We appreciate the reviewers for their time and efforts to review this manuscript. Below we list detailed responses to their suggestions and comments. The suggestions and comments are in italics, followed by the response in normal font with changes highlighted in blue.

**Comments from Reviewer2**

*This paper presents some previously unpublished data on the isotopic values in nitrate in air samples from Summit, Greenland. It also compiles previous data on isotopes in nitrate in air, surface snow and "archived" snow at the same site. The paper then focusses on a discussion of the role of snow photolysis of nitrate in influencing the observed isotope values, their seasonality, and the differences between air and snow.*

*There are a few general comments to make about the paper. Firstly, the new data are potentially very useful and deserve to be published, even if I have a few questions about them. The authors have also done a nice job of compiling previous data, shown in Figure 2, which serves as an excellent starting point for a discussion.*

*The discussion is quite a tough read, even for someone who is quite well-versed in the issues but this probably reflects the difficulty of making clear statements in the light of sparse data, and a definite divergence in opinion between the major groups working on this topic. My main concern with the paper is that some statements are made, sounding quite definite, that are based on differences that are highly marginal. I realise it is disappointing when the conclusion of a study is "we're not sure if this is real", but in some cases this would have been a fairer conclusion. I think the overall structure of the paper and the way it tries to use the different datasets is good, so my comments mainly focus on particular statements that seem too definite or not to be well-supported by the data shown. For that reason, I just go through the paper in order, with both minor and major comments mixed in.*

*Title: the word "reality" seems a bit misplaced here. Of course one can read in the text an undercurrent that the message of the paper is directed at a competing group and that this paper is saying "there really is an effect". But I think for the neutral reader it would be less provocative and more accurate to write "evidence for the postdepositional effect".*

**Response:** We're grateful to the reviewer's comments which make us more cautious about our statements in the paper. We agree that although the complied dataset reveals some general features that are more or less consist with our current understanding, the limited data resolution as well as the large uncertainties somewhat don't allow definite conclusions. We have weakened our statements in many places to better focus on whether the observed systemic trends can be explained by post-depositional processing, instead of directly concluded that these are the "reality". We also change the new title in to "Impacts of post-depositional processing on nitrate isotopes in the snow and the overlying atmosphere at Summit, Greenland".

*Abstract: line 24: since you argue that you have collected aerosol and gas-phase nitrate, the word "aerosol" should be removed here.*

**Response:** Thanks for this suggestion. We have substituted the phrase "aerosol nitrate" into "atmospheric nitrate" throughout the text.

*Abstract: line 28: you should review the wording "no apparent seasonality". I will discuss this later, at line 248.*
**Response:** We have rechecked the atmospheric $\delta^{15}N(NO_3^-)$ data in spring (March to May) and indeed found a significant negative shift compared to other seasons (two side T-test, $p = 0.01$). We have rewritten the sentence as follows:
"…The atmospheric $\delta^{15}N(NO_3^-)$ remained negative throughout the year, ranging from –3.1 ‰ to –47.9 ‰ with a mean of (–14.8 ± 7.3) ‰, and displayed a minimum in spring which is distinct from the observed spring $\delta^{15}N(NO_3^-)$ maxima in snowpack…".

*Abstract: please review what you have written in the light of edits elsewhere in the paper. I am particularly concerned that lines 37-43 are stronger than the data really allow (see later).*
**Response:** Thanks for this comment. In the revised manuscript, we have reduced the discussions on the relative importance of photolysis-induced fractionation and cage effect on snowpack $\delta^{18}O(NO_3^-)$, and reworded the abstract as follows:
"…The atmospheric $\delta^{18}O(NO_3^-)$ varied similarly as atmospheric $\Delta^{17}O(NO_3^-)$, with summer low and winter high values. However, the difference between atmospheric and snow $\delta^{18}O(NO_3^-)$ was larger than that of $\Delta^{17}O(NO_3^-)$. We found a strong correlation between atmospheric $\delta^{18}O(NO_3^-)$ and $\Delta^{17}O(NO_3^-)$ that is very similar to previous measurements for surface snow at Summit, suggesting that that atmospheric $\delta^{18}O/\Delta^{17}O(NO_3^-)$ relationships were conserved during deposition. However, we found linear relationships between $\delta^{18}O/\Delta^{17}O(NO_3^-)$ that were significantly different for snowpack compared to atmospheric samples. This likely suggests the oxygen isotopes are also affected before preservation in the snow at Summit, but the degree of change for $\delta^{18}O(NO_3^-)$ should be larger than that of $\Delta^{17}O(NO_3^-)$ given that photolysis is a mass-dependent process.".

