

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 ($0.9\text{‰} / \text{°C}$ shown by observations vs. $0.4\text{‰} / \text{°C}$ 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 $^\circ\text{C}$) (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).”

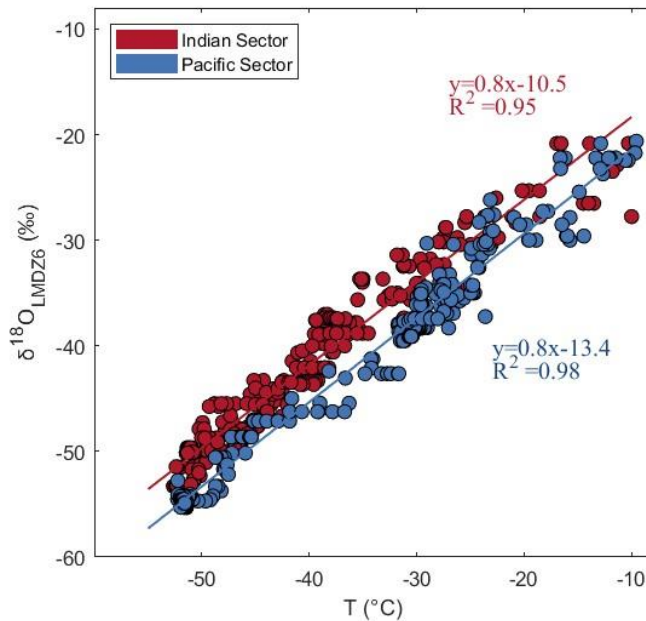


Figure C2. $\delta^{18}\text{O}$ -T relationship predicted by LMDZ6iso for the Antarctic dataset, separated into Pacific and Indian sectors.

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.”

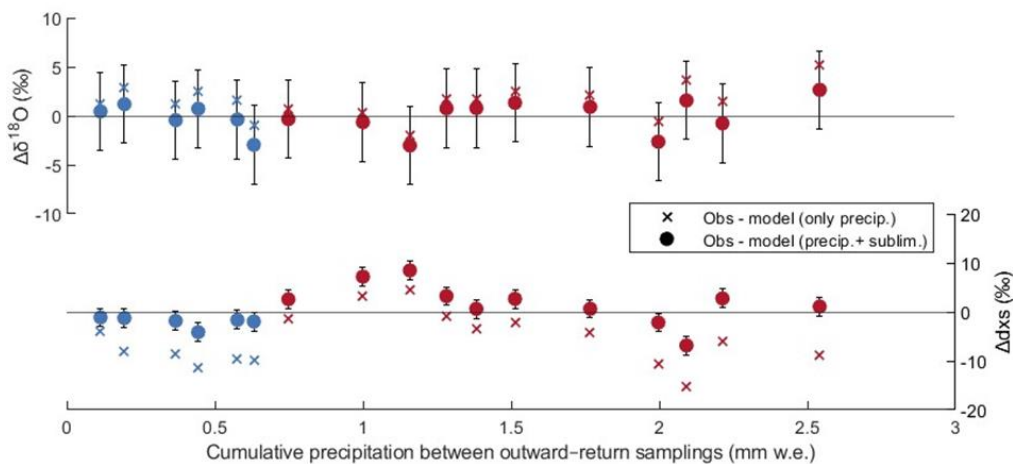


Figure 10. Difference between return-snow observations and modelled $\delta^{18}\text{O}$ and d-excess values, for Section I (red) and Section II (blue). Modelled values are calculated considering either precipitation only (crosses) or both precipitation and sublimation effects (circles). Error bars 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 (Landais 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>