

We appreciate the reviewer's time in reviewing our manuscript and respect the need for clarification on several points. However, the use of the word "fraud" is unprofessional, at best. We focus our point-by-point response on aspects of the review that reasonably discuss or question the intellectual merit of our work (comments in black; response in blue).

General comments

The manuscript by Shi et al. reports snow pit results of nitrate and its isotopic composition along a traverse from coastal East Antarctic to the interior of the plateau and evaluates the effects of post-depositional processing on snow nitrate. The dataset covers an extensive area and appears to be valuable. However, much of the work (mainly the interpretation) in this manuscript repeats, at a less comprehensive level, what have been done in previous publications (e.g., [Erbland et al., 2013; Frey et al., 2009]) in the same journal. The perhaps only new content compared with the previous work is that the snowpits in this study cover a greater depth. However, the interpretation/discussion on this part is fraud. In particular, the authors state in the abstract that "Predicting the impact of post-depositional loss, and therefore changes in the isotopes with depth, is highly sensitive to the depth interval over which an exponential decrease is assumed". This statement/conclusion comes from the practice that the authors break down the entire snowpit to several depth intervals, use the Rayleigh fractionation model to calculate the fractionation constant, and apply the approach in Erbland et al. [2013] (i.e., using an exponential regression to calculate the asymptotic values of nitrate concentration and isotopes at depth below the photic zone) to a greater depth than the photic zone (up to 300 cm deep). This practice is particularly problematic for several reasons:

We disagree that our work is less comprehensive; in fact, we are providing a more extensive dataset than previously presented across the plateau (esp. with depth), newer data from the inland EAIS, and observations from coastal snowpits that resolve seasonality, which has not been included in previous work. Additionally, it is critically important that we see repeatability in the results that have been presented in Frey et al. and Erbland et al. In fact, it is suggested that Dr. Erbland agrees with this point based on his comments on this same manuscript. The extensive work that has been done particularly at Dome C has provided almost the entire insight we have into the post-depositional photolytic loss of nitrate from surface snow and we are applying this understanding to other sites to better our understanding overall.

First, the Rayleigh Fractionation model calculates the "apparent fractionation constant" [Erbland et al., 2013], which is influenced by the degree of post-depositional processing. The degree of the post-depositional processing is in turn influenced by surface UV intensity, snow accumulation rate and snow light-absorbing impurities (e.g., [Zatko et al., 2013]). When use this model to calculate the fractionation constant at different depth intervals, e.g., 0-25 cm, 25 -100 cm, and 100 cm to bottom, difference is expected on the result, as well as the logarithmic relationships between the isotope values and concentrations (or mass fraction of nitrate called in this study). This is because that at 25-100 cm, UV intensity is less than in the shallower layer, therefore the degree of post-depositional processing is smaller, which leads to a smaller "apparent fractionation constant" and a weaker relationship between the isotope values and concentrations. This is similar to the observations on snowpits from sites with different snow accumulation rates, i.e., at site with a lower snow accumulation rate the apparent fractionation constant is greater [Erbland et al., 2013] and the calculated logarithmic relationship is stronger (Table 2 of this manuscript) due to the higher degree of post-depositional processing.

The apparent fractionation can vary between sites, but should be constant at a given site over a period of time in which the boundary conditions have not changed. In the Rayleigh equation, the fractionation constant does not vary with the fraction of reactant lost (i.e. the degree of post-depositional loss by a single process (e.g. photolysis)). A simple analog is the evaporation of water – the fractionation constant does not vary with the *amount* of water lost to evaporation. Thus, for a single site in which loss is considered to be dominated by a single process (photolysis) the apparent fractionation constant should hypothetically reflect the true fractionation constant and would not change for the amount of nitrate lost by that process, under Rayleigh conditions. We agree with the referee that UV intensity would be greatest in the near surface snow, and that light is attenuated with depth. This leads to an expected decrease in the amount of loss of nitrate with depth, as the light is attenuated the chance for photolytic loss is also attenuated. However, the fractionation constant associated with photolysis should not change (there is a small caveat to this discussed below at the **). Take for example, nitrate right at the surface of the snow. If it were exposed for a short amount of time and 30% of the nitrate was photolyzed, the fractionation constant associated with photolysis would be $^{15}\epsilon_{\text{photo}}$. The same nitrate at the surface exposed for a longer amount of time, would lead to greater loss, e.g. 80%, but the fractionation due to photolysis ($^{15}\epsilon_{\text{photo}}$) does not change, only the amount of loss changes. In this example, the $\delta^{15}\text{N}$ of nitrate would be different for the shorter and longer exposure times, because of the amount of loss, but the $^{15}\epsilon_{\text{photo}}$ would remain the same in each case.