*Line 51: Wolff 2008 is not in the reference list, whereas Wolff 1995 is, but is not cited in the paper.*
**Response:** Sorry for this omission. The right reference shall be Wolff et al. (2008) ACP paper. We have added it in the revised manuscript.

*Line 148, 156 and surrounds. Obviously a lot hangs on the quality of the atmospheric data. I have two issues I'd like clarified. The first concerns the use of GF filters. I agree they have often been assumed to collect gas phase nitrate as well as aerosol but the evidence is quite minimal for polar sites; the mechanism is assumed to be through attachment to sea salt loads on the filters (see eg Wagenbach et al, JGR, 103, 11007-11020, 1998 for a discussion of this, albeit related to cellulose filters). Given this I propose that Fig 2 (or a supplementary figure) should show a comparison of the concentrations of nitrate in this study compared to those found in previous studies* *(including Fibiger and Jarvis) that used mist chambers. This would allow a more informed discussion of whether this study is reporting a similar fraction of total nitrate to earlier studies.*

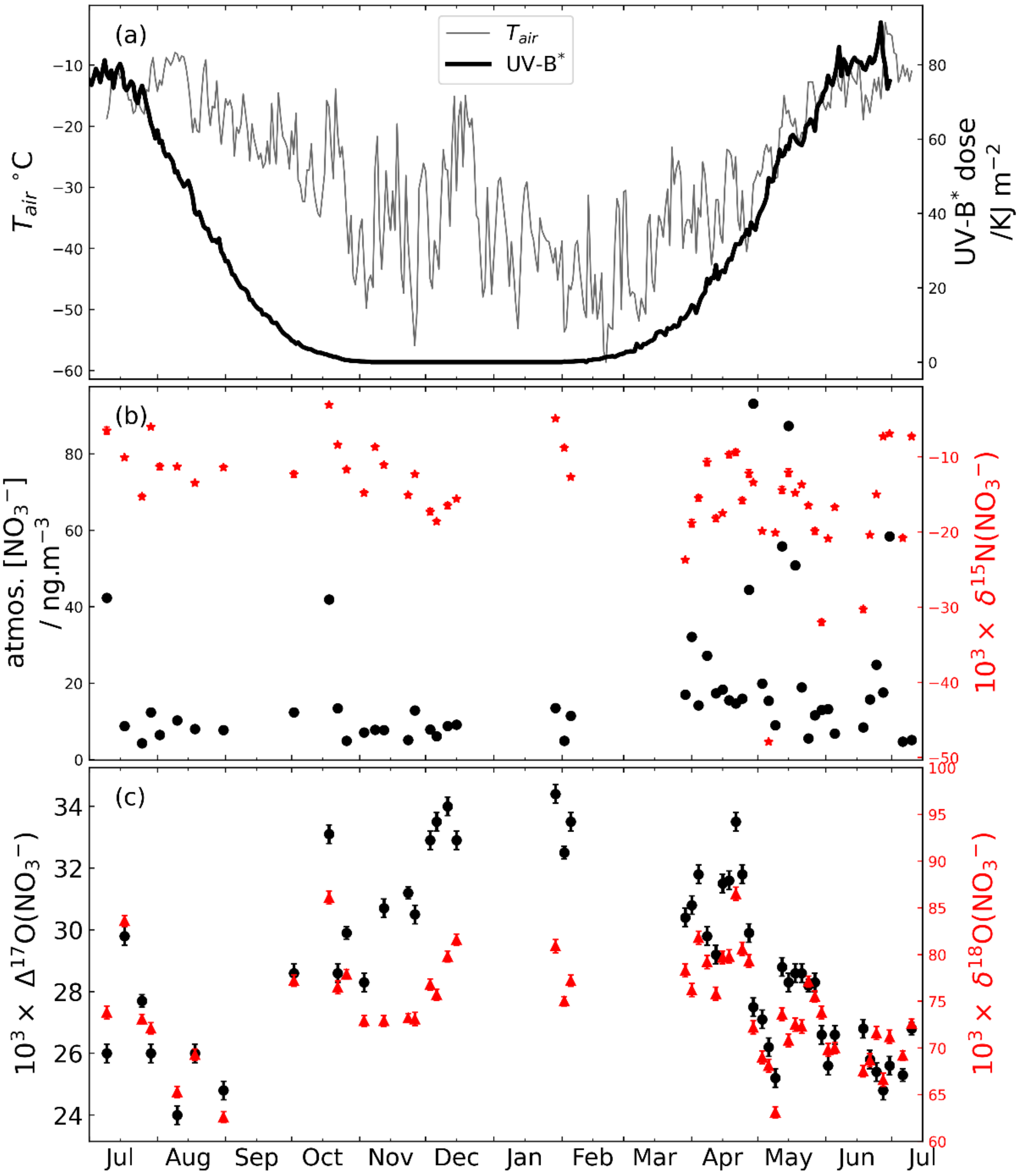
**Response:** We agree that quantitative collection efficiency is necessary to ensure no artifact in the measured isotope data. The HVAS+GF filter has been shown to be capable to quantitatively collect atmospheric nitrate at Dome C (Erbland et al., 2013) by comparing with the annular denuder method. Erbland et al. (2013) suggested that this is because the high NaCl blank in the GF filter can improve the collection efficiency, as it's known that sea salt aerosol could trap the gaseous nitric acid via chloride-substitute reaction. The similarity of Dome C and Summit is that at both sites gaseous nitric acid dominates total atmospheric nitrate (>90% at Dome C, 94% at Summit, Dibb et al., 1994).

