial analysis, we compare our data with the Antarctic surface snow database of Masson-Delmotte et al., (2008), which offers a comprehensive overview of isotopic variability across the continent. From the original dataset, which includes different types of snow samples, we selected surface snow, bulk snow, snowpit and firn cores that capture signal ranging from annual to approximately 20 years, based on sample depth and local precipitation rates. For the comparison with our snow samples, we divide the dataset in Pacific and Indian sectors, based on Sodemann and Stohl, (2009). To do this, we classified the region at west of 60°W and the area near the Ross Sea as Pacific sector, while the sampling sites located north then 80°S and between 60°E and 180°E as Indian sector.”

15) • L. 77: please add brackets → Casado et al. (2021)

Taken into account

• Chapter 2.2:

16) o Please carefully check the language in this chapter.

Taken into account

o Please provide more details on the snow samples. Did you take several samples at a location or only one each time? Did you always take a surface and a bulk sample at the same location? Why do you have 85 surface samples but only 52 bulk samples?

Taken into account

Line 94: “Two types of surface snow samples were collected: 85 surface samples, representing the upper 3 cm of snow, and 52 bulk samples, consisting of snow integrated over a vertically dug 1 m-deep snowpit. Surface samples were taken at each stop during daytime approximately every 20 km along the 1,600 km route from DDU to MD, and onward to DC. Bulk sampling required longer processing time and was therefore carried out only during lunch and evening stops”

17) o We know that different labs show discrepancies when measuring the same samples. Have you performed an independent quality control or something similar between both labs?

Taken into account

Line 110: “Previous inter-calibration experiments revealed mean discrepancy between UNIVE and LSCE measurements of the same samples equal to 0.14 ‰ and 0.80 ‰, for $\delta^{18}\text{O}$ and d-excess respectively (Petteni et al., 2025).”

18) o What is your uncertainty for d-excess values? It would be interesting to have a number for d-excess as well, not only $\delta^{18}\text{O}$ and δD , to compare this to d-excess variations.

Line 106: “The accuracy of PICARRO measurements was determined as the mean difference between measured and true values of laboratory standards, with uncertainty represented by their standard deviation. This yielding an accuracy of -0.01 ‰ for $\delta^{18}\text{O}$, -0.07 ‰ for δD , and -0.02 ‰ for d-excess, with corresponding uncertainties of ± 0.07 ‰, ± 0.4 ‰, and ± 0.4 ‰.”

• Chapter 2.4:

19) o l. 130: I don't agree to call the output of ERA5 *snow accumulation*.

Considering the mentioned depositional and post-depositional modifications, I would refer to *snowfall* or *snow precipitation* provided by ERA5.

This has been taken into account and modified accordingly throughout all sections.

20) o L. 133: can you be more specific what you mean with *couple of months*?

Taken into account

Line 155: “represent from 1 to 3 months of snowfalls”

21) • L. 161f.: Are you considering densification for the bulk samples? This might be relevant for the bulk samples on the plateau, considering that they contain up to 15 years of snowfall.

We used density values reported by Ooms et al. (2025), which show limited densification (Fig. A2). The mean densities derived from trench measurements at Dome C are 290 kg m^{-3} for the upper 3 cm of snow and 320 kg m^{-3} for the upper 1 m.

The density values were not reported in the original manuscript and is now included as follows:

Line 146: “Prior to the comparison with ERA5, all snow samples were converted to water equivalent using density values of trench measurements at Dome C (Ooms et al. 2025). The densities are equal to 290 kg m^{-3} for the upper 3 cm of snow and 320 kg m^{-3} for the upper 1 m. For each sample, precipitation events in ERA5 were sequentially accumulated until the target water-equivalent thickness of the sample was reached. Due to the strong gradient in precipitation rates from the coast

(100–300 mm w.e. yr⁻¹) to the plateau (20–50 mm w.e. yr⁻¹), surface samples near DDU represent from 1 to 3 months of snowfalls, whereas those collected on the plateau correspond to up to ~6 months. Similarly, bulk samples represent approximately 1 year of precipitation in coastal areas and up to 15 years at the highest-elevation sites.”

22) • L. 167: cinetic → kinetic

Taken into account

23) • L. 176f.: outburn/outbound → outbound; free-precipitation → precipitation-free

Taken into account

24) • L. 182: Looking at Fig. 2 the DC plot, I also see lines that are colored in blue and orange.

Would that imply that not all 100% are originating from the Indian Ocean?

The referee is right; we have corrected the text accordingly.

Line 211: “At Dome C, 90 % of the back-trajectories originate from the Indian Ocean (red), as expected for this part of the East Antarctic Plateau (Sodemann and Stohl, 2009).”

25) • L. 195: few → please be more specific if possible

Taken into account

26) • Table 1: can you mark Section 1 and 2 in the map of Fig.4 ? Are relationships with R² > 0.5 tested for significance?

Section 1 and Section 2 are now indicated in the figure caption, as suggested. We did not mark the sections directly on the figure, as it is already quite crowded.