*(**Even if it is assumed that the irradiance spectrum does not remain constant with depth (i.e. the attenuation of shorter wavelength light is greater than for longer wavelengths and Zatko et al. calculated that the e-folding depth for 400 nm is 10 cm deeper than 305 nm), this increase in proportion of longer wavelengths would shift the fractionation constant to slightly more negative values with depth – but this change in the fractionation constant is again not directly related to the amount of loss but it is rather the shift in the spectral actinic flux that influences the “true” $^{15}\epsilon$. This is opposite of what is actually observed (e.g. the apparent $^{15}\epsilon$ shifts to more positive values deeper in fig 4).*

Savarino et al.'s (2007) atmospheric measurements attribute a distinct seasonal cycle in the isotopes of nitrate to variable nitrate/NO_x sources. A similar seasonal cycle is found in the coastal snowpits presented here. At these sites any loss due to photolysis is convolved with the imprint of the seasonal changes in the isotopes. Thus, the derived apparent fractionation factor changes, not simply because there is a different amount of loss, but because it is obscured by the addition of other factors – in this case the imprint of a significant seasonal cycle, which is preserved in the coastal region (due to less loss by photolysis). At the other extreme is the inland EAIS (Dome A, Dome C, Vostok), where the photolysis of nitrate is so dominant that the isotopic expression of a single loss process is well explained by the fractionation constant associated with photolysis. Following from this, the changes in the apparent $^{15}\epsilon$ and strength in the calculated logarithmic relationship noted by the reviewer above for different accumulation regimes can be viewed as reflecting the degree to which other factors (source/oxidant seasonality) are overprinted by photolytic fractionation which, although obviously related to loss, is different than attributing these changes to fractionation progressively changing with loss.

Second, it does not make sense to use the Rayleigh model to calculate the fractionation constant at depth below the photic zone. The authors perform this at the depth intervals of 25-100 cm and 100-bottom (200 or 300 cm deep). The e-folding depth of UV radiation in the plateau is 10-20 cm based on observation [France et al., 2011], and 18-22 cm based on model [Zatko et al., 2013] which was recently suggested to be overestimated by ~20% [Libois et al., 2013]. But in any case, most (95%)

of UV radiation disappears below the photic zone (in general 3 times of the e-folding depth, which is ~60 cm the maximum), where photolysis of snow nitrate is inhibited. Therefore, what is the point do use the Rayleigh model for the depth interval of 100-200(300) cm?

As the reviewer points out below *"Underneath the photic zone, nothing will change any more in theory."* We are, in fact, working under this very assumption. The reviews and comments on the manuscript raise the need to better address and clarify this in the text, and we have re-calculated the apparent fractionation factors ($^{15}\epsilon_{app}$ and $^{18}\epsilon_{app}$) over the depth intervals of 0-20cm, 0-40cm, and 0-60cm, to better represent our aim. If indeed nothing changes under the photic zone (our working hypothesis), then the apparent fractionation constant that is derived over an interval below the photic zone, should reflect that for an interval near the surface. In other words, as the nitrate moves out of the photic zone the imprint of the photolysis (fractionation constant) should be "locked in." So if that apparent fractionation constant changes with the depth interval over which it is calculated (even in the photic zone), why would this be? Our paper aims to consider this question and test our current understanding of how we explain the changes in the isotopes of nitrate with depth (e.g. is it a result of changing boundary conditions such as overhead ozone, something in situ, or something else?). If the isotopic behavior is not constant with depth, then what is the appropriate interval *near the surface* over which to calculate an apparent fractionation factor or construct an asymptotic function? The extensive work by Erbland et al. on the EAIS suggested that we need only understand the top ~20 cm. We find that this is not sufficient and indeed the apparent fractionation factor changes with the depth interval considered. We are then exploring how to explain these changes with depth and how to build confidence in quantifying them near the surface.