We have compiled the measured atmospheric nitrate concentration in different years at Summit and summarized in the table below. As shown in the table, our results are in general consistent with others, except for Jarvis et al. (2009) who reported a much higher value than all other studies by using mist chamber. This should reflect the collection efficiency and in the revised manuscript, we added this table as SI.

| **Year** | **Month** | **type** | **Conc** (ng $m^{-3}$) | **Reference** |
|---|---|---|---|---|
| 1991 | 7-8 | denuder | 38 ± 53 | Silvente and Legrand, 1993 |
| 1993 | 6-7 | Mist chamber | 55 ± 37 | Dibb et al., 1994 |
| 1993 | 5-7 | Teflon Zefluor filter | 26 ± 2.9 | Bergin et al., 1995 |
| 1994 | 5-8 | Mist chamber | 32 ± 37 | Dibb et al., 1994 |
| 1995 | 4-7 | Mist chamber | 27 ± 32 | Dibb et al., 1998 |
| 2001-2002 | annual | glass fiber filters | 19.9 ± 19.1 | This work |
| 2006 | 5-7 | Mist chamber | **202** | Jarvis et al., 2009 |
| 2010 | 5-6 | Mist chamber | 32 ± 30 | Fibiger et al., 2016 |
| 2011 | 5-6 | Mist chamber | 42 ± 22 | Fibiger et al., 2016 |

*In addition, I am a bit alarmed by the observation that nearly half the collected samples were discarded because they were too close to the blank (for nitrate concentration, I assume). This might imply that there remains a significant blank component in many of the filters that were not discarded and this could then affect the isotopic ratios measured if the blank is contributing significantly. Please comment on this (I would assume you have some isotopic measurements on blank filters?).*
**Response:** Yes, we have collected a total of 9 blank filters during the sampling period and found a significant amount of nitrate in these blank samples. The average nitrate concentration in the extracted filtrate for all atmospheric samples were (1363 ± 1603) ng $g^{-1}$, while that of the nine field blank samples were (183 ± 44) ng $g^{-1}$. For the sake of validity, we excluded the samples with concentration less than three times of the average concentration of these blank samples in further analyses. This procedure excluded a fair amount of total samples especially in September. The blank samples were also collected together to measure its isotope ratio the same as measuring atmospheric samples. And we deducted the contribution from the blank to obtain the real isotope ratio for atmospheric samples. We have added these details about sample handling in the revised manuscript.

*Figure 1: I find it a bit strange that you choose not to plot the data chronologically but instead that you have Jan to Jul 2002 followed by Jul-Dec 2001. I would propose plotting the data chronologically (jul-Jul) in Fig 1, and from Jan-Jan in Fig 2.*
**Response:** Thanks for this suggestion. We now plot the data chronologically in the revised manuscript.