The referee is correct that we did not explain how we define statistical significance. R² values greater than 0.5 are shown in bold when p < 0.05 (Pearson correlation).

We have corrected the Table 1 caption as follows:

Line 270: “Table 1. Slope and correlation coefficients (R²) of linear relationships between isotopic composition and geographical/climatic variables, calculated for the Antarctic dataset and Sections 1 and 2 of the EAIIST traverse. Relationships are shown in bold when statistically significant (defined by p < 0.05).”

27) • L. 254: Did you test for significant differences?

No, the difference between the two regression lines was not tested for statistical significance; we only highlighted here the difference between their intercepts.

28) • L. 267R.: For me, this reads already like discussion. You can consider to move this part to the discussion in Section 4.

We agree with the referee’s comment. We have reorganized the Results and Discussion sections accordingly.

29) • L. 285: how did you test for significance?

The referee is right. We didn’t test the difference statistically, we rephrase the sentence as follow to be consistent with the results showed:

Line 289: “The return isotopic composition is, on average, slightly higher for δ¹⁸O by approximately 3 ‰ (Fig. 8a), while d-excess is lower, by 5-10 ‰ (Fig. 8b).”

30) • L. 326 and Fig. 9: All plots with the new data show an R² of 0.9 but 0.8 is mentioned in the text. Please correct this.

Taken into account

References:

Frezzotti, M., Urbini, S., Proposito, M., Scarchilli, C., Gandolfi, S., 2007. Spatial and temporal variability of surface mass balance near Talos Dome, East Antarctica. *J. Geophys. Res. Earth Surf.* 112, F02032. <https://doi.org/10.1029/2006JF000638>

Hirsch, N., Zuhr, A., Münch, T., Hörhold, M., Freitag, J., Dallmayr, R., Laepple, T., 2023. Stratigraphic noise and its potential drivers across the plateau of Dronning Maud Land, East Antarctica. *The Cryosphere* 17, 4207–4221. <https://doi.org/10.5194/tc-17-4207-2023>

Ooms, A., Casado, M., Picard, G., Arnaud, L., Hörhold, M., Spolaor, A., Traversi, R., Savarino, J., Ginot, P., Akers, P., Twarloh, B., Masson-Delmotte, V., 2025. Inter-annual snow accumulation and meter-scale variability from trench measurements at Dome C, Antarctica. *EGUsphere* 2025, 1–39. <https://doi.org/10.5194/egusphere-2025-3259>

Petteni, A., Fourné, E., Gautier, E., Spagnesi, A., Jacob, R., Akers, P.D., Zannoni, D., Gabrieli, J., Jossoud, O., Prié, F., Landais, A., Tchenguiz, T., Stenni, B., Savarino, J., Ginot, P., Casado, M., 2025. Interlaboratory comparison of continuous flow analysis (CFA) systems for high-resolution water isotope measurements in ice cores. *Atmospheric Measurement Techniques* 18, 5435–5455. <https://doi.org/10.5194/amt-18-5435-2025>

Scarchilli, C., Frezzotti, M., Grigioni, P., De Silvestri, L., Agnoletto, L., Dolci, S., 2010. Extraordinary blowing snow transport events in East Antarctica. *Clim. Dyn.* 34, 1195–1206. <https://doi.org/10.1007/s00382-009-0601-4>

Roussel, M.-L., Lemonnier, F., Genthon, C., and Krinner, G., 2020. Brief communication: Evaluating Antarctic precipitation in ERA5 and CMIP6 against CloudSat observations. *The Cryosphere* 14, 2715–2727. <https://doi.org/10.5194/tc-14-2715-2020>

Dear editor,

We would like to thank the two reviewers for their comments. We have been working toward a new version of the manuscript taking their respective comments into account. We include and number the reviewers' comments in **black**. The referees' comments have been addressed individually, as requested by the journal.

Our responses are in **blue**, and the modifications to the manuscript in **red** in this response file.

On behalf of all the co-authors,
Agnese Petteni

Reviewer #2:

Review of “Air Mass Origin Effects on Antarctic Snow Isotopic Composition: An Observation and Modelling Study” by Petteni et al.

Review Comments

The article showcases snow isotope data from an impressive campaign in the East Antarctic plateau and identifies different moisture sources along the transect of the campaign. The data and campaign are very impressive, and the research questions are novel and relevant for readers of *The Cryosphere*. However, I have a few major comments regarding:

- (1) the choice of snow isotope model used,
- (2) the validation of modeling results—particularly in the Pacific sector, where the model seems to have little predictive power, and
- (3) ambiguity between slope values of $\delta^{18}\text{O}$ and temperature ($0.9\text{‰} / ^\circ\text{C}$ shown by observations vs. $0.4\text{‰} / ^\circ\text{C}$ based on model outputs, indicating poor model performance).

Major Comments

- 1) **L125:** Daily ERA5 data from the 2009–2019 period were used to determine 7,300 trajectories for each site, which were then weighted-averaged to calculate moisture trajectories. Instead of using a large ensemble of potential trajectories, why were the *actual* trajectories not calculated using data for the appropriate look-back periods? Wouldn't the results be more realistic if, instead of average trajectories, actual trajectories were used?