Third, the authors apply the approach of the exponential regression to different depth intervals and to a depth well below the photic zone, and claim that the "asymptotic values and thus the prediction of nitrate concentration and isotopic values in ice cores depend on which depth interval the exponential regression is done". This also does not make sense. The exponential decrease is expected from the effect of post-depositional loss. Underneath the photic zone, nothing will change any more in theory. So what's the point to do this regression to a depth well below the photic zone? The basic idea of doing such a regression in Erbland et al. [2013] is, given the condition that the degree of post-depositional processing is constant during the period that a certain layer of snow stays in the photic zone, nitrate concentration and its isotopic composition in that layer will approach constant values and can be predicted (i.e., the asymptotic values in Erbland et al. [2013]) once that layer is buried below the photic zone. However, this is not saying that at every snow layer below the photic zone, nitrate concentration and its isotopic composition should be the same. Because the concentration and isotopic composition below the photic zone are also influenced by that in the originally deposited snow. In addition, the degree of post-depositional processing could also vary with time, given the possible changes in UV radiation, accumulation rate and snow impurities. This alone can lead to difference in nitrate concentration and its isotopic composition in different snow layers below the photic zone. For example, snow layer at 100 cm vs. that at 300 cm in Dome A, snow at the layer of 300 cm deep deposited probably ~ 40 to 50 years ago, while the layer at 100 cm deposited slightly over a decade ago. During this time period, the degree of post-depositional processing certainly varies (e.g., at least stratospheric ozone layer change a lot since the 1980s), thus even the originally deposited values were the same, their asymptotic values (after post-depositional processing) should be different. Therefore, the values of concentrations and isotopic below the photic zone differing from the predictions of exponential regression, as observed by the authors, doesn't mean necessary that other processes occur in deeper layers below the photic zone. This makes their major conclusion, which is "Predicting the impact of post-depositional loss, and therefore changes in the isotopes with depth, is highly sensitive to the depth interval over which an exponential decrease is assumed", fraud.

Ignoring the reviewer's tone and inappropriate use of the word fraud, we nonetheless agree that there is a clear need to clarify our starting assumptions in the manuscript and what we are testing. If we assume that nothing changes below the photic zone and that the apparent fractionation factor derived near the surface is explained by photolytic processing of nitrate, why then does the apparent fractionation factor change depending on the depth interval over which you calculate it? The relationship that is captured by calculating the asymptotic values can be compared against what has actually happened since we have deep snowpits. Comparing the calculated relationships against the observations can then allow for deriving how the isotopic composition has changed and by what processes other than photolytic loss near the surface – *for example*, changes associated with the originally deposited source signal, changes in chemistry prior to deposition, changes in ozone, changes in situ at depth outside of the photic zone or ? This effectively asks what observations in the snow do you use to calibrate the asymptotic function? How do we build confidence that we can distinguish photolytic loss from other changes in the isotopic composition of nitrate? How well can you reconstruct past nitrate signals at the surface – how much do you need to know about what happened in the surface snow and over what depth interval? We are exploring these questions in this manuscript.

Because of these problems, I unfortunately can't recommend this manuscript for publication.

In addition to the above, there are many other concerns as follows:

1. Section 2.2.: sample analysis: the authors claim that only 5 nmol nitrate in sample is required to do the isotopic analysis. I am concerned in practice, how the authors ensure that their samples in each run exactly contain the same amount of nitrate as the standards. In other words, how close is their sample peak area to the peak area of all standards? This is critical, because that it is known the influence of background/blank is larger at smaller quantity of sample nitrate, thus a decay of the measured isotope ratios along with sample nitrate quantity is observed [Costa *et al.*, 2011]. It is okay if the standards are exactly at the same size as the samples, as all the background is equally corrected. However, at such a small 5 nmol level, a slight size difference between the standards and the samples will lead to probably significant over-correction or under-correction. So if the data are going to be published, I suggest the authors add information on the sample peak area vs. standard peak area in a particular run to validate the quality of the data.

We agree with the reviewer, and the additional information on the nitrate amounts of samples and standards will be included in the revised manuscript. At Brown University, it is a standard practice that nitrate concentrations in the samples and standards be very similar for each run of samples, i.e., the injected volumes of both samples and standards are similar (see table below). In this case, any effects due to blanks/different volumes can be minimized. In addition to this point, we also run replicates and report the pooled standard deviation for paired replicates (or triplicates) along with the pooled standard deviations of the reference materials. Because our reference materials are run many times, the pooled standard deviations for these are necessarily smaller than that which we find for replicate sample analyses (that are only run a small fraction of the number of times the reference materials are run).