*Line 208-209. I don't request a change but I note that this is a bit circular. You use the similarity in seasonality to support your seasonal assignments in snowpack, and then later you use the same alignment as evidence that capdelta-17O in particular is unaffected by photochemistry.*

**Response:** We agree. But $\Delta^{17}O$ is not the sole indicator to support our snowpack dating, we also used $Na^+$ (peaked in winter) and $Cl^-/Na^+$ ratio (peaks in summer) to distinguish the seasons.

*Line 243. You attribute spikes to Arctic Haze events. Could it also be that it reflects more efficient scavenging during inputs of high sea salt (you would be able to support or deny this by looking at seasalt in the aerosol data)?*

**Response:** This is a good suggestion. Unfortunately, the high NaCl blank of the filters prevent this. We have checked the seasonality of aerosol $Na^+$ concentration at Summit

form another study (Rhodes et al., 2017) and found the $Na^+$ concentration is generally higher from January to May than June to September. This seems to coincide with our observation that nitrate concentration mostly peaks in April and May. Thus we cannot rule out this possibility. We add the following statement in our revised text:
"…There was no distinct seasonal pattern in atmospheric nitrate concentrations, but some spikes (samples with much higher nitrate concentrations than average) in spring/summer months were observed, typical to intrusion of Arctic haze events at the altitude of the Ice Sheet (Quinn et al., 2007; Jaffrezo et al., 1997). Alternatively, these nitrate concentration spikes could reflect more efficient scavenge of atmospheric nitrate by sea salt aerosol during transport, as indicated by the elevated $Na^+$ concentration in Summit aerosol during April and May (Rhodes et al., 2017).".

*Line 248. You say there is no distinct seasonality in the atmospheric 15N. But I look at Fig 2b, where you also show the snowpack 15N from Geng, which you claim has a clear seasonality. While obviously the aerosol data have large variability within each month, I see just as strong a seasonal dip in the aerosol data as I see a seasonal peak in the snowpack data. In the end this isn't crucial because it's the differences between the air and the snowpack in different months (which is clear) that you focus on, but still please reword more cautiously.*
**Response:** Thanks for pointing this out. We have rechecked our dataset and found that the spring $\delta^{15}N(NO_3^-)$ was in fact on average lower than other seasons (two-side t-test, p=0.001). In the revised manuscript, we have rewritten the relative statement as following:
"…The atmospheric $\delta^{15}N(NO_3^-)$ was negative throughout the year with an annual mean of (–14.8 ± 7.3) ‰. The springtime atmospheric $\delta^{15}N(NO_3^-)$ exhibited a significantly lower shift compared to other seasons (two-side t-test, p = 0.001), and the average for the winter half year (–12.0 ± 4.2) ‰ was slightly higher than that in the summer half year (–16.0 ± 3.9‰) …".

*Line 257: "($^{18}O$) displayed an almost identical seasonal pattern with $\Delta^{17}O(NO_3)$ as expected". I'm sure if I'd read your previous papers I would know why this was expected but it's not obvious, given that the former is a mass dependent fractionation and the latter is a mass independent one that could be quite separate. Please spell out why it's expected.*
**Response:** Thanks for this comment. We have added the following statement in the revised version:
"…The atmospheric $\delta^{18}O(NO_3^-)$ data ranged from 49.7 to 86.5 ‰ and displayed an almost identical seasonal pattern with $\Delta^{17}O(NO_3^-)$. The similar seasonality between $\delta^{18}O(NO_3^-)$ and $\Delta^{17}O(NO_3^-)$ is expected. At the seasonal scale, the primary controlling factor of atmospheric $\delta^{18}O(NO_3^-)$ and $\Delta^{17}O(NO_3^-)$ is the relative importance of $O_3$ versus $HO_x$ to nitrate formation in different seasons. In summer, HOx oxidation is more important and leads to nitrate with lower $\delta^{18}O(NO_3^-)$ and $\Delta^{17}O(NO_3^-)$, while in winter $O_3$ oxidation is more important and leads to higher $\delta^{18}O(NO_3^-)$ and $\Delta^{17}O(NO_3^-)$ (Alexander et al., 2020; Michalski et al., 2012)".