In the manuscript, we used two types of snow samples: surface snow, roughly 3 cm thick, accounting for two to three months of accumulation, and bulk samples, averaging from 5 to 20 years of accumulation, as represented on Fig. 1b). The back-trajectory presented here were computing to represent the bulk samples, and in this context, averaging 10 years of precipitation, which showed extremely similar results than Sodemann and Stohl, (2009). We modified the text to indicate that the back-trajectories are representing the origin for each sites, and not for the specific surface/bulk samples that were gathered:

Line 213: “Note that these air-mass origins are more representative of the bulk samples rather than the surface snow, as they are computed as the average over ten years.”

- 2) **L134:** What snow density was assumed to convert bulk snow samples to water equivalent? Were density measurements taken during the campaign, or adopted from previous studies? In L160, it is mentioned that precipitation events were stacked until reaching target snow thickness. Does this mean that density was assumed to be 1?

We used density values reported by Ooms et al. (2025), which show limited densification (Fig. A2). The mean densities derived from trench measurements at Dome C are 290 kg m^{-3} for the upper 3 cm of snow and 320 kg m^{-3} for the upper 1 m.

The density values were not reported in the original manuscript and is now included as follows:

Line 146: “Prior to the comparison with ERA5, all snow samples were converted to water equivalent using density values of trench measurements at Dome C (Ooms et al. 2025). The densities are equal to 290 kg m^{-3} for the upper 3 cm of snow and 320 kg m^{-3} for the upper 1 m. For each sample, precipitation events in ERA5 were sequentially accumulated until the target water-equivalent thickness of the sample was reached. Due to the strong gradient in precipitation rates from the coast ($100\text{--}300 \text{ mm w.e. yr}^{-1}$) to the plateau ($20\text{--}50 \text{ mm w.e. yr}^{-1}$), surface samples near DDU represent from 1 to 3 months of snowfalls, whereas those collected on the plateau correspond to up to ~6 months. Similarly, bulk samples represent approximately 1 year of precipitation in coastal areas and up to 15 years at the highest-elevation sites.”

- 3) **Eq. 2:** I have a general question about the snow metamorphism model used here. The model doesn't include any temperature term, which likely implies no kinetic fractionation during sublimation. Given that sublimation is a kinetically fractionating process (see review by Beria et al., 2018), can you please justify this modeling choice? This might explain the low sublimation fluxes (0.04–0.09 mm w.e. /day from isotopes vs. 0.05–0.35 mm w.e. /day from other studies; see lines 356–358).

That is not correct, the temperature is taken into account in the calculation in particular in the equilibrium fractionation coefficient, but we did not explicitly write it in Equation 2. We modified equation 2 to:

$$R_{sub\ snow}^i = \alpha_{eq-sub}^i(T) \left[R_{snow}^i \left(\frac{D^i}{D} \right)^k (1 - RH) + RH \cdot R_a^i \right]$$

In theory, D^i/D should also change with temperature, but no determination has been made at temperature below 0°C (Casado et al., 2016). We added the following sentence to justify why we used this model:

Line 326: “Here, we assess whether sublimation - commonly associated with an enrichment in $\delta^{18}O$ along with a decrease of *d-excess* (Dietrich et al., 2023; Hughes et al., 2021) - can explain, at least in part, the variations observed. To this end, we compare the relative variations in *d-excess* and $\delta^{18}O$ against the snow metamorphism conceptual model proposed by Casado et al., (2021), which is able to link the relative change of *d-excess* and $\delta^{18}O$ to the sublimated fluxes in the atmosphere (see Section 2.7).”

Indeed, this is strength of this conceptual model, in which the sublimation fluxes can be estimated from the change of surface snow isotopic composition. The model proposed by Casado et al. (2021) has been peer-reviewed and was estimated to be appropriate for Antarctic Plateau sites. The estimated values fall within the range reported by previous studies (Ollivier et al., 2025), and is indicating the average value for up to 50 days, while in Ollivier et al, 2025, they are reporting daily values, which can show large variations of latent heat fluxes for days with different environmental conditions (insolation, cloudiness, temperature, wind speed). As a result, we expect our values to be in a more reduced range since they are averaging cloudy and even snowy days without any sublimation and days with large amount of sublimation. We include the following sentence to the comparison with the results of Ollivier et al, 2025:

Line 340: “The estimated sublimation fluxes range from 0.04 to 0.09 mm w.e. day⁻¹ (Fig. 9), consistent with the range reported in previous studies (0.05–0.35 mm w.e. day⁻¹; Ollivier et al., 2025). It is important to note that the values reported by Ollivier et al. (2025) represent daily estimates and therefore reflect substantial day-to-day variability driven by changes in insolation, cloud cover, temperature, and wind speed. In contrast, our estimates represent averages over periods of up to 50 days. Therefore, they integrate over both days with intense sublimation and days with limited or no sublimation (e.g., cloudy or snowy conditions), resulting in a narrower overall range.”

