**Reply to Anonymous Referee #1**

We thank Anonymous Referee #1 for his/her positive review of this work.

Replies to Anonymous Referee #1's remarks and suggestions are given below. For clarity, we keep the reviewer's comments in blue and italic while our response is in black font.

*Nitrate is massively lost from the snowpack to the atmosphere making an interpretation of nitrate concentrations measured in the ice difficult if not impossible. Isotopes of N and O have been claimed to have the potential to disentangle post depositional processes from the initial nitrate deposition. Both isotopes have their specifics shedding light on different processes. Erbland et al. constructed a conceptual model for air snow interaction for nitrate including its isotopes. The model is not and does not claim to be perfect and include all the processes. However, the relevant processes are claimed to be included by parametrization.*

*The model and its concepts are well described. The simulations and discussions are hard to follow and naturally depend largely on the assumptions. That part of the manuscript would benefit from a bit of reorganization. State clearly what is working and even more what is not working. There are huge discrepancies between the model and the data. What is the motivation for the "modified Rayleigh plot"? The 21 year response time due to recycling in the model results in a major discrepancy in the concentration in the snow and points, in my view, to a major flaw in the concept of the model.*

Sections 3.2 and 3.3 ("Evaluation and discussion") have been largely reworked it order to clarify what is working and what is not. We also suggest to add a small paragraph in the conclusion of the paper.

The motivation for the modified Rayleigh plot is to offer a framework for the interpretation of nitrate isotope profiles in ice cores. Indeed, from ice cores, one can measure w(FA), d15N(FA) et D17O(FA). Given an assumption or constraints on the snow accumulation rates, one can calculate FA = w(FA) * A. In such a representation, we can observe the different impacts of changes in various variables and parameters. Since no direct information on eps_photo can be obtained from ice cores, one can use this representation to obtain indirect information.

Regarding the 21 years response time, the reviewer can refer to our reply to Prof. Wolff review, especially the second part of "Reply to the "Conceptual issue with the age of nitrate and number of recyclings" by Eric Wolff" when a detailed experiment illustrates this time response. We note that, in this case ("spike experiment"), the reported response time is 16 years while it is 21 years in the experiment described in the manuscript (FPI multiplied by a factor 10). The difference comes from the different criteria in determining the return to stable conditions.

*Still the model is a step forward in our approach to understand nitrate as a climate parameter at low accumulation Antarctic sites and certainly deserves being published.*

*Minor comments:*

*Title: the subtitle says "part 2". I had difficulties finding part 1, published in 2013. I suggest to explicitly referring to part 1 in the introduction.*

The following sentence has been added at the beginning of section 1.4 in the introduction: "This paper is a companion paper of "Air-snow transfer of nitrate on the East Antarctic Plateau – Part 1: Isotopic evidence for a photolytically driven dynamic equilibrium in summer", published in the same journal (Erbland, et al., 2013)."

*p. 6893, line 16 and 23: f is the remaining fraction. On line 23 it becomes "loss (f)" which is the opposite.*

Thank you for pointing this out. Sentences referring to the loss of nitrate (such as p. 6893 line 23) now refer to "1 - *f*" and not to "*f*".

*p. 6894: eq 4 is identical to eq 2 written in a different form. The difference in epsilon is that e_app potentially includes more than one process while in eq. 4 epsilon is pure photolytic. I suggest to remove one of the equations and explain the difference properly.*

Indeed, equations 4 and 2 are almost the same equations. Following the reviewer's suggestion, Eq 4 has been removed and the text has been changed to refer to the use of 15eps_pho when it comes to calculating the effects of the pure photolytic process on nitrogen isotopes of nitrate.

*p. 6894: eq 5: eq 5 is the accumulated product not the immediate product as the word emitted suggests. Please reword. Also where is eq 5 relevant? As the processes described later are fast I do not see where the accumulated product comes into play. I did not find any reference to Eq. 5. Therefore I suggest removing it entirely.*

Agreed. Eq 5 is removed as well.

*p. 6898, line 1: This is the isotopic mass balance. Please call it that.*

Agreed. The sentence has been changed to: "The isotopic mass-balance equations write…".

*p. 6898, line 7: "Compartment" should be "box"*

Agreed. The text has been changed accordingly.

*p. 6901, line 28: typo should read "nitrate is kept the same"*

The text has been changed accordingly.

*section 2.4.2: I do not understand the consequence of f_app. f_app is not used later on as much as I can see. I suggest removing this section and discussing the effect in depth when D17O is discussed.*

Section 2.4.2 cannot be removed from the chapter on model description since it is an important process, which must be described. However, it is true that introducing f_app is not necessary. Therefore, the sentence "This results in an apparent remaining nitrate mass fraction denoted $f_{app}$ which writes: $f_{app} = f + f_{cage} \times (1 - f)$ and, consequently, to a lower apparent quantum yield" has been removed.

*p 6903: Can you please indicate the "Leighton cycle" in figure 1 and 2.*

Done. However, note that Figure 1 has been removed from the manuscript (cf our answers in the review process).

*p6907, line 16, 17: Why is the boundary layer set to 50m not 30m as others have found?*

The boundary layer height is set to 50 m which is the median height between winter (30m) and summer (ca. 100m) simulated by (Swain & Gallée, 2006) and (Gallée, et al., 2014), respectively. The text has been rephrased as follows: "The thickness of the atmospheric boundary layer is set to a constant value of 50 meters, a value which sits between the median wintertime value (ca. 30 m) simulated by Swain and Gallée (2006) and the mean value simulated around 27 December 2012 (Gallée et al., 2014)".

A number of additional changes have been made to the model and the main text. The reviewers can refer to our other uploaded file.