*Figure 2. In part a, please clarify that the curve refers to the actinic dose (not "does") that would have been experienced by the snowpack samples.*

**Response:** Thanks for this suggestion. We have made more detailed illustrations in the caption as follows:

"The cumulative UV-B* dose represents the actinic dose that would have been experienced by snow nitrate deposited at different times of a year."

*Fig 2e: I assume these seasalt data refer to the snowpack data (but then which: Geng et al?). Please clarify this in the caption. Also, I'd be really surprised if the Na are in mg/L, surely they are ppb or ug/L?*

**Response:** Thank you so much for this, the data is from Geng et al. (2014), and should be at μg $L^{-1}$. We have made the relevant corrections in the revised manuscript.

*Fig 2 caption. "The vertical lines represent the interval of seasons". I don't understand what this means. Are the error bars the differences between years for the same month/period, or are they the variability within a month or season. This is crucial to understanding what values are significantly different to others.*

**Response:** The four dashed lines in Fig 2 represent the beginning and ending of each season (simply defined as March 1st, June 1st, September 1st and December 1st, respectively). The error bars represent one stander error calculated from all data available from a certain month or season from all years (some of them have data point from one year, but others have data from several years). We have made this clearer in the revised manuscript.

*Line 290. This is the first case where I really feel you say things the data don't support. You refer to a progression of 15N from atmosphere to surface to snowpack. However when looking at Figure 2, it would be really stretching it to say that the surface snow data of Fibiger et al are significantly different from the snowpack data, taking into account the error bars shown. I agree there is a difference between the single Jarvis data point but as you later question this data I don't feel it's justified to make a wide-ranging and repeated statement about a progression on the basis of that. To me this is a place where you have to say that there is a clear difference between atmosphere and snow, but the data are insufficient to state with any certainty whether the surface snow and snowpack are different. The same issue is repeated in line 340. (As an aside if the Jarvis atmospheric data in Fig 2b are right then the variability in the atmosphere between years is also too high to make a clear statement but I think it's Ok just to have noted the discrepancy).*

**Response:** We agree that the Fibiger's surface snow $\delta^{15}N(NO_3^-)$ data are not very different with the snowpack $\delta^{15}N(NO_3^-)$ data compiled from Geng et al. (2014). However, **when looking at the Jarvis et al. (2009) original surface snow and snowpack data,** these differences are quite clear, i.e., surface snow $\delta^{15}N(NO_3^-)$ in spring and summer were (-6.8 ± 0.5) ‰ and (-2.5 ± 1) ‰ respectively, while snowpack $\delta^{15}N(NO_3^-)$ in spring and summer were (4.4 ± 1.9) ‰ and (2.4 ± 2.1) ‰

respectively. These are also seen in Figure 2b but their snowpack data points were somewhat hidden behind the data from Geng et al (2014). In the revised manuscript, we have changed the color of the symbols to emphases the Jarvis et al 2009 data. Note the Jarvis 2009 atmospheric data is not justified (high concentration compared to all other studies), but **their surface snow and snowpack data should be valid**. What is more, their surface snow and snowpack samples are corresponding to each other for the same season, see the figure below, and a difference is clearly seen for spring and summer. So we think the difference between surface snow and snowpack is real, but Jarvis et al. (2009) only reported seasonal averages (the original data in the below figure was not available) so we only plotted the seasonal averages in Figure 2b of our manuscript.