- 4) **Figure 6:** Section II samples in the left subplot likely do not show any strong correlation with temperature. Without the Antarctic dataset, one cannot obtain a high $R^2 = 0.95$. Doesn't this imply that the dataset collected in this study suggests that a linear relationship between $\delta^{18}O$ and temperature does not exist for Section II? Please report R^2 values for Section II samples and the regression line derived from these datapoints. Additionally, Figure 7 (left subplot) shows that any potential relationship between model and observation breaks down for Section II, suggesting the model doesn't have much predictive power for the Pacific sector.

The referee raises an important point: when considered independently, Section II samples do not exhibit a statistically significant $\delta^{18}O$ –temperature relationship. These values were already reported in Table 1 (Section II $\delta^{18}O$ –T slope = $-0.16 \text{ ‰ } ^\circ\text{C}^{-1}$, $R^2 = 0$, $p > 0.05$), confirming the absence of a meaningful correlation.

We note that Section II is very limited spatially because the traverse did not go further to Pacific coast, while section I goes all the way to the coast. The absence of a spatial isotope-temperature for this section is not an adequate null-hypothesis for the overall absence of such relationship in this part of Antarctica

We have now expanded the Discussion to better highlight that the lack of correlation in Section II is primarily due to the very narrow temperature range covered along this portion of the traverse, which limits the ability to resolve a robust $\delta^{18}\text{O}$ -T relationship

Line 379: “The spatial analysis of EAIIST Section I showed a significant $\delta^{18}\text{O}$ -T relationship over the Indian sector ($p < 0.05$; Tab. 1), while no significant correlation was detected for Section II. It is important to note that Section II has limited spatial coverage, as the traverse did not extend to the Pacific coast, and the temperature range along this 300 km transect is correspondingly narrow. Therefore, the absence of a detectable correlation cannot be interpreted as evidence that a $\delta^{18}\text{O}$ -T relationship is lacking in this sector, as confirmed by analysis of the broader Antarctic dataset (Fig. 6a), which shows statistically significant correlations in both sectors ($p < 0.05$). The spatial slopes for the two basins are similar ($0.9 \text{‰ } ^\circ\text{C}^{-1}$), reflecting comparable thermal history of air mass from the coasts to the high-elevation regions in the interior of the continent (Helsen et al., 2007), while differences in the intercepts are attributed to contrasting vapor isotopic compositions associated with distinct source regions and longer transport pathways for Pacific air masses.”

- 5) **L265–266:** The model only predicts isotopic values for the Indian sector (and not for the Pacific sector), according to Section II samples collected in this study, making this sentence inaccurate. Please see my previous point related to Figure 6. It is important to clearly mention this in the manuscript and add potential reasons for it.

The referee’s comment is correct. The modelled $\delta^{18}\text{O}$ values for the bulk samples along Section II span an insufficient spatial range to robustly assess whether LMDZ can differentiate between the Indian and Pacific $\delta^{18}\text{O}$ -T relationships. Indeed, the bulk samples collected in Section II are not adequate to characterise the Pacific sector, as they only capture the coldest conditions (-50 to $-45 \text{ } ^\circ\text{C}$) (see point 4).

To strengthen the comparison and better support the EAIIST findings, we include in the revised version of the manuscript an additional figure (Figure C2 in Appendix) illustrating the modelled $\delta^{18}\text{O}$ composition for a broader Antarctic dataset. Masson-Delmotte et al., (2008) doesn’t provide a temporal range for samples, we adopt a representative reference period (1980–2000) for all the samples.

This extended dataset provides a more representative temporal coverage and allows a more reliable evaluation of sector-specific $\delta^{18}\text{O}$ -T relationships.

We have improved the manuscript accordingly by adding the following sentences:

Line 389 : “Results from LMDZ6iso model reproduce the inter-sector differences in $\delta^{18}\text{O}$ -T spatial slopes for EAIIST (Fig. 7a and C1a), and for the broader Antarctic dataset (Fig. C2). For this extended dataset, mean $\delta^{18}\text{O}$ values were calculated over 1980-2000 due to the lack of precise temporal coverage of the snow samples in Masson-Delmotte et al. (2008). Although a comprehensive quantitative evaluation of the LMDZ6iso model is beyond the scope of this study, the qualitative comparison indicates that the model successfully captures the observed spatial variability associated with differing air mass influences. This supports the hypothesis that LMDZ6iso is also able to simulate temporal $\delta^{18}\text{O}$ -T slopes.

Monthly $\delta^{18}\text{O}$ -T relationships, derived from modelled precipitation at single sites (i.e. Dome C, AGO5, PALEO and MD; Fig. 11), account for varying moisture sources and yield temporal slopes of 0.4 - $0.5 \text{‰ } ^\circ\text{C}^{-1}$ ($R^2 = 0.9$). At Dome C, this prediction is consistent with observed monthly

precipitation collected at Concordia Station, which exhibits a temporal slope of $\sim 0.5 \text{ ‰ } ^\circ\text{C}^{-1}$ (Dreossi et al., 2024).”