The maximum injection volumes are near 55 mL for the method at Brown University (see the figure below). The autosampler carousel is designed to fit either 20 mL vials or 60 mL vials – both vials have the same size septa caps (see the figure below), and the purging needle is designed such that the carrier gas and sample outflow into the rest of the system near the very top of the vial (such that a volume near 60 mL can be achieved). We have tested the addition of high volume samples extensively, and will include additional information on this in the supplemental material. As

requested by the referee, below is an example table of data obtained from a 5 nmol run. It is critically important when running very low concentrations to include standards/reference materials that are very close in concentration for correcting the data (i.e. there is a “volume” effect such that is accounted for when using samples and standards of the same volume), and we apologize for not including detailed information on this originally. Below is also two additional tables of data obtained on internal working standards. The first is a mix of USGS 35+USGS 34, and the second is KNO₃. Both are used as internal standards for quality control purposes (they are treated as samples and corrected to reference materials in each run). Each has been injected for different volumes in different runs and its isotope values corrected to reference materials in each run that are close in concentration (i.e. close in injection volume). As you can see this also shows excellent reproducibility over a variety of runs and range in (low) concentrations for $\Delta^{17}\text{O}$, $\delta^{15}\text{N}$, and $\delta^{18}\text{O}$.

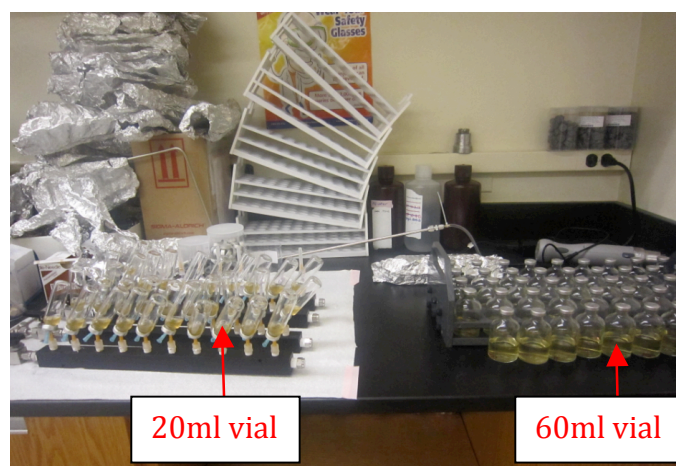


Figure: The two types of injection vials (e.g., 20ml and 60ml) used at Brown University.

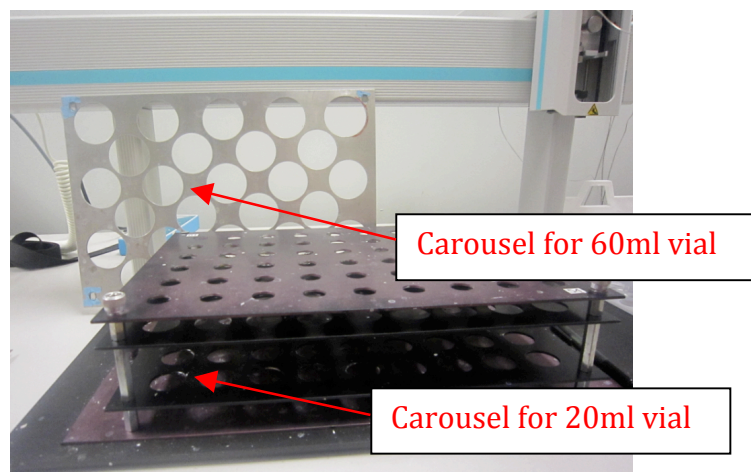


Figure: The autosampler carousel designed to fit either 20 mL vials or 60 mL vials

Table: Example raw data using 5nmol injections. Note that the “flush vial N₂O” is a vial filled with reference gas that serves as an additional quality check on the system prior to and after completing a sample run.