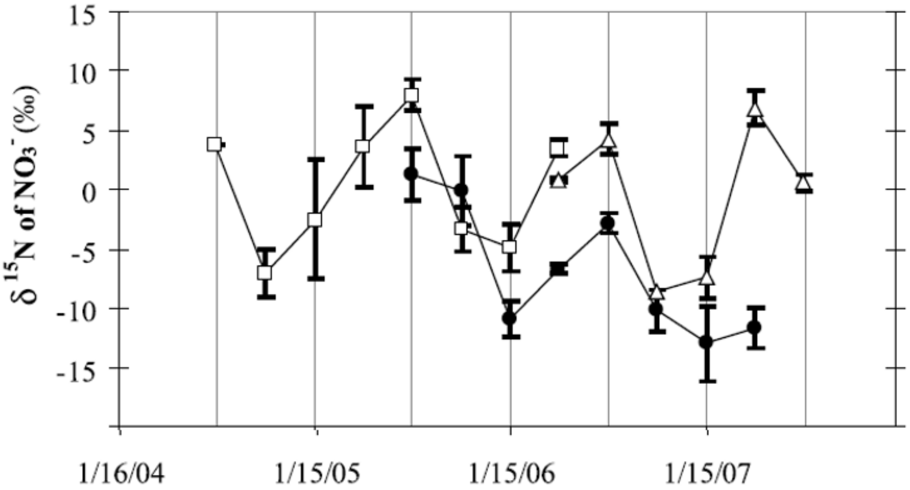


**Figure 1.** The comparison between surface snow (solid circles) and snowpack $\delta^{15}N(NO_3^-)$ (open square and triangles for two different snowpits) at Summit (Jarvis et al., 2009).

*Fig 3: Why do you only show J against SZAs (in the inset) almost entirely smaller than those experienced at Summit in the main figure?*
**Response:** Thanks for this suggestion. We have narrowed the range of SZA in the inset.

*Line 413, Fig 3 and surrounding discussion. I am really mystified by this lengthy section. Of course it's a nice advance that you can find a simple formula to represent the complex output of the model for PIE. However you don't then use it. Apparently PIE is the "difference between surface snow 15N(NO3–) and archived snow $\delta^{15}N(NO_3^-)$" . So why don't you plot the actual data in Fig 3 and see if it agrees with the model and its simplified (eq 3) representation. And of course the answer is that it doesn't. The observed PIE in May-July looks from Fig 2b to be about 3 permil, not the predicted values of about 10. (We can argue about the significance of the single value for spring, which you later suggest you don't believe). In any case my point is that there is no point having this section and figure unless you also show and discuss* *the data.*

**Response:** The comparison between the modeled PIE (using TRANSITS model) and observed PIE has been presented in our previous paper in a more direct manner (please see the Fig2 below; Jiang et al., 2021). The observed PIE was calculated based on the observations in Jarvis et al. (2009). Again, the Jarvis et al. (2009) atmospheric data is not justified because of the high concentration, but we didn't find reasons why their snow data are also not justified. From the Figure 2 below it can be seen that the modelled PIE is generally consistent with the observations (adapted from Jiang et al., 2021). We think that the observed PIE is strong evidence of post-depositional processing occurring at Summit, though it was not the major conclusions in this study (detailed in Jiang et al., 2021 and we tried to avoid repeating the same discussion or comparison).

In addition, in this study we wanted to provide a simplified (but accurate) equation that we could be easily used by others to evaluate the impact of photolysis on snowpack $\delta^{15}N(NO_3^-)$ without a complex modelling exercise. We have stated it more clearly in our revised manuscript as follows:

"To better understand the effects of the photo-driven post-depositional processing, we quantitatively compared and analyzed the $\delta^{15}N(NO_3^-)$ averages in spring when the isotopic differences between surface snow and snowpack are the most pronounced as indicated by the compiled data and the modeling results by Jiang et al. (2021). Since the surface snow $\delta^{15}N(NO_3^-)$ data in Fibiger et al. (2016) only covered two months, we mainly focus on the seasonal data covering two years from Jarvis et al. (2009). However, we note the Fibiger et al. (2016)'s surface snow $\delta^{15}N(NO_3^-)$ data was remarkably higher than Jarvis et al. (2009) for the same months, which likely indicated the heterogeneity among data from different years. Compared to surface snow nitrate, snowpack nitrate was enriched by (12.8 ± 2.6) ‰ in spring, as seen in Fig 2b. This value should reflect the effect of post-depositional processing on snow nitrate throughout its preservation, i.e., time from being deposited to the surface to being archived below the photic zone. In Jiang et al. (2021), this effect was defined as PIE, i.e., the photo-induced isotope effect, and calculated as the difference between surface snow $\delta^{15}N(NO_3^-)$ and archived snow $\delta^{15}N(NO_3^-)$. The PIE in spring calculated by the TRANSITS model is averaged at (14.3 ± 1.1) ‰, consistent with the observations. Calculating the PIE only requires one to compute the relative nitrate loss induced by nitrate photolysis, which makes the PIE independent of the initially deposited nitrate $\delta^{15}N$ and a good tracer of the isotopic effect of post-depositional processing. Here we propose a simplified formula of PIE for quick assessment of the photo-driven post-depositional processing effect on $\delta^{15}N(NO_3^-)$ at any sites of interest:".