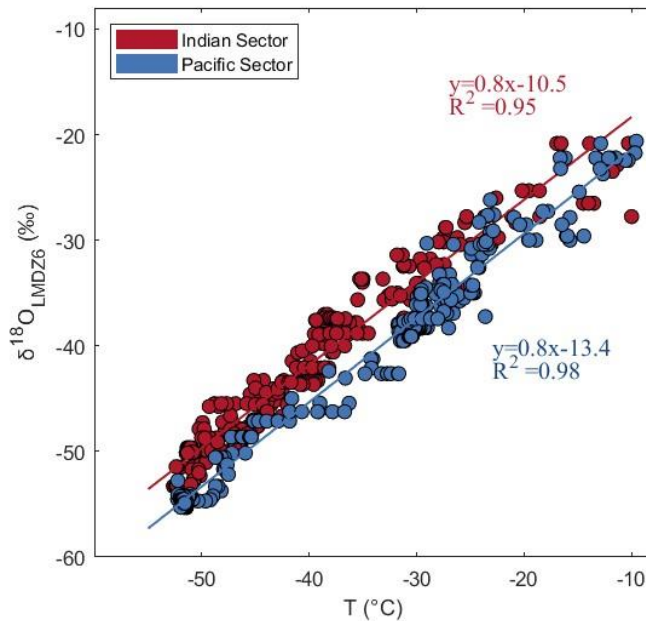


Figure C2. $\delta^{18}\text{O}$ -T relationship predicted by LMDZ6iso for the Antarctic dataset, separated into Pacific and Indian sectors.

6) **L323:** The model does not predict well for Section II (see previous comments).
As detailed in our response to point 5), we have revised the manuscript accordingly.

7) **L326–328:** Can you please explain the reason for differences between monthly snow-precipitation samples and snow-surface samples? These values also seem closer to $\delta^{18}\text{O}$ -Tpw slopes (~ 0.6 ; Figure 3). Any thoughts on this?

In these lines, the term “snow-precipitation sample” refers either to the model outputs, which do not account for post-depositional processes, or to precipitation samples collected after deposition, as in Dreossi et al. (2024). In contrast, “snow-surface sample” refers to the samples collected from the surface during the traverse, as presented in this study.

The difference is better indicated in the text as follow:

Line 399: “We note that in these lines the term “snow-precipitation samples” refers to both model outputs and precipitation samples collected immediately after deposition, as in Dreossi et al. (2024), where snow was captured on a wooden platform positioned 1 m above the surface to minimize post-depositional alterations. In contrast, the term “snow-surface sample” refers to material collected directly from the surface during the traverse, representing an integrated signal of multiple precipitation events and influenced by wind redistribution and post-depositional metamorphism.”

For the model, the monthly snow-precipitation samples yield a $\delta^{18}\text{O}$ -temperature slope similar to that obtained from precipitation observations at Dome C, with temperature representing the precipitation events.

As noted by the referee, this slope closely matches the $\delta^{18}\text{O}$ -Tpw slope calculated for the EAIIST snow samples (Fig. 3), consistent with expectations, since the temperature values used here reflect conditions during precipitation events.

We have modified the text as detailed in point 5).

- 8) **Figure 9:** At Dome C, the $\delta^{18}\text{O}$ –temperature slope is $0.4 \text{ ‰} / ^\circ\text{C}$ based on model outputs, whereas observations suggest a slope closer to $0.9 \text{ ‰} / ^\circ\text{C}$ (Figures 3, 6). Doesn't this imply that the model is not performing well?

We thank the referee for the comments, which highlight that our manuscript did not clearly present the spatial and temporal slopes in this study.

The spatial $\delta^{18}\text{O}$ –T slope is obtained from observations and modelled values distributed over space, and is equal to $0.9 \text{ ‰} ^\circ\text{C}^{-1}$. The LMDZ6 model can distinguish the different origin of air masses between Indian and Pacific sectors (Fig. 7a and C2).

Then, in absence of samples collected at one site over time, we tried to calculate a temporal slope using the LMDZ6iso predictions. At Dome C site, we obtain a temporal slope equal to $0.4 \text{ ‰} ^\circ\text{C}^{-1}$ (Fig. 11), consistent with the temporal slope reported by Dreossi et al. (2024), equal to $0.5 \text{ ‰} ^\circ\text{C}^{-1}$. These spatial and temporal slopes are in line with previous studies which indicate that temporal slopes are generally significantly lower than spatial slopes, often by up to a factor of two (Stenni et al., 2016). So, we can conclude that our model is able to perform well for both spatial and temporal slopes.

We have revised the text to better clarify this distinction:

Line 379: “The spatial analysis of EAIIST Section I showed a significant $\delta^{18}\text{O}$ -T relationship over the Indian sector ($p < 0.05$; Tab. 1), while no significant correlation was detected for Section II. It is important to note that Section II has limited spatial coverage, as the traverse did not extend to the Pacific coast, and the temperature range along this 300 km transect is correspondingly narrow. Therefore, the absence of a detectable correlation cannot be interpreted as evidence that a $\delta^{18}\text{O}$ –T relationship is lacking in this sector, as confirmed by analysis of the broader Antarctic dataset (Fig. 6a), which shows statistically significant correlations in both sectors ($p < 0.05$). The spatial slopes for the two basins are similar ($0.9 \text{ ‰} ^\circ\text{C}^{-1}$), reflecting comparable thermal history of air mass from the coasts to the high-elevation regions in the interior of the continent (Helsen et al., 2007), while differences in the intercepts are attributed to contrasting vapor isotopic compositions associated with distinct source regions and longer transport pathways for Pacific air masses.”

