

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 question are novel and relevant for readers of The Cryosphere. However, I have a few major comments on: (1) choice of snow isotope model used, (2) validation of modeling results, particularly on the Pacific sector where model seems to have little predictive power, and (3) ambiguity between slope values between  $^{18}\text{O}$  and temperature (0.9 per mil / C shown by observations vs 0.4 per mil /C based on model outputs, showing poor model performance).

### **Major comments**

L125: Daily ERA5 data from 2009-2019 period was 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 result be more realistic if instead of average trajectories, actual trajectories are used?

L134: What is the snow density assumed to convert bulk snow samples to water equivalent? And were density measurements taken during the campaign, or used from previous studies? In L160, its mentioned that precipitation events were stacked until reaching target snow thickness. Does this mean that density was assumed as 1?

Eq.2: I have a general question about the snow metamorphism model used here. The model doesn't have any temperature term in it, 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 the modeling choice here? This might be one of the reasons behind low sublimation fluxes (0.04-0.09 mm weq / day with isotopes vs 0.05 to 0.35 mm weq / day with other studies; see lines 356-358)

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

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 in reference to Figure 6. It is important to clearly mention this in the manuscript, and add potential reasons for this.

L323: The model doesn't predict well for Section II (see previous two comments)

L326-328: Can you please explain the reason for difference between monthly snow precipitation samples and snow surface samples? These values also seem closer to 18O-Tpw slopes (0.6; Figure 3). Any thoughts on this?

Figure 9: Dome C slope between 18O and temperature is 0.4/C based on model outputs, however observations suggest that the slope should be closer to 0.9/C (Figures 3, 6). Doesn't this imply that the model is not working well?

Figure 11: Wouldn't it be better to show scatterplots instead for the different locations (as subplots) and plot R2 value showing the accuracy of the model? With the current figure, it's hard to say if the model is even working correctly

L386-387: with the current set of figures, one cannot say that LMDZ6iso model correctly simulates isotopic ratios (see my previous comments on different slopes between observations and models). If you add a scatterplot between model and observation (see previous comment), one can make an appropriate judgement.

L400: I don't get where you are seeing 0.5 mil / C slope in any of your figures. Your observations are suggesting slope of 0.9 mil / C (Figures 3, 6).

#### **Minor suggestions:**

L27: I think it might be better to phrase it as "temporal slope between between xxx and yyy".

L33: add "stable" before "water isotopic composition"

L43: why is there a paragraph break at the end of this line?

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

L52: Period (.) is twice in this line.

L57: Why is "condensation" mentioned as a post-depositional mechanism? Do authors mean vapor condensation on snowpack at night? If yes, wouldn't this already be accounted for in "water vapor exchange"? This is important because this line explains "key post-depositional mechanisms".

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

L63: It will be useful to give some insights into depth or period of accumulation in “surface snow samples” definition.

L70: why “snow precipitation”? Does the model differentiate between “snow precipitation” and “rain precipitation”?

L65-66, L70-72, L86: These are key results of this study, Introduction / Method are not the appropriate sections for key results

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

L177: Typos: “Outbound”, “input”

L264: Should be “observations”

L269: should be “assimilate”

L282: “composition” typo

L287: “post-depositional” typo

Figure 8 description: “prior” (no ‘i’ in prior) typo

L393: “average metamorphism” typo in the text

Reference:

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