Sample/Standard	Injection volume, ml	Peak area, Vs	rd ¹⁵ N uncorrected	rd ¹⁸ O uncorrected
flush vial N ₂ O		22.1	-0.7	-0.2
flush vial N ₂ O		23.6	-0.9	-0.5
Blank		<0.2		
USGS 35	25.0	3.7	3.7	44.9
USGS 32	25.0	4.3	156.9	20.3
USGS 34	25.0	4.0	-3.5	-28.6
IAEA N3	25.0	4.2	3.7	17.7
Sample-2	24.5	3.8	271.6	40.2
Sample-4	24.5	4.4	299.1	35.8
Sample-7	25.1	4.5	323.3	35.2
Sample-8	22.1	4.3	299.0	36.5
Sample-9	23.6	4.2	290.3	37.2
IAEA N3	25.0	4.2	3.9	17.6
USGS 35	25.0	4.0	3.5	44.2
USGS 34	25.0	4.1	-3.0	-28.2
USGS 32	25.0	4.2	155.5	20.3
Sample-12	24.6	4.3	346.2	33.0
Sample-13	29.0	3.7	318.2	34.6
Sample-15	27.3	3.9	323.2	37.3
Sample-16	29.3	3.7	308.9	38.9
Sample-17	26.4	3.4	284.1	42.8
Sample-18	24.4	3.9	289.5	40.2
Sample-19	28.0	4.0	269.7	42.4
USGS 32	25.0	4.2	155.3	20.4
IAEA N3	25.0	4.4	3.9	17.4
USGS 35	25.0	3.9	3.4	44.7
USGS 34	25.0	4.0	-3.2	-28.8
flush vial N ₂ O		25.5	-1.1	-1.0

$\Delta^{17}\text{O}$ for a USGS 35/34 mix internal standard. Mix is not quite 50/50.

Mean = 11.45 +/- 0.44‰. Range = 10.88 - 12.21‰.

$\Delta^{17}\text{O}$ (‰)	Amount (nmol)	Concentration (uM)	Injected volume (mL)
11.26	50.00	5.00	10.00
10.89	50.00	5.00	10.00
10.88	50.00	3.00	16.67
11.06	50.00	3.00	16.67
11.78	50.00	2.00	25.00
10.94	50.00	2.00	25.00
11.62	50.00	1.50	33.33
11.71	50.00	1.00	50.00
12.21	50.00	1.00	50.00
11.83	50.00	1.00	50.00
11.59	45.00	3.00	15.00
11.37	45.00	2.00	22.50
11.23	40.00	1.00	40.00
11.20	40.00	1.00	40.00
12.20	40.00	1.00	40.00

KNO3 internal standard.

$\delta^{15}\text{N}$: Mean = 58.37 +/- 0.62‰. Range = 57.52 - 59.59‰.

$\delta^{18}\text{O}$: Mean = 30.65 +/- 0.52‰. Range = 29.55 - 31.50‰.

$\delta^{15}\text{N}$	$\delta^{18}\text{O}$	Injected volume (mL)	Concentration (uM)
59.39	30.43	20.00	0.50
58.19	30.85	20.00	0.50
58.37	30.80	20.00	0.50
57.52	31.13	16.67	0.60
58.77	30.62	16.67	0.60
58.12	30.59	13.33	0.75
57.66	31.50	13.33	0.75
58.73	30.16	13.33	0.75
58.39	31.31	10.31	0.97
57.71	31.00	10.00	1.00
58.26	31.01	10.00	1.00
58.48	30.82	10.00	1.00
57.70	30.45	10.00	1.00
57.57	31.04	10.00	1.00
58.96	29.93	10.00	1.00
59.59	29.77	10.00	1.00
59.31	30.16	10.00	1.00
58.70	30.88	10.00	1.00
58.27	29.55	10.00	1.00
57.76	31.01	6.67	1.50

2. Section 4.1.2.: in this part, the authors completely ignore the what likely occurs in the gas phase. Re-oxidation in the overlying air (i.e., the equilibrium in the air snow interface [Erbland *et al.*, 2013; Frey *et al.*, 2009]) and in the interstitial air of snowpack also occurs. Although the re-oxidation in the condensed phase is likely dominated, the authors should at least discuss other processes.

We appreciate the reviewer's point here. The title of the section should be changed since the focus of the section is on the condensed phase alone. Other parts of the manuscript consider the gas phase re-oxidation, but again this point needs to be clarified and better organized in the text.

3. Section 4.1.3.: Why "the importance of this process (volatilization) is unclear"? Hasn't the relative importance of this physical release to photolysis been evaluated extensively in [Erbland *et al.*, 2013; Frey *et al.*, 2009] and concluded clearly that photolysis dominates in the post-depositional processing? These two references are cited in this same section and the authors pretty much rely on the results of these two previous publications here to make the discussion.