Results from LMDZ6iso model reproduce the inter-sector differences in $\delta^{18}\text{O}$ -T spatial slopes for EAIIST (Fig. 7a and C1a), and for the broader Antarctic dataset (Fig. C2). For this extended dataset, mean $\delta^{18}\text{O}$ values were calculated over 1980-2000 due to the lack of precise temporal coverage of the snow samples in Masson-Delmotte et al. (2008). Although a comprehensive quantitative evaluation of the LMDZ6iso model is beyond the scope of this study, the qualitative comparison indicates that the model successfully captures the observed spatial variability associated with differing air mass influences. This supports the hypothesis that LMDZ6iso is also able to simulate temporal $\delta^{18}\text{O}$ -T slopes.

Monthly $\delta^{18}\text{O}$ -T relationships, derived from modelled precipitation at single sites (i.e. Dome C, AGO5, PALEO and MD; Fig. 11), account for varying moisture sources and yield temporal slopes of 0.4 - $0.5 \text{ ‰} ^\circ\text{C}^{-1}$ ($R^2 = 0.9$). At Dome C, this prediction is consistent with observed monthly precipitation collected at Concordia Station, which exhibits a temporal slope of $\sim 0.5 \text{ ‰} ^\circ\text{C}^{-1}$ (Dreossi et al., 2024).”

- 9) **Figure 11:** Wouldn't it be better to show scatterplots for the different locations (as subplots) and include R^2 values to show model accuracy? With the current figure, it's hard to evaluate whether the model is working correctly.

The referee is right, from the first version of the figure was difficult to distinguish the capability of the model to account the sublimation impact on surface snow. A new Figure 10 is proposed, showing modelled values including only precipitation input and precipitation + sublimation effects. We note that the aim of this study is not to assess the capability of the model in predict the absolute values of the surface snow, but mainly to represent the variability associated with sublimation.

We indicate the capability of the model as absolute differences between observations and modelled values, as follows:

Line 350: “The difference between observed versus modelled final values are shown in Figure 10. Red symbols represent Section I sampling sites, characterized by high precipitation (> 1 mm w.e.) between outbound and return samplings, for which freshly precipitation represents ~25% of the sampled snow. Blue symbols represent Section II sites with low precipitation (< 1 mm w.e.), characterised by negligible precipitation. The modelled values are presented considering either only the precipitation input or both precipitation and sublimation effects. Including sublimation in the computation reduces the discrepancy for all Section II sites and for the majority of Section I sites. The mean absolute difference decreases from 1.9 to 1.3 ‰ for $\delta^{18}\text{O}$, and from 6.6 to 2.9 ‰ for d-excess.”

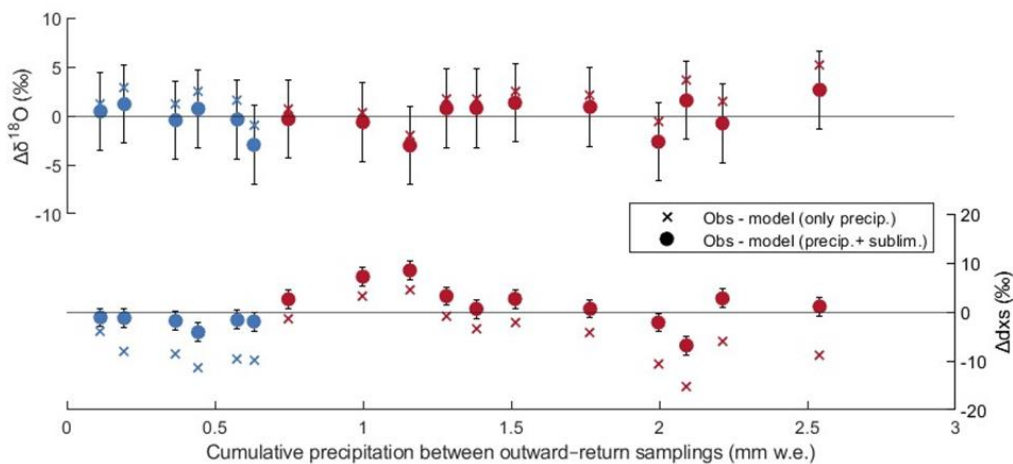


Figure 10. Difference between return-snow observations and modelled $\delta^{18}\text{O}$ and d-excess values, for Section I (red) and Section II (blue). Modelled values are calculated considering either precipitation only (crosses) or both precipitation and sublimation effects (circles). Error bars represent the uncertainty of the LMDZ6iso model in simulating the isotopic precipitation at Concordia (Dutrievoz et al., 2025). The x-axis indicates the cumulative precipitation between the outward and return samplings.

