

We appreciate the reviewer for the time and efforts to review this manuscript, and for the suggestions/comments which improve the manuscript significantly. Below we list detailed responses to the reviewer's suggestions and comments. The comments are listed in italics, followed by the response in normal font.

## Comments

*Jiang et al. revisited previously published Summit snowpack nitrate isotope data using a snow photochemical column model (TRANSITS). It was found that post-depositional processes at Summit can explain the observed seasonal variability of  $d15N$  without considering the source variability of reactive nitrogen species. Although the modeling result may be further tested and improved by observation-based parameterizations of some parameters such as  $d15N$  of atmospheric nitrate, this work challenges previous expectations and highlights that post-depositional processes on isotopic compositions of cryospheric nitrate at a site with relatively high snow accumulation rates like Summit still should be carefully considered in the future. The model is scientifically sound and its uncertainties are well discussed. I expect that the findings will stimulate more studies. I recommend its publication in The Cryosphere and only have some minor suggestions to improve the clarity of this manuscript.*

**Response:** Thank you for the comments.

*Lines 30-35: I would suggest the authors to introduce  $d15N$  as well because  $d15N$  is the major focus of this study.*

**Response:** Thanks for this suggestion. We have added such text and relevant references in the introduction part to illustrate the importance of snow/ice core  $\delta^{15}N(NO_3^-)$ .

*Line 49: As stated later by the authors, “the degree of post-depositional processing and the induced effects on snow nitrate and isotopes vary site by site” (Line 56). The values presented here seem too precise to represent the fractionation factors “under typical polar conditions”.*

**Response:** We removed the use of “under typical polar conditions” and specified it as Dome C local conditions. This is just to show how large the fractionation factor could be.

*Lines 51-55: The local atmospheric chemistry of photolysis product  $NO_2$  as stated earlier (lines 42-47) may also alter  $D17O$  values.*

**Response:** Thanks for pointing this. We added a sentence to illustrate this point as follows: “On the other hand, the photoproducts of snow nitrate released into local atmosphere would rapidly reform nitrate and redeposit in situ, thus imprinting the snow  $\Delta^{17}O(NO_3^-)$  with local atmospheric oxidizing conditions.”

*Lines 60-63: The logic may be more clear if the authors can give the snow accumulation rate at Summit and discuss whether post-depositional processes were expected based on this number.*

**Response:** We have added “Summit, Greenland is a typical high snow accumulation site ( $250 \text{ kg m}^{-2} \text{ a}^{-1}$ , Dibb et al., 2004), and under this high snow accumulation condition nitrate would be preserved better compared to low snow accumulation sites such as at Dome C, but the magnitude remains unquantified.”

*Lines 65: It may be better to quantify “surface” (<3 cm?) so that readers can compare the number to 30-40 cm without reading Fibiger’s papers.*

**Response:** Fibiger et al. (2013) stated that their surface snow sample comprised the top 1-2 cm snow, while in Fibiger et al. (2016) they became 0.5-3 cm in depth. In the revised manuscript, we have added the overall range of their samples (<3 cm) as reference.

*Lines 82-89: The term “snow-sourced” nitrate appears many times in the rest of manuscript but not here. I would suggest the authors to define “primary” and “snow-sourced” nitrate at the beginning.*

**Response:** Thanks for this suggestion. We have introduced these two terms in the introduction.

*Line: 170: The value of  $k$  is not given.*

**Response:** It is  $3 \times 10^{-12} \text{ cm}^3 \text{ molecule}^{-1} \text{ s}^{-1}$  and has been added.

*Lines 178-182: It is unclear how the epsilon-d is determined and which value is used. (I suggest the authors to give a table, in either main text or supplementary materials to show all parameters and values used in the model and sensitivity tests).*

**Response:** The  $\epsilon_d$  (+10%) was chosen from Erbland et al. (2013). The +13% difference from Fibiger et al. (2013) was not used here because this value was measured in late spring and could possibly be perturbed by photolysis of snow nitrate. In our revised manuscript we have made the value clearer. We have also added a table in supplement file to show the parameters and values in our simulation for better reference. Thanks for this suggestion.

*Lines 264-271: There are too many terms that are similar but not clearly organized (FD, FP,  $F_{pri}$ , snow-sourced). They could have been more clear. I would suggest the authors to define everything at the very beginning (e.g., Section 2). Otherwise it may be difficult for readers to follow.*

**Response:** Thanks for this suggestion. In the revised manuscript, we defined  $F_{pri}$  and snow sourced nitrate in the introduction, and the other two in sections where they first mentioned.

*Figure 2: Since FP is zero in winter, the authors may want to remove red lines during wintertime in panels b and c.*

**Response:** Thanks for this suggestion. We have revised the figures and removed the wintertime part of the  $\delta^{15}\text{N}/\Delta^{17}\text{O}(\text{FP})$  in our new figures.

*Lines 299: The definition is not clear. What is the baseline?*

**Response:** PIE refers to the difference between  $\delta^{15}\text{N}(\text{NO}_3^-)$  of surface snow and the same layer that buried below photic zone. We have added this statement to make it more specified in our revised manuscript.

*There are some grammatical errors that need to be corrected (e.g. Lines 172, 230, 248).*

**Response:** Revised as suggested.