In addition, the field experiments in [Erbland *et al.*, 2013]) suggested a fractionation constant with respect to the physical release of close to or below zero. Doesn't this imply that the assumption and model result on the isotopic effects of physical release in [Frey *et al.*, 2009] are less reliable?

Our purpose here was to review the state of the science and consider both the theoretical and the experimental results in terms of our observations. The wording in the manuscript needs to be modified to better reflect this. There are several inconsistencies between the theoretical and experimental results in the two publications, and we are noting the disagreements and the lack of experimental values for $\delta^{18}\text{O}$. The experiments are far from perfect, but do indeed give a sense that the theoretical cases considered by Frey *et al.* may not be applicable in the field or to conditions at Dome C. (And note here that Dr. Erbland's comments on our manuscript suggest, as we have, using the field experiment results with caution).

4. Section 4.2.: This is by far the part with the largest issue. Much of them have been discussed above in the general comments, but there are additional concerns. In page 31959, the last paragraph, the authors suggest the $\delta^{18}\text{O}$ continued to change below the photic zone is due to re-formation of nitrate in the gas phase through O_3 oxidation. There are several problems with this. First, $\delta^{18}\text{O}$ increases from the depth of ~10 cm continued to a depth of 200 cm. At depth below the photic zone (i.e., < 60 cm), how NO_x is produced and how much oxidants (O_3) are available in the interstitial air?

We suggest the in situ processing as a hypothesis, but there is a clear need to clarify this, consider several hypotheses, and to better construct our arguments as to whether this in situ change is even likely/possible. However, under the current state of understanding, it is very difficult to explain an increase in $\delta^{18}\text{O}$ below the photic zone. (And please see our response to the second reviewer regarding changes in ozone.)

Second, why this phenomenon is not observed in Pit 6? which should be expected if the authors are correct.

It is not clear why each pit must behave the same in order to justify or negate the hypothesis. Three of the four pits in the interior of the EAIS show increases in $\delta^{18}\text{O}$ with depth. (The same could be said about the apparent $\delta^{15}\text{N}$ that has been observed in previous work on the EAIS as it is generally

more negative and more variable than that predicted by theory, but the balance of evidence supports this being the photolytic imprint.)

Third, [Meusinger *et al.*, 2014] suggests that nitrate in snow grains can be categorized into two domains: photolabile nitrate and buried nitrate. Photolabile nitrate is nitrate in the surface of snow grain which is easily photolyzed and the photoproduct mainly escapes into the air; while the buried nitrate are in the snow grains, the photolysis of this nitrate leads to re-formation of nitrate in the condensed phase and depletion of oxygen isotopes due to exchange with water oxygen. The experiments in [Meusinger *et al.*, 2014] then suggest that the photolabile nitrate is quickly removed under UV radiation, and buried nitrate started to be photolyzed after photolabile nitrate is almost gone. Therefore, in the field, at surface, the photoproducts mainly escapes to the air (because nitrate are in the surface of snow grains) without significant re-oxidation in the condensed phase; but at depth, re-oxidation in the condensed phase becomes more important as where it is "buried nitrate" photolysis and the products stay in the condensed phase longer before escaping to the interstitial air. This means, as going deeper, NO_x is more difficult to escape (more re-oxidation in the condensed phase). This offsets the effect of re-oxidation of NO_x by O₃ on d18O of snow nitrate at greater depths, even the later indeed occurs.

Agreed. The strength of the hypothesis regarding in situ processing needs to be revisited in the text and better addressed. Reviewer #2 also comments on this and indeed it seems difficult to explain the increase in $\delta^{18}\text{O}$ by this mechanism.

By the way, in the second paragraph in Page 31960, the authors are pretty much against themselves.

The text deserves clarification, but we are not sure how we go against ourselves here. Expecting that the isotopic composition should not change below the photic zone, and that the apparent fractionation factor be preserved with burial is, as discussed above, the starting assumption this study works under.

5. Section 4.3.: as discussed in the general comments, this section makes no sense. It is meaningless to apply this approach for depth below the photic zone, and give the time period the snowpit covers (300 cm, approximately 50 years), other factors have to be considered.