Line 440: “Including this sublimation effect in the modelled isotopic predictions substantially improved the agreement with observations, reducing the discrepancy for d-excess compared to simulations considering precipitation input alone (Fig. 10).

This improvement is particularly evident at sites where cumulative precipitation between samplings is negligible (< 1 mm w.e.). The remaining differences and variability between observed and modelled values can be partially attributed to uncertainties in ERA5 precipitation and LMDZ6iso model. Previous studies have shown ERA5 overestimates precipitation over the East Antarctic Plateau, with biases reaching up to 50% relative to satellite-based measurements (Roussel et al., 2020). As a result, our modelling likely represents the maximum contribution of precipitation, implying that the metamorphism would be even greater if actual precipitation were lower. We emphasize that the aim of this study is not to quantify the ability of the combined ERA5-LMDZ6iso in reproducing the absolute isotopic values, but rather to evaluate whether accounting sublimation improved the qualitative representation of surface snow isotopic composition compared to precipitation-only scenarios. These results further reinforced the key role of post-depositional processes in shaping the isotopic composition of surface snow.”

- 10) L386–387: With the current set of figures, one cannot conclude that the LMDZ6iso model correctly simulates isotopic ratios (see my previous comments on different slopes between observations and models). Adding scatterplots between model and observation (see previous comment) would allow for a more appropriate evaluation.

We note that the aim of this study is not to evaluate LMDZ6iso's ability to simulate isotopic ratios. Previous studies have already show this and we reported in the new version of the manuscript the uncertainty of LMDZ6iso model as indicated by Dutrievoz et al, (2025):

Line 187: "The error in LMDZ6iso precipitation at Concordia, defined as the difference between modelled and observed values for a supersaturation parameter $\lambda = 0.004 \text{ K}^{-1}$, is 4.0 ‰ for $\delta^{18}\text{O}$ and 1.9 ‰ for d-excess."

Here, we want to see if LMDZ6iso was reproducing the impact of the change of origin of the air masses for the Indian and Pacific sectors, which would be key to understand if the model is predicting accurately $\delta^{18}\text{O}$ for the right reason. Through the new figure C2 (see point 5), that is presented in the revised manuscript, we clearly observe that the shift of isotopic composition between the two sectors is accurately reproduced by LMDZ6iso. We include this specifically in the manuscript:

Line 386: "Results from LMDZ6iso model reproduce the inter-sector differences in $\delta^{18}\text{O}$ -T spatial slopes for EAIIST (Fig. 7a and C1a), and for the broader Antarctic dataset (Fig. C2). For this extended dataset, mean $\delta^{18}\text{O}$ values were calculated over 1980-2000 due to the lack of precise temporal coverage of the snow samples in Masson-Delmotte et al. (2008). Although a comprehensive quantitative evaluation of the LMDZ6iso model is beyond the scope of this study, the qualitative comparison indicates that the model successfully captures the observed spatial variability associated with differing air mass influences. This supports the hypothesis that LMDZ6iso is also able to simulate temporal $\delta^{18}\text{O}$ -T slopes.

Monthly $\delta^{18}\text{O}$ -T relationships, derived from modelled precipitation at single sites (i.e. Dome C, AGO5, PALEO and MD; Fig. 11), account for varying moisture sources and yield temporal slopes of 0.4-0.5 ‰ °C⁻¹ (R²= 0.9). At Dome C, this prediction is consistent with observed monthly precipitation collected at Concordia Station, which exhibits a temporal slope of ~0.5 ‰ °C⁻¹ (Dreossi et al., 2024)."

11) L400: It is unclear where you see a 0.5 ‰ / °C slope in any of your figures. Observations suggest a slope of 0.9 ‰ / °C (Figures 3, 6).

We thank the referee for the comment, which highlight that our manuscript did not clearly presents the spatial and temporal slopes in this study (see also point 8).

Spatial $\delta^{18}\text{O}$ -temperature slope from observations is equal to 0.9 ‰ °C⁻¹ (Fig. 6a), but there is a difference of about 7‰ in the intercept of these two relationships for Indian and Pacific sectors. The model is shown to accurately reproduce this difference in the intercept, with equal spatial slope of 0.9 ‰ °C⁻¹ (Fig. C2).

We then used the model to assess the temporal $\delta^{18}\text{O}$ -temperature slope at different single site over monthly values. As the referee correctly notes, this temporal slope is not exactly 0.5 ‰ °C⁻¹, but ranges between 0.4 and 0.5 ‰ °C⁻¹ across the main sampling sites along the EAIIST traverse (Fig. 11). These values are consistent with the temporal slope derived from precipitation observations at Dome C (Dreossi et al., 2024), which is equal to 0.5 ‰ °C⁻¹.

We insist on the difference between the temporal and spatial relationships within this manuscript because historically, the isotopic paleothermometer has been calculated from the spatial relationship (Dansgaard 1965, Lorius 1969), but several studies have shown that this is not accurate and that there are differences (Casado et al, 2017). We believe that this specific example is extremely interesting because the two sectors are nearby and have relatively similar climatic conditions, despite having followed two independent air masses history.