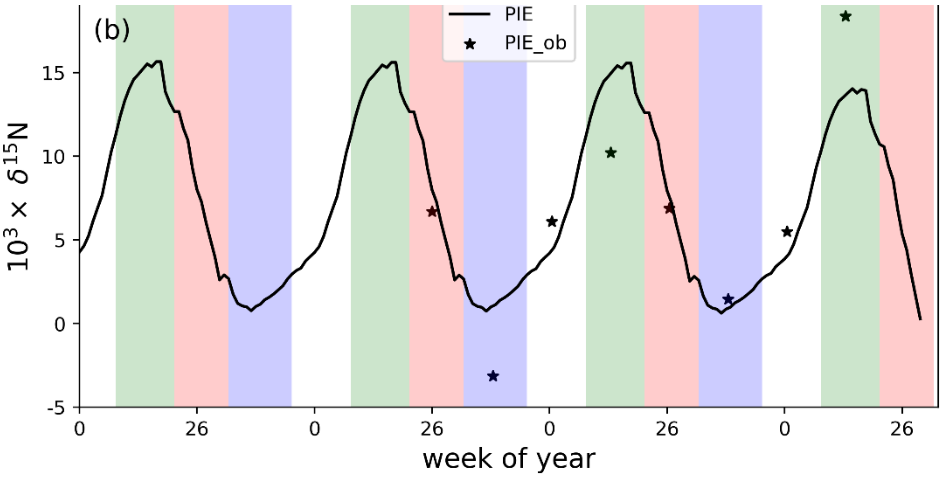


**Figure 2**. Modelled PIE (Jiang et al., 2021) compared with the observed PIE from Jarvis et al. (2009).

*Line 423 – "J also varies with depth". This is wrong because J is the surface photolysis rate constant. The exponential term changes the actinic flux seen at depth.*
**Response:** Thanks for pointing out this. We have rephrased as "Both $\varepsilon$ and $J$ varies seasonally owing to the time-varied actinic flux, while the decrease of nitrate photolysis rate constant with depth is constrained by the exponential term".

*Line 458-9. This is a strange statement in that it's not clear how a data point obtained by another group can be "explored and confirmed".*
**Response:** We agree and have deleted this sentence in the revised version.

*Line 680, 685 and what follows, plus 715 and following. A quite definite conclusion is based on the assertion that the slope of the snowpack data is significantly different from that of the atmosphere (not aerosol) and surface snow data. Your reported uncertainties on the slopes might indeed suggest that but sometimes it's better just to look at the data – would anyone really say that the yellow data are a significantly different population from the blue and black data? In fact you suggest that what is happening is an enrichment of 18O in the snowpack samples. But again, just look at the data: it's quite obvious that if there is a difference it is that the snowpack data have a subpopulation that is enriched in 17O (I admit I am not sure how that leads to a lower slope and indeed by eye its very hard to see how the yellow points can have a slope of 0.3). Please reconsider this whole discussion; I think you are building a lot on very shaky differences.*
**Response:** We double checked the regression line calculation, and found the same slopes. Although the snowpack data look like a subgroup of atmosphere and surface snow data, the time resolution for these data are quite different. For example, the thickness of surface snow is ~1-2 cm per sample while the snowpack is about 5 cm per sample (Geng et al., 2014). Given the annual accumulated snow depth at Summit is ~65 cm, the time resolution of surface snow is likely to be weekly, while for snowpack samples it shall be monthly. We note that the atmospheric samples

resolution is 3 days, so we feel it's unfair to directly compare how snowpack $\Delta^{17}O(NO_3^-)$/$\delta^{18}O(NO_3^-)$ was shifted as they had been averaged in time owing to the limited resolution.