This is addressed in the general comments above, but we will also note here that the time period of coverage varies considerably across the pits; for example, Pit 4 only covers ~12-13 years. We are applying our understanding here across the pits in the context of what is captured by each. To clarify the timescale Table 1 from the supplement will be moved into the text and/or replaced by a figure suggested by anonymous referee #2 – please also see our response to referee #2 regarding adding timescales to the figures.

6. Section 4.4.: First, beside the discussion on stratospheric sources, it is unclear throughout this part which sources and how they shift. **Last paragraph in page 31965:** why the more stratospheric nitrate in the most recent winter in P1 is relate to the smaller ozone hole in the spring of 2012? Theoretically, chemical ozone loss in the stratospheric requires the growth of PSCs and which favors at lower temperature in polar winter (e.g., [Manney *et al.*, 2011]). Only severe cold promotes the growth of PSCs, when it grows big enough it starts to descend, leading to surface enhancement of nitrate. Lower ozone loss indicates probably less abundance / growth of PSCs in polar winter, and should be consistent with less stratospheric nitrate input. So the argument in this paragraph does not make sense.

We do think it's worth noting that these two events coincide even though a mechanistic link is not clear at this point. We will amend the discussion to consider other possible influences.

First paragraph in page 31966: the authors claim that the high d15N values (31 per mil) in cold seasons indicate that stratospheric nitrate should be higher than [Savarino *et al.*, 2007] calculated. In Figure 7, in the season with the largest stratospheric nitrate contribution (the most recent one, as indicated by the largest d18O and D17O values), d15N is the lowest.

Not clear which peak the referee is looking at? The $\delta^{15}\text{N}$ for the most recent wintertime snow averages $\sim 32\text{‰}$ – so the connection here is that the $\delta^{15}\text{N}$ of stratospheric nitrate, which has been invoked to explain similarly high $\Delta^{17}\text{O}$ and $\delta^{18}\text{O}$ values in Savarino *et al.*, may contribute values that are greater than the $19 \pm 2\text{‰}$ suggested in Savarino *et al.*

Therefore, it is not reasonable to attribute the high d15N values in the cold seasons to stratospheric influence alone, as if so d15N in the most recent cold season should be the highest. In addition, [Savarino *et al.*, 2007] measured seasonal atmospheric nitrate in coastal regions of East Antarctica, which should be more directly influenced by stratospheric signals. The fact that the snow nitrate in P1 possesses much higher d15N in all seasons compared to the atmospheric measurements [Savarino *et al.*, 2007] (though not in the same location, but if considering stratospheric signal, the spatial variability should be small), should not imply other processes (e.g., post-depositional processing) in the air-snow interface play a role? As a reminder, the stratospheric signals [Savarino *et al.*, 2007] measured in the boundary layer is less than 10 per mil for d15N, while in the snow in this study is 31 per mil.

The discussion here seems counter to what is published in Savarino *et al.*, and we limit our discussion to what appears in that work. We are looking to explain the very high $\Delta^{17}\text{O}$ and $\delta^{18}\text{O}$ that coincide with values of $\sim 32\text{‰}$ for $\delta^{15}\text{N}$. We offer several lines of evidence for why limited post-depositional loss is apparent in these coastal pits.

There are some other points:

- 1) P31964, line 16: the oxygen-17 anomaly reflects oxidants involved in nitrate production, not oxygen isotopes. NO_x itself and water vapor in the atmosphere also influence oxygen isotope in nitrate.
- 2) P31967, line 8-10: what are the problems?
- 3) P31949, line 15-16: The sentence starts with "secondly..." is confusing. Does it mean that one sample was replicated 38 times?
- 4) P31949, line 23 to the end of this paragraph: when corrected for oxygen isotope exchange with water, it seems only water in the sample matrix is considered. How about water in the bacterial media? or it is dry bacteria added to the liquid samples?
- 5) P31951, line 11-15: It must be made clear that the 2.5 per mil underestimate without step 2 correction when using pre-concentration is only true for samples with nitrate at 35 nmol around; if there are 350 nmol nitrate in each sample, this effect will be much reduced to minor.
- 6) P31956, line 8: it is "sect. 4.1.3", not 4.1.2;
- 7) P31958, line 14: this is true at only low snow accumulation sites. Please make it clear.
- 8) P31958, line 25-26: This statement is not true according to table 2, as where it shows the relationship between d15N and nitrate concentration shifts from negative to positive, opposite to that between d18O and concentration.

All of these comments can be clarified in the revised text.