We included this in the discussion:

Line 372: "The spatial $\delta^{18}\text{O}$ -T relationship has traditionally been used to calibrate isotopic paleothermometer (Dansgaard, 1964; Lorius et al., 1969). However, several studies have shown that it is generally less accurate than the $\delta^{18}\text{O}$ -T temporal slope derived at a single site (Sime et al., 2008;

Casado et al., 2017). The EAIIST traverse, which crosses two sectors of East Antarctica influenced by distinct air mass origins yet characterised by relatively similar climatic conditions, provides valuable insights into the role of moisture sources in shaping both spatial and temporal slopes. These observations also highlight the potential to model such relationships in regions where long-term temporal records are not available.”

Minor Suggestions

12) L27: It might be better to phrase it as “temporal slope between xxx and yyy”.

Taken into account

Line 29: “Temporal slopes based on monthly precipitation values range from 0.4 to 0.5 ‰ °C⁻¹ for the EAIIST drilling sites.”

13) L33: Add “stable” before “water isotopic composition”.

Taken into account

14) L43: Why is there a paragraph break at the end of this line?

Taken into account.

15) L47: It is important to also highlight post-depositional processes that modify snow isotopes here.

The referee’s comment is appreciated. We have now improved the text to clearly indicate that post-depositional processes affect the overall isotopic composition of the snow:

Line 45: “Additionally, the reliability of water isotopes as climate proxy from ice core is debated due to the local influence of post-depositional processes in the snowpack. These processes can alter the snow isotopic composition during prolonged exposure at the atmosphere-snow interface and modify the original precipitation’s isotope-temperature relationship (Petit, 1982; Stenni et al. 2016, Touzeau et al., 2016, Casado et al., 2018). Key post-depositional mechanisms include wind-driven snow redistribution and vapor exchange with the atmosphere, such as sublimation-condensation processes, snowmelt and vapor diffusion within the snowpack driven by forced ventilation (Steen-Larsen et al., 2014; Beria et al., 2018; Casado et al., 2018; Wahl et al., 2022; Ollivier, 2025). Among these, sublimation plays an important role especially during the warmer months, decreasing d-excess values (Landaï et al, 2017, Casado et al, 2021).

These post-depositional alterations are poorly understood and often overlooked in climatic reconstructions, underscoring the need to better constrain the processes shaping the isotope-temperature relationship in surface snow to improve the reliability of ice core-based temperature estimates (Xiao et al., 2013; Ma et al., 2020)”

16) L52: There is a double period (“..”) in this line.

Taken into account

17) L57: Why is “condensation” mentioned as a post-depositional mechanism? Do the authors mean vapor condensation on the snowpack at night? If yes, wouldn’t this already be accounted for under “water-vapor exchange”? This clarification is important, since this section explains the key post-depositional mechanisms.

Taken into account. Re-phrased following referee’s comment see point 15).

18) L56–58: Is snowmelt a relevant process in this landscape (especially during summer)? If yes, melt is another post-depositional process that can fractionate snow isotopes (see review by Beria et al., 2018).

Taken into account. Re-phrased following referee’s comment and including snowmelt.

19) L63: It would be useful to give insights into the depth or accumulation period represented by “surface snow samples”.

Taken into account, rephrased as follow:

L 75: “The surface and bulk sample types reflect precipitation over time scales ranging from seasonal to multi-annual periods, respectively”

20) L70: Why “snow precipitation”? Does the model differentiate between snow and rain precipitation?

The referee is correct. We have removed the term “snow” and re-phrased this sentence.

Line 73: “We then evaluate the ability of the atmospheric general circulation model LMDZ6iso to reproduce the observed $\delta^{18}\text{O}$ -temperature spatial slope by comparing surface snow observations with modelled precipitation.”

21) L65–66, L70–72, L86: These are key results of the study; the Introduction/Method sections are not appropriate places for them.

Taken into account. The referee’s comment was very helpful in making the context clearer. We have removed these results from the Introduction and the Methods and reported them only in Results and Discussion sections.

22) Figure 1: On the y-axis of the right subplot, please include a break symbol between 2016 and Jan 2018 to make this visually clearer. Currently, there is only a small whitespace separating these periods.

Taken into account

23) L177: Typos: “Outbound”, “input”.

Taken into account

24) L264: Should be “observations”.

Taken into account

25) L269: Should be “assimilate”.

Taken into account

26) L282: Typo: “composition”.

Taken into account

27) L287: Typo: “post-depositional”.

Taken into account

28) Figure 8 description: Typo: “prior” (not “priior”).

Taken into account

29) L393: Typo: “average metamorphism”.

Taken into account

Reference:

Beria H., Larsen J. R., Ceperley N. C., Michelon A., Vennemann T., Schaefli B. (2018). *Understanding snow hydrological processes through the lens of stable water isotopes. WIREs Water*, 5:e1311. <https://doi.org/10.1002/wat2.1311>