We agree that using the regression relationship to infer how the $\Delta^{17}O(NO_3^-)$/ $\delta^{18}O(NO_3^-)$ relationship changed after deposition is suspicious as it highly depends on the assumption of little changes in snowpack $\Delta^{17}O(NO_3^-)$. In the revised manuscript, we treat the different linearity as evidence of post-depositional processing altering snowpack oxygen isotope but avoid further discussion about the relative importance of photolysis fractionation and cage effect. The revised text is shown as follows below:

"Fibiger et al. (2013) found a strong linear relationship between their measured $\Delta^{17}O(NO_3^-)$/$\delta^{18}O(NO_3^-)$ in surface snow samples at Summit. Based on this relationship they proposed a direct transfer of atmospheric oxygen isotope signals to surface snow at Summit. However, as discussed in Jiang et al. (2021), this relationship should not be viewed as evidence of little to no post-depositional processing. Instead, examining the $\Delta^{17}O(NO_3^-)$/$\delta^{18}O(NO_3^-)$ relationships among atmospheric, surface snow and snowpack samples may provide some clues on whether or not the photo-driven post-depositional processing impacts the $\Delta^{17}O(NO_3^-)$/$\delta^{18}O(NO_3^-)$ ratio, since post-depositional processing influences $\Delta^{17}O(NO_3^-)$ and $\delta^{18}O(NO_3^-)$ differently. We note that different types of observations are different in their time resolutions. Our atmospheric measurement is typically 3 days per sample, while the surface snow samples (1-2 cm thickness) in Fibiger et al. (2013) represented weekly accumulation and snowpack sample resolution (5 cm per sample, Geng et al., 2014) is closer to monthly resolution. The linearity in surface snow shall not be changed by aggregation if post-depositional processing was negligible. Here we plotted our atmospheric and snowpack $\Delta^{17}O(NO_3^-)$/$\delta^{18}O(NO_3^-)$ data together with the four months (in year 2010 and 2011) of surface snow data from Fibiger et al (2013) in Figure 4.

As shown in Figure 4, the linear relationship between atmospheric $\Delta^{17}O/\delta^{18}O(NO_3^-)$ ($\Delta^{17}O(NO_3^-) = (0.44 \pm 0.04) \times \delta^{18}O(NO_3^-) - (3.45 \pm 3.28)$, r = 0.81) is very similar to the reported surface snow relationship ($\Delta^{17}O(NO_3^-) = (0.41 \pm 0.01) \times \delta^{18}O(NO_3^-) - (3.19 \pm 0.41)$, r = 0.90) despite their different time coverages. Such a relationship suggests that the linearity of $\Delta^{17}O/\delta^{18}O(NO_3^-)$ in surface snow may directly originate from atmospheric nitrate, consistent with the conclusion of Fibiger et al. (2013). The conservation of $\Delta^{17}O/\delta^{18}O(NO_3^-)$ relationship during deposition is somehow unexpected, as the current observed air-snow $\delta^{18}O(NO_3^-)$ difference is highly variable in both magnitude and sign (Jarvis et al., 2009; Fibiger et al., 2016). Further studies are required to understand why these observed atmospheric $\delta^{18}O(NO_3^-)$ are so different between different years. However, in the snowpack data, the linearity between $\Delta^{17}O$ and $\delta^{18}O(NO_3^-)$ ($\Delta^{17}O(NO_3^-) = (0.30 \pm 0.06) \times \delta^{18}O(NO_3^-) + (6.72 \pm 5.29)$, r = 0.58) was significantly different from that of atmosphere or surface snow nitrate, suggesting that post–depositional processing likely has changed the originally deposited oxygen isotope signals up on archival. We note that similar observations, i.e., better linearity of $\Delta^{17}O/\delta^{18}O(NO_3^-)$ in atmosphere and surface snow nitrate than that in the whole snowpack, were observed at Dome C where the photolysis of snow nitrate has been unambiguously shown to be dominant (Erbland et al., 2013). This emphasizes again that, when evaluating the degree of post–depositional processing, one should consider samples covering all depths of the photic zone, not only surface samples.”.

*Lines 724-736. Yes, I like this paragraph.*

**Response:** Thanks for your comments. We are aware of the limitations of the current dataset and looking forward to more systematic studies at Summit in the future.

## Reference:

Dibb, J. E., Talbot, R. W., and Bergin, M. J. G. R. L.: Soluble acidic species in air and snow at Summit, Greenland, 21, 1627-1630, 1994.

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Silvente, E.: Contribution à l'étude de la fonction de transfert air neige en régions polaires, Université Joseph-Fourier - Grenoble I, 1993.

Jiang, Z., Alexander, B., Savarino, J., Erbland, J., and Geng, L.: Impacts of the photo-driven post-depositional processing on snow nitrate and its isotopes at Summit, Greenland: a model-based study, The Cryosphere, 15, 4207-4220, 10.5194/tc-15-4207-2021